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ABSTRACT

Validation of happiness measures is inherently challenging because subjective sensations are unobserved. We introduce a novel validation method: subjects report how happy they would feel (or did feel) after some specified event, as well as how they would respond (or would have responded) to a survey question about their happiness after the same event. The difference between these two responses measures "self-reported misreporting." We demonstrate that self-reported misreporting varies across events and is substantial for certain types of events. These findings imply that caution is warranted when interpreting differences in self-reported well-being across contexts.

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1. Introduction

The most direct method for evaluating an individual's well-being is simply to ask them how they feel.[1] This approach has long been popular in the field of psychology and has gained considerable acceptance within the public policy community. Perhaps the most visible application has involved the construction and refinement of "national happiness accounts" (see, for example, Helliwell et al., 2014; Kahneman et al., 2004).

The recent growth of economic research on self-reported well-being reflects an emerging interest in exploring alternatives to traditional choice-based welfare measures. Naturally, increased visibility elicits increased scrutiny. Accordingly, an emerging methodological literature addresses the conceptual and practical difficulties these methods encounter. For example, Bond and Lang (2019) criticize typical uses of categorical responses to questions about well-being; see also Liu and Netzer (2023) for a proposed solution.

This paper asks whether people report happiness accurately. Veenhoven (1984) articulates two reasons to fear they may not.[2] First, people "may in their heart know that they are disappointed with life, but repress that thought because they cannot deal with its consequences" (pp. 44-45). Second, they may "tend to be dishonest in their communications on the matter" due to "social desirability bias" (p. 48). This bias could have a variety of sources. For example, people may project happiness to conform with social norms and promote positive interactions. Relatedly, they may fear that others will interpret expressions of unhappiness negatively, as cries for help or admissions of weakness.

Validation of happiness measures is inherently challenging because subjective sensations are unobserved. While the literature has attempted to explore the accuracy of self-reported well-being, existing methods are inherently limited, and consequently past findings are inconclusive; see Section 2.

This paper employs a novel validation strategy involving the measurement of "self-reported misreporting." We seek to determine not only whether people misreport happiness, but also whether misreporting varies systematically according to the types of conditions an individual experiences. Such variation would raise questions concerning the usefulness of self-reported well-being in contexts where the analyst seeks to compare welfare under two or more distinct conditions. Accordingly, we begin by selecting a set of reasonably common events, both positive and negative. We instruct respondents to imagine that the event occurs at some specified point in the future, and pose two prospective questions: First, how happy do

[1] Typical survey questions reference a variety of related concepts, such as happiness, life satisfaction, and domain-specific satisfaction. Our analysis is not specific to any particular concept. Throughout this paper, we will use the term "happiness" as shorthand for any notion of self-assessed well-being.

[2] To be clear, these concerns relate to *misreporting*. Veenhoven (1984) mentions other reliability concerns including the possibilities that people may have no actual opinion, that they may not know their true happiness, that their responses may be inconsistent and unstable, that they may give stereotyped responses, that interviewers may impart biases, and that responses may be sensitive to context and methods. See also Veenhoven (2008, 2015).

they think they would feel? Second, assuming they were asked on a survey how happy they felt, how do they think they would respond? Likewise, for those who have recently experienced the event, we ask two parallel questions: First, how happy did they feel? Second, had they been asked on a survey how happy they felt, how do they think they would have responded? In each case, any discrepancy between the two answers measures "self-reporting misreporting" of happiness.

We document substantial self-reported misreporting. Moreover, there is no simple pattern such as consistent overstatement, understatement, or compression. Instead, responses indicate that people significantly underreport happiness to various degrees after certain events we consider, but minimally after others. This variation implies that comparisons of happiness across conditions may be inherently problematic.

The questions we pose concerning expected happiness contingent on some future event resemble those used in studies that construct measures of projection bias (see, e.g., Gilbert et al., 1998, and Loewenstein et al., 2003). Accordingly, as an application, we use our results to estimate the extent to which measured projection bias reflects misreporting of happiness once events actually occur. While evidence of projection bias remains, a substantial portion of the effect appears to be spurious.

Because true happiness is unobservable, we cannot validate our validation method directly. However, we can do so indirectly by comparing self-reported misreporting to actual misreporting in another setting where the truth is likewise unknown for individuals, but the distribution of outcomes is known for the group. In that context, self-reported misreporting correctly detects and accurately measures misreporting.

The rest of the paper proceeds as follows. Section 2 describes related literature. Section 3 details our survey design. Section 4 clarifies some important interpretive issues. Section 5 presents our basic findings on self-reported misreporting. As an application, Section 6 explains how our results impact the interpretation of evidence on projection bias. Section 7 provides validation for the validation method. Section 8 offers some brief conclusions.

2. Related Literature

The literature on happiness discusses the possibility that people may report subjective well-being inaccurately and acknowledges the consequent need for validation. Efforts to assess the accuracy of self-reported happiness have involved two main validation strategies. One is to investigate whether such reports align with other measures of happiness, such as non-verbal cues (e.g., amount of smiling), expert ratings by psychologists, interviewer ratings, reports of family and friends, recollection of positive events, and various biometric responses. Correlations among these measures are said to provide "congruent validation." The second strategy is to examine correlations with other phenomena that are generally thought to connect

with happiness, such as measures of social success and other aspects of "the good life." These correlations are said to provide "concurrent validation." For reviews of this evidence, see Veenhoven (1984, 2008, 2015) and Diener and Oishi (2004); with respect to biometric evidence, see Larsen and Frederickson (1999) and Davidson (2004).

Unfortunately for these validation methods, there is no domain within which ground truth (i.e., "true happiness") is known.[3] Accordingly, one cannot rule out the possibility that correlations across indicators of happiness reflect interrelated biases rather than variations in true happiness. Moreover, even if we construe this evidence as establishing the existence of a correlation between reported and actual happiness, it cannot establish the absence of systematic bias. Formally, if reported happiness under condition x is given by $r(x) = h(x) + b$ where $h(x)$ is true happiness and b is the reporting bias, then even a strong positive correlation between $h(x)$ and $r(x)$ does not imply that b is white noise. On the contrary, b may vary systematically with x, in which case reports may reverse rankings for categories of events.

Other evidence attempts to speak directly to the hypothesis that people overstate happiness. First, reported happiness is "slightly higher in personal interviews" than on questionnaires, for which social pressures are perhaps less salient (Veenhoven, 2008). While suggestive, that comparison does not control for differences between the samples—e.g., for the possibility that those who consent to personal interviews are a bit more social and consequently happier. Nor does it establish whether bias remains in questionnaires.

Second, some studies have asked whether reported happiness correlates with personal characteristics that are plausibly associated with misreporting, such as general defensiveness or a desire for social approval. For a survey of these results, see Veenhoven (1984), who describes them as mixed and concludes that such correlations are modest (pp. 46, 50). Because true happiness is also presumably related to personality traits, the implications of these findings are far from clear. For example, the literature construes helpfulness as indicating a need for social approval. But helpful people may report greater happiness because happy people tend to be helpful, or because helping others contributes to happiness.

Third, on average, people characterize their level of happiness as above average (e.g., Goldings, 1954). While this finding is consistent with the hypothesis that survey responses tend to overstate happiness, there are other possible explanations. As noted in the literature on overconfidence, such findings are potentially consistent with Bayesianism. One possibility is that different people assess happiness differently, placing the greatest weight on the dimensions of experience from which they personally derive the greatest happiness (analogously to the mechanism described in Santos Pinto and Sobel, 2005). Another possibility

[3] Veenhoven (1984, p. 53) makes the point as follows: "This approach obviously requires that we must be sure about the validity of at least one happiness indicator: the measure that is used as a standard. Unfortunately this requirement cannot be met. We are not sure of the validity of any."

is that the pattern reflects asymmetries in the underlying signal structure (analogously to the mechanism described in Benoit and Dubra, 2011). Stepping outside the Bayesian paradigm, it is also possible that people underestimate the happiness others feel, possibly because popular media coverage skews toward misfortune.

Considering the limitations of these efforts to validate self-reported happiness, the literature has been surprisingly sanguine concerning its use. For example, after acknowledging that "[m]any misgivings have been advanced about such self-report of happiness," Veenhoven (2015, p. 382) concludes that "[t]hese qualms have not been supported by empirical research…" (p. 383; similar statements appear in Veenhoven 1984, 2008). Veenhoven (1984, p. 54) declares that, "[u]ntil the reverse has been proven I will assume that these indicators do in fact tap happiness." Likewise, with respect to evaluating the quality of life as a whole, Van Praag and Ferrer-i-Carbonell (2004) observe that respondents "apparently have no difficulty in answering such a question, and… those responses seem to be comparable. Hence, we will accept this as empirical evidence that respondents are able to evaluate their life and that those responses lend themselves to scientific analysis."

3. Study design

We analyze self-reported misreporting for four categories of life events, two negative and two positive. Negative events include the end of an existing romantic relationship (for those currently in relationships) and the death of a loved one. Positive events include receiving a promotion at work (for currently employed subjects) and being hired for a dream job (for currently unemployed subjects).

We fielded three short surveys on Prolific between April and May 2023, one for each of the negative events, and one for the two positive events. In total, 1000 respondents participated in the survey on romantic relationships, 300 in the one on loss of a loved one, and 306 in the one on employment. The full text of each survey appears in the Appendix.[4]

All three surveys begin with a standard elicitation of current happiness:

> *On a scale from 1 to 7, where 1 is not happy and 7 is very happy, how do you feel now on a typical day?*

Next, we pose questions that separate respondents into pertinent groups. For the survey on romantic relationships, we ask whether the respondent is currently in a relationship. Those who are in relationships answer the prospective breakup questions, while those who are not in relationships answer the retrospective questions if a breakup occurred within the previous six months.[5] For the survey on death of a loved one, we

[4] We conducted this study under human subjects protocol IRB-42264 approved by Stanford University's IRB.

[5] We fielded two waves of the survey on romantic breakups. The text describes Wave 1. Wave 2 was identical, except that we asked a "debiasing" question before the questions about actual and reported feelings: "Imagine that you and

ask whether the respondent experienced such a loss within the previous six months. Those who had that experience answer the retrospective loss questions, while those who did not have that experience answer the prospective loss questions. For the employment survey, we first ask whether the respondent is employed. Those who are not employed and who indicate a potential interest in finding a job answer the "dream job" question. We ask those who are currently employed whether they received a promotion within the previous six months. Those who received promotions answer the retrospective promotion questions, while those who did not receive promotions answer the prospective promotion questions.

For the prospective happiness questions, the sample sizes are 466 (romantic relationship), 240 (loss of a loved one), 198 (promotion), and 58 (dream job). For the retrospective happiness questions, the sample sizes are 90 (romantic breakups), 60 (loss of a loved one), and 39 (promotion). A feature of the employment survey is that we do not gather the data needed to construct a retrospective measure of self-reported misreporting for the "dream job" event. Note that the sum of the sample sizes is less than the number of survey participants. The explanation is that some subjects answer neither the retrospective nor the prospective happiness questions—e.g., the romantic breakup survey terminates if the respondent is not in a romantic relationship and has not recently experienced a breakup.

All the prospective happiness questions are worded similarly, as are all the retrospective happiness questions. To control for order and anchoring effects, we randomize the order of the questions concerning feelings and reports of feelings. Using romantic breakups as an example, a subject who is currently in a relationship might first encounter the following prospective question about their anticipated feelings:

> *Imagine that you and the person you're involved with break up within the next week. On a scale of 1 to 7, where 1 is not happy and 7 is very happy, how do you think you would feel on a typical day two months from now?*

The next question would then elicit anticipated reports of feelings:

> *Now that you've told us how you would ACTUALLY feel two months after a breakup, we'd like to know what you think you would SAY IF ASKED on a typical day two months after a breakup to rate your overall happiness.*
>
> *Continue to imagine that you and the person you're involved with break up within the next week. Two months later, you participate in a survey, which asks you how happy you feel on a typical day using a scale from 1 to 7, where 1 is not happy and 7 is very happy. How do you think you would answer that question at that time?*

A subject who is not in a relationship, and who experienced a breakup within the previous six months, might first encounter the following retrospective question about their feelings:

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the person you're involved with break up within the next week. Using a scale from 1 to 7, where 1 is not at all likely and 7 is very likely, how likely do you think you would be to spend more than an hour a day thinking about the breakup on a typical day two months from now?" As documented in Table 2, this question had no detectable effect on answers to the questions of interest.

On a scale to 1 to 7, where 1 is not happy and 7 is very happy, how happy did you feel on a typical day shortly after your breakup?

The next question would elicit the feelings they would have reported:

Now that you've told us how you ACTUALLY felt after your breakup, we'd like to know what you think you would have SAID IF ASKED on a typical day shortly after your breakup to rate your overall happiness.

Suppose you had participated in a survey shortly after your breakup, which asked you how you felt on a typical day using a scale from 1 to 7, where 1 is not happy and 7 is very happy. How would you have answered that question?

For subjects who encounter the question about reported feelings prior to the question about actual feelings (whether retrospective or prospective), we adjust the wording to reflect the alternative sequencing.

As discussed in the introduction, our focus is on the difference between how people say they would (or did) feel, and what they say they would report (or would have reported), with respect to each type of event. Our survey design allows us to make within-subject comparisons by measuring the difference subject by subject. In addition, because we randomize the order of the questions, we can check for order and anchoring effects (e.g., whether answering a question about reporting influences the answer to a subsequent question about how they would or did feel). The same randomization feature allows us to remove these effects entirely by restricting attention to the first question and inferring self-reported misreporting from cross-subject differences. An additional advantage of cross-subject comparisons is that they minimize priming effects—i.e., the possibility that a change in wording may elicit an exaggerated response by artificially inducing a participant to focus on the difference.

4. Interpretative issues

Before turning to results, we address some issues of interpretation. Here we focus mostly on prospective happiness questions, but similar considerations apply to retrospective questions.

Our objective is to determine whether people report happiness accurately. Because standard survey questions measure happiness on a unitless scale, there is some ambiguity about the meaning of "accurate" reporting. We assume each individual adopts a fixed normalization for internally gauging degrees of happiness. When we say that their reporting is accurate, we mean that their quantitative responses are appropriate given this normalization.

Let H and h denote, respectively, the happiness someone expects to feel, and says they expect to feel, contingent on some future event. Likewise, let R and r denote, respectively, the level of happiness they expect to report, and say they expect to report, contingent on the same event. Our strategy is to measure and compare h and r. By providing different values of h and r, a participant indicates that they expect to misreport happiness, thereby implying that their statements concerning their happiness are unreliable. In

that case, one cannot credit their answers to questions about their happiness without disbelieving their responses concerning the accuracy of those answers. In other words, one must assume the respondent is selectively and fortuitously reliable. At a minimum, the hypothesized finding ($r \neq h$) therefore impugns the credibility of statements concerning happiness.

It is, of course, logically possible that an individual might expect to report happiness accurately ($R = H$) but nevertheless say they expect to report it inaccurately ($r \neq h$). Likewise, it is logically possible that they might expect to report happiness inaccurately ($R \neq H$) but nevertheless report it accurately. However, the mere existence of these possibilities provides a meager foundation for evaluating well-being based on self-reports. Indeed, some of the psychological mechanisms that give rise to reporting bias may apply more strongly to responses concerning current happiness than to the questions we use to elicit h and r. As noted in the introduction, Veenhoven (1984) articulates two mechanisms: repression of feelings of disappointment with life, and social influences. The psychological motives for repression, such as ego defense, plainly apply to contemporaneous feelings, but not necessarily to anticipated feelings associated with an event that may not occur. Likewise, people may be less likely to interpret statements about contingent future feelings as cries for help or expressions of weakness. More generally, the norms that govern social interactions call for the appearance of contemporaneous happiness, rather than happiness at points in time and under conditions far removed from the interaction.

That said, there is also no a priori foundation for assuming that self-reported misreporting of happiness is entirely accurate, while reported happiness is not. Still, under the plausible assumption that social desirability bias induces people to exaggerate their honesty, we would expect self-reported misreporting to *understate* the degree to which they think they would misreport happiness. For example, in a setting where people think they will exaggerate their happiness ($R > H$), we might expect them to report exaggeration ($r > h$), but of a smaller magnitude ($r - h < R - H$). If we assume, in addition, that people have unbiased beliefs about the degree of their own misreporting, then this lower bound also applies to actual misreporting.

Because the observations in the two preceding paragraphs are somewhat speculative, we also provide objective evidence concerning the accuracy of self-reported misreporting; see Section 7. The evidence is necessarily indirect, in that validation is only possible in settings where the distribution of correct answers to the question of interest is known. Notably, we find that self-reported misreporting is accurate in a setting with systematic and substantial misreporting.

5. Self-reported misreporting

Panels A through D of Figure 1 show distributions of responses to questions about happiness conditional on negative events. The figure uses all available data; Figures A.1 and A.2 show that these

distributions are not terribly sensitive to the order of the questions. The top two panels, A and B, are for romantic breakups, while the bottom two, C and D, are for death of a loved one. Within each row, the left panel is for prospective events, the right for recalled events. Each panel displays two distributions. The solid bars depict the distribution of how people say they would feel contingent on the event in question, while the hollow bars depict the distribution of what people say they would report. In every case, asking about reported happiness rather than actual happiness shifts the pertinent distribution to the right. In other words, these data exhibit substantial self-reported misreporting.

Panels E through G of Figure 1 show distributions of responses to questions about happiness conditional on positive events. The layout is similar to that of Panels A through D, except that the first row pertains to promotions and the second to landing a dream job. For the reasons described in Section 2, there is no figure for recalled "dream job" events. Here, we see no evidence of significant self-reported misreporting.

Because we asked each individual about both their contingent happiness and their contingent reported happiness, we can construct measures of self-reported misreporting (henceforth SRM) at the subject level. Specifically, for prospective events, SRM equals "expect to report" minus "expect to feel," contingent on the event occurring. For retrospective events, it equals "would have reported" minus "felt" immediately after the event.

Figure 2 displays distributions of SRM. Its structure is identical to that of Figure 1. Each of the seven distributions has a mode of zero. However, the distributions for the negative events in Panels A through D are strikingly right-skewed. Focusing first on prospective events, we see that for breakups, SRM is strictly positive for 49% of participants and strictly negative for only 13%; for loss of a loved one, these figures are 43% and 10%, respectively. Focusing next on retrospective events, we see that for breakups, SRM is strictly positive for 43% of participants and strictly negative for only 23%; for loss of a loved one, these figures are 37% and 17%, respectively. Moreover, in all cases, almost all strictly negative values of SRM are -1 (the smallest possible magnitude). In contrast, the distributions for positive events in Panels E through G exhibit little if any systematic skewness.

Table 1 reports regressions that quantify these differences and explore their sensitivity to the order of the questions. The table segregates results for negative and positive events (top and bottom, respectively). We include regressions for both prospective and retrospective events (except the "dream job" event, for which we only have prospective data). For the regressions involving prospective events, we pool the "Expect to Feel" and "Expect to Report" responses. Similarly, for the regressions involving retrospective events, we pool the "Felt" and "Would Have Reported" responses. Accordingly, there are always two observations for each participant.

For each event and time perspective, we report two regressions. All include controls for demographics, a Wave 1 indicator (in the case of romantic breakups), and a constant. The first relates the happiness responses to an indicator for either "Expect to Report" (for prospective events) or "Would Have Reported" (for retrospective events). The corresponding coefficient, which appears in the column labeled "Report," therefore measures average SRM. The second regression explores order effects by adding two variables: a "Report Before Feel" indictor (indicating that the question about reported feelings comes first) and an interaction between "Report Before Feel" and "Report." The coefficient of the "Report" dummy measures average SRM when the question about feelings comes first. Adding that coefficient to the one for the interaction term gives average SRM when the question about reported feelings comes first. The regression also allows one to remove order and anchoring effects entirely by inferring SRM only from first responses using cross-subject variation: to obtain the estimate, we simply add the coefficients of "Report," "Report Before Feel," and the interaction.

According to equation (1), for romantic breakups, the mean value for our measure of prospective SRM is 0.57 ($\sigma = 0.10$). To put this figure in perspective, it represents 37.2% of a standard deviation for the distribution of prospective reported happiness for this event. We reject the absence of a difference with high confidence ($p < 0.001$). The coefficients in equation (2) reflect significant order effects: average SRM is 0.77 ($\sigma = 0.14$) when "Expect to Feel" appears first, and 0.38 ($\sigma = 0.14$) when "Expect to Report" appears first. In both cases we continue to reject the absence of a difference with high confidence. The cross-subject first-response estimate of SRM is 0.73 ($\sigma = 0.14$). A close inspection of the coefficients reveals that asking the "Expect to Report" question first increases reported happiness for the "Expect to Feel" question (by 0.36) without materially changing the response to the "Expect to Report" question. In other words, the higher answer to the "Expect to Report" question exerts an upward pull on the "Expect to Feel" answer, but does not eliminate the difference.

Equation (3) shows that the mean value of SRM for the retrospective perspective, 0.61 ($\sigma = 0.23$), is similar to that of the prospective perspective. Once again, we reject the absence of an effect with high confidence ($p < 0.01$). The point estimates for equation (4) reflect the same pattern of order effects: average SRM is 0.77 ($\sigma = 0.29$) when "Felt" appears first, and 0.35 ($\sigma = 0.37$) when "Would Have Reported" appears first. The smaller sample size ($N = 180$ versus 932) reduces precision, rendering the interaction effect statistically insignificant, though economically large. The cross-subject first-response estimate of SRM, 0.24 ($\sigma = 0.34$), is based on an even smaller sample (56 observations for "Felt" first and 44 observations for "Would Have Reported" first) and lacks statistical significance.

For loss of a loved one, equation (5) shows that the mean of SRM is similar, 0.49 ($\sigma = 0.12$), representing 36.6% of a standard deviation for the distribution of prospective reported happiness for this event. Interestingly, in equation (6), we see no evidence that the order of presentation affects our estimate

of SRM (although the standard error of the interaction term is large). The cross-subject first-response estimate of SRM is 0.78 ($\sigma = 0.17$). Patterns for retrospective questions are similar (equations (7) and (8)), but the sample size is even smaller than for romantic breakups ($N = 120$), so the coefficients are not statistically significant. However, the cross-subject first-response estimate of SRM is 0.80 ($\sigma = 0.30$).

Consistent with our discussion of Panels E to G of Figures 1 and 2, we mostly obtain null effects for the positive events. The overall means for SRM cluster around zero, and the coefficients generally lack statistical significance. The one exception is that we find significant order effects for the prospective questions about job promotions, but a change in the order simply flips the sign of the difference; overall, the mean difference is 0.01 ($\sigma = 0.12$).

As discussed in Section 4, while self-reported misreporting of prospective or retrospective happiness conditional on the negative events we consider does not necessarily imply that people misreport happiness contemporaneously, it is reasonable to think the second proposition follows from the first. To provide some suggestive evidence concerning this issue, we estimate regressions of the following form:

$$h_i = \alpha + \beta h_i^e + \gamma r_i^e + X_i\delta + \varepsilon_i,$$

where h_i is the current happiness subject i reports, h_i^e is the happiness i expects to feel (or felt in the case of retrospective questions) after event e, r_i^e is the happiness i expects to report (or would have reported in the case of retrospective questions) after event e, and ε_i is a disturbance term. Notice that we can rewrite this regression as

$$h_i = \alpha + (\beta + \gamma)h_i^e + \gamma SRM_i^e + X_i\delta + \varepsilon_i.$$

In other words, it describes the relationship between happiness and self-reported misreporting, conditional on the happiness i expects to feel (or felt in the case of retrospective questions) after event e. In Section 4, we explained that people may report h_i^e accurately even if they misstate current happiness. In that case, one might expect to find $\beta + \gamma > 0$, either because happier people generally tend to report higher levels of happiness, or because different people cardinalize happiness differently. If the mechanisms that drive self-reported misreporting also affect the accuracy with which respondents report current happiness, we would also expect to find $\gamma > 0$—in other words, people who say they would exaggerate happiness conditional on a specified event also exaggerate current happiness.

Results appear in Table 2. There are seven regressions, one for each combination of event and time perspective (again with the exception of the retrospective perspective for the "dream job" event). As expected, $\beta + \gamma$ is strictly positive in all seven regressions and statistically significant in six of them. More importantly, γ is strictly positive in six of the seven regressions and statistically significant in four of them. This finding is consistent with the hypothesis that misreporting also infects measures of current happiness.

Taken as whole, the findings reported in this section have troubling implications for the use of happiness data. Suppose, for example, that a negative event occurring simultaneously with the receipt of $100 leaves reported happiness unchanged. One might be tempted to conclude that the compensating variation for the event is $100. But if, as our results suggest, people understate the impact on happiness of certain events while correctly stating the impact of others (including the receipt of money), the true compensating variation may be greater than $100. More generally, our results suggest that the magnitude of the reporting bias may be related to the specific combination of salient events the respondent has recently experienced. In other words, this bias does not necessarily preserve the ordering of outcomes, as would a uniform bias or simple compression.

6. Implications for projection bias

The survey questions we pose concerning expected happiness contingent on some future event resemble those used in studies that construct measures of projection bias (see, e.g., Gilbert et al., 1998, and Loewenstein et al., 2003). Accordingly, as an application, we use our results to estimate the extent to which measured projection bias may reflect misreporting of happiness once events actually occur.

The term "projection bias" refers to a tendency for people to exaggerate the resemblance between their future tastes and their current tastes. A widely used strategy for measuring projection bias involves comparing happiness experienced after a positive or negative event occurs, and prospective expectations of happiness contingent on that event occurring. The standard finding is that people believe negative events will lead to lower happiness, and positive events to greater happiness, than is actually the case.

Our findings raise the possibility that evidence of projection bias may be attributable, in whole or in part, to systematic misreporting of happiness. Specifically, if people exaggerate their happiness when responding to survey questions after bad events, they will appear to be more adaptable than they actually are.

To gauge the *potential* significance of misreporting, we will assume provisionally that self-reported misreporting provides an accurate (or at least conservative) measure of the degree to which people misreport happiness. (See also Section 7 for validation.) Under that assumption, we can estimate *ex post* happiness following some specified event by subtracting SRM from reported happiness. We then obtain an alternative estimate of projection bias by subtracting predicted happiness from this adjusted measure of *ex post* happiness.[6]

[6] Our adjustment does not account for the possibility that expectations concerning happiness following some specified event may differ from reports of those expectations. In that sense, it is only a partial "fix." For the reasons discussed in Section 4, it may be reasonable to assume that misreporting of contingent expectations is less severe than

Table 3 uses our data to construct alternative estimates of projection bias for three of the four events we consider (romantic breakups, losses of loved ones, and promotions). It displays the means of (1) ex post happiness, reported by those who have recently experienced the event,[7] (2) anticipated happiness after experiencing the event, reported by those who have not recently experienced it, and (3) the standard measure of projection bias (the difference between (1) and (2)). We find strong evidence of projection bias for the negative events we consider: the difference between the means is 1.44 ($\sigma = 0.14$) for romantic breakups, and 2.23 ($\sigma = 0.17$) for loss of a loved one. For the positive event (promotion), we find no evidence of significant projection bias: the difference between the means is 0.18 ($\sigma = 0.17$).

To adjust these measures of projection bias for self-reported misreporting, we use the estimates of SRM reported in Table 1. For prospective romantic breakups, our estimate of average SRM is 0.57 when we use all the data (equation (1)), and 0.74 when we avoid order and anchoring effects by limiting the sample to first responses (equation (2)). The first figure represents 40% of measured projection bias, while the second represents 51%. For loss of a loved one, our estimate of average SRM is 0.49 when we use all the data (equation (5)), and 0.78 when we limit the sample to first responses (equation (6)). The first figure represents 22% of measured projection bias, while the second represents 35%. For promotions, we do not find meaningful projection bias (as noted above), and the modification makes no difference.

While these results imply that a significant fraction of projection bias may be attributable to misreporting of happiness, they also appear to confirm the importance of the bias, at least for the negative events we consider. However, it is essential to keep two considerations in mind. First, for the reasons discussed at the end of Section 4, under plausible assumptions SRM is a lower bound on the bias in contemporaneous reports of happiness. In that case, our modified measure of projection bias serves as an upper bound rather than as a point estimate. Second, the standard measure of projection bias is also susceptible to other critiques. For example, because happiness is measured on a unitless scale, respondents may renormalize the scale when their circumstances change. If they always use the same numerical value to indicate happiness on a (locally) typical day, they will appear more adaptable than they actually are. Accordingly, even if misreporting does not fully explain the evidence commonly cited in favor of projection bias, it could potentially do so in combination with other factors.

misreporting of contemporaneous happiness. Even if that assumption is false, our analysis still calls the reliability of common measures of projection bias into question.

[7] As explained in Section 4, our surveys identify subjects who have experienced the events in question within the previous six months. We also ask how many months prior to the survey the event occurred. Consequently, we could in principle identify subjects who experienced the event shortly before the survey, just as the prospective question asks. However, the resulting samples would be extremely small. Analyses of the data reveal no relationships between happiness responses and the time elapsed since the occurrence of the event. We therefore report average happiness for all respondents who experienced the event within the previous six months.

To be clear, the literature on projection bias also marshals other types of evidence. For example, the current weather appears to exert disproportionate influence over the purchase and retention of durable goods (see, e.g., Conlin, O'Donoghue, and Vogelsang, 2007, and Busse, Pope, Pope, and Silva-Risso, 2015). However, these findings have other possible explanations—for example, that people do not have well-defined preferences, and that cues impacting the salience of particular aspects of experience may influence the weight they receive during process of "preference construction."

7. Validation of the validation method

In Section 4, we identified conditions under which self-reported misreporting would either accurately capture or provide a lower bound on the degree of misreporting. Because true happiness is unobservable, we cannot test those conditions directly. However, we can validate our validation method indirectly by comparing self-reported misreporting to actual misreporting in another setting where the truth is likewise unknown for individuals, but the distribution of outcomes is known for the group.[8]

For this purpose, we conducted a second experiment consisting of two components surveys. The first is, effectively, a replication of previous studies, beginning with Fischbacher and Föllmi-Heusi (2013), that measure the extent to which people lie when only they know the truth: we ask each online participant to flip a coin 10 times, and then to report the number of heads, knowing that they will receive a bonus equal to that number times $0.10. For example, if they report flipping eight heads, they receive a bonus of $0.80. While we cannot observe the number of heads for any given participant, we know the true population distribution, and can test whether it coincides with the distribution of reports.

The second component survey implements our validation method. Specifically, we describe the first survey to a second group of participants and ask what they think they would report. We then compare the distribution of anticipated reports to the distribution of actual reports, as well as to the true population distribution.

The first survey starts with a screen that asks the participant to flip a coin ten times, and to record the number of heads. In the second survey, participants view the same screen, and are then asked whether they think they would actually flip a coin.

The next screen in the first survey describes the bonus scheme and asks the participant to report the number of heads. It includes the following statement: *We would appreciate an honest answer*. In the second survey, participants view the same screen. Those who previously said they would not flip a coin are asked whether they would do so at this point in the survey. All participants in both surveys also answer an understanding question concerning the incentive scheme.

[8] We are grateful to Nick Netzer for suggesting the general strategy of validating the method in a domain for which the truth is known.

Participants in the second survey then see a screen on which they are asked to indicate how they think they would have answered the question concerning the outcome of flipping the coin 10 times. For those who say they would actually flip a coin, we ask:

> *Imagine that you flipped the coin 10 times, and that it has come up Heads [N] times. What do you think you would answer concerning the number of times the coin came up Heads, out of 10, if you were actually a participant in the study? We would appreciate your honest response.*

We supply the number N to each participant, mimicking the population distribution of coin flips. For those who say they would not actually flip a coin, we ask:

> *Given that you would not have flipped the coin 10 times, what do you think you would have answered concerning the number of times the coin came up Heads, out of 10? We would appreciate your honest response.*

We fielded the two surveys on Prolific in November 2023. In total, 202 respondents completed the first survey and 212 completed the second. The full text of each survey appears in the Appendix.[9]

The mean of the true population distribution for the number of heads out of 10 flips of a fair coin is 5.0. In contrast, the mean reported number of heads in the first survey is 5.59 ($\sigma = 0.13$). We strongly reject the hypothesis that people respond truthfully ($p < 0.001$). Instead, we find that the typical respondent exaggerates the number of heads by 0.59. In the second survey, the mean number of heads participants say they would report in such a survey is 5.57 ($\sigma = 0.13$). We strongly reject the hypothesis that people say they would respond truthfully ($p < 0.001$). Instead, we find that the typical respondent says they would exaggerate the number of heads by 0.57. Remarkably, the difference between misreporting (0.59) and self-reported misreporting (0.57) is only 0.02 ($\sigma = 0.18$).[10] According to the point estimates, self-reported misreporting slightly understates misreporting, but we cannot reject the hypothesis that the two are identical ($p = 0.90$).

Figure 3 chows CDFs for (i) the true population distribution for the number of heads out of 10 flips of a fair coin (in blue), (ii) the distribution of reports from the first survey (in red), and (iii) the distribution of predicted responses from the second survey (in green). The second and third of these CDFs are similar but not identical. However, both are plainly right-shifted relative to the first; the second distribution is weakly higher than the first in the sense of first-order stochastic dominance, and the same is nearly true for the third versus the first.[11]

---

[9] We conducted this study under human subjects protocol IRB-42264 approved by Stanford University's IRB.

[10] Initially we collected 98 responses to the first survey and 105 to the second. The mean reported number of heads was 5.69 ($\sigma = 0.18$) in the first survey, and the mean number of heads participants said they would report was 5.62 ($\sigma = 0.20$) in the second survey. We then doubled the sample size to check that the small difference between these means was not a fluke.

[11] For zero heads and one head, the cumulative density of predicted responses from the second survey is slightly greater than the cumulative density for coin flips.

Overall, 88% of the participants in our second survey said they would actually flip a coin if they participated in the first survey. For those subjects, we can compare the hypothetical outcomes we gave them to their predicted responses. The average simulated number of heads is 4.84, and the average predicted response is 5.40. The average difference for this group, 0.56, is only slightly less than the overall average (0.57).[12] Self-reported misreporting declines sharply as the hypothesized outcome becomes more favorable. Subjects say they would exaggerate by an average of 1.16 flips ($\sigma = 0.29$) if the hypothesized outcome is less than 4, by an average of 0.49 flips ($\sigma = 0.12$) if the hypothesized outcome is between 4 and 6, and by an average of 0.09 flips ($\sigma = 0.19$) if the hypothesized outcome is greater than 6.

Despite the difference in domains, these results confirm our conjecture that self-reported misreporting detects, and does not overstate, actual misreporting.

8. Conclusions

This paper introduces a novel method for validating measures of self-reported well-being. Specifically, we ask subjects how happy they would feel (or did feel) after some specified event. We also ask them how they would respond (or would have responded) to a survey question about their happiness after the same event. The difference between these two responses is an estimate of "self-reported misreporting." Our results show that self-reported misreporting is substantial for two types of events (romantic breakups and death of a loved one), and negligible for two others (workplace promotion and finding a dream job). They also imply that self-reported misreporting may account for a substantial fraction of measured projection bias. We have validated the method in another domain wherein the truth is likewise unknown for individuals, but the distribution of outcomes is known for the group. These findings suggest that considerable caution is warranted when interpreting studies that attempt to infer how conditions affect welfare by analyzing measures of self-reported well-being.

[12] The average response for those who said they would not flip a coin is 6.84. In other words, this group is more inclined to exaggerate relative to the true distribution (by 1.84, rather than 0.56). Although the group who said they would not flip a coin is small (25 subjects), the difference between the average response for this group and those who said they would flip a coin is statistically significant ($p<0.001$).

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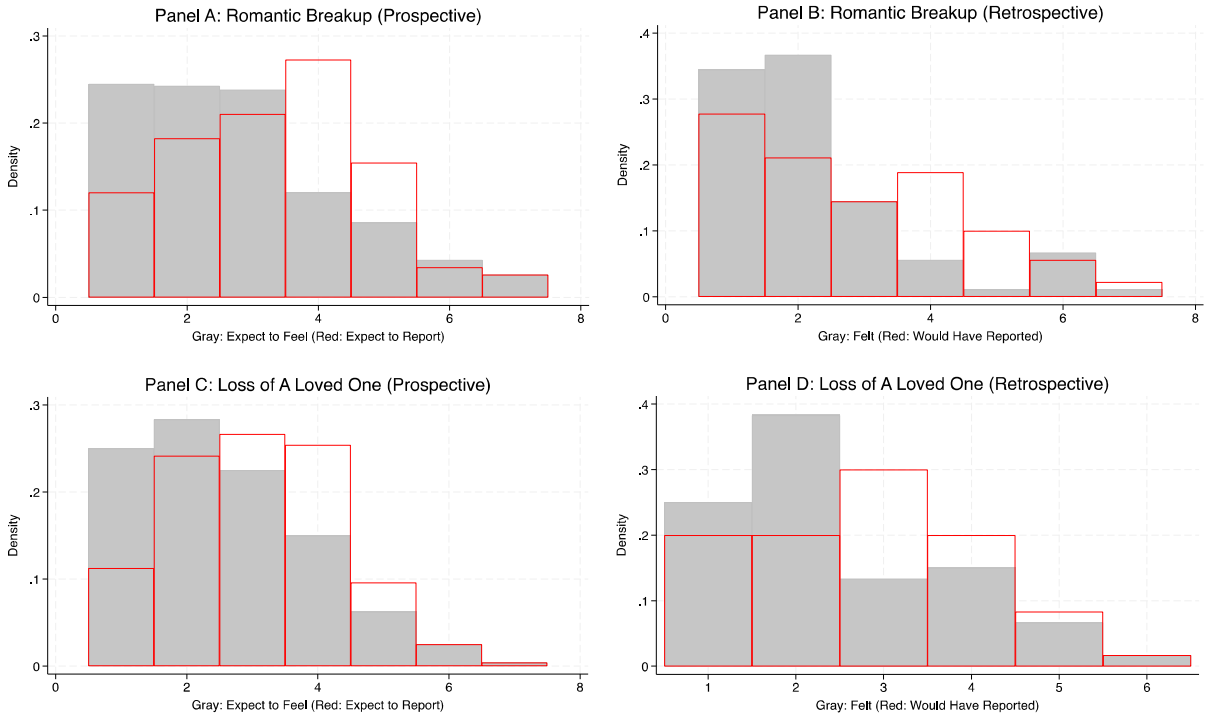
Figure 1: Expected Happiness versus Expected Reports of Happiness

Negative Events: Panels A through D



Positives Events: Panels E through G

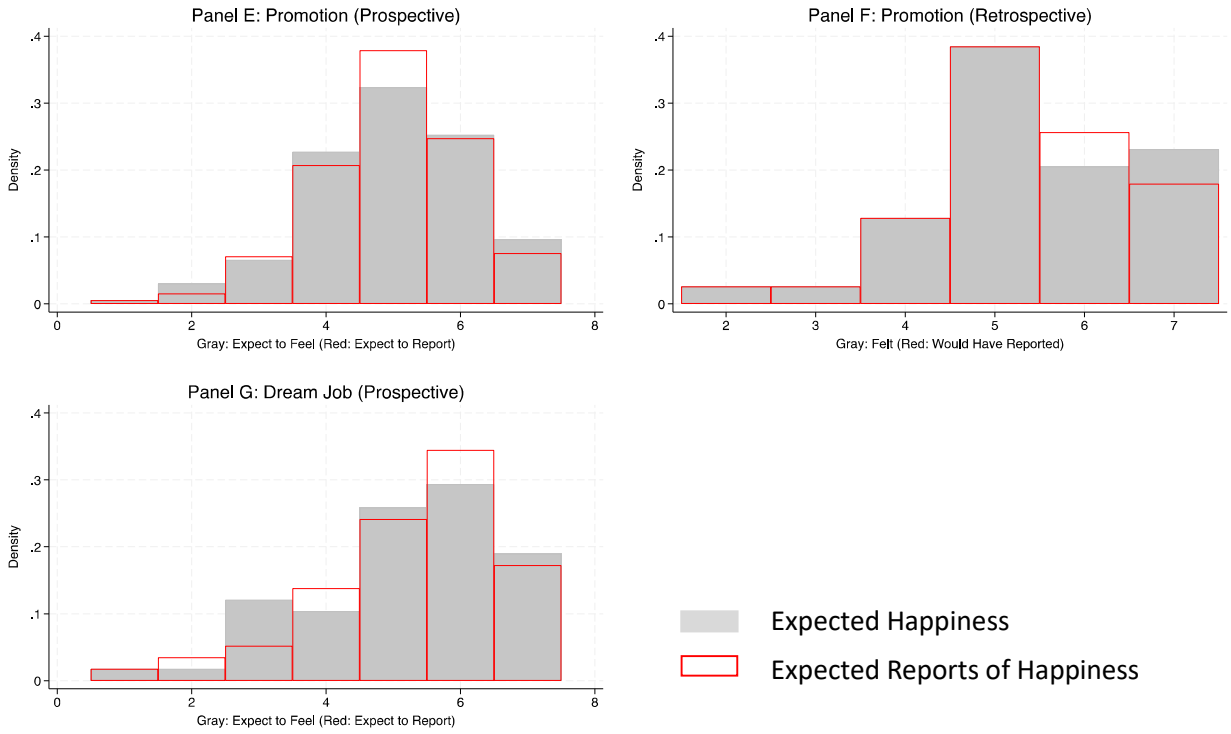
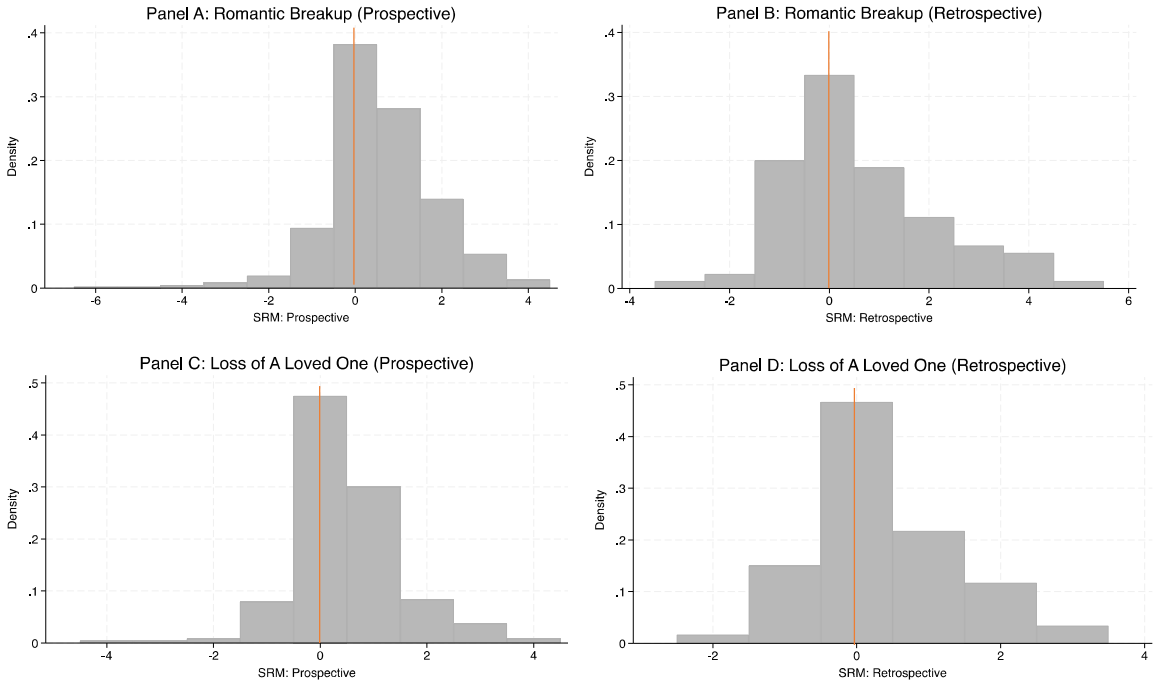


Figure 2: Distributions for Self-Reported Misreporting (within subject)

Negative Events: Panels A through D



Positive Events: Panels E through G

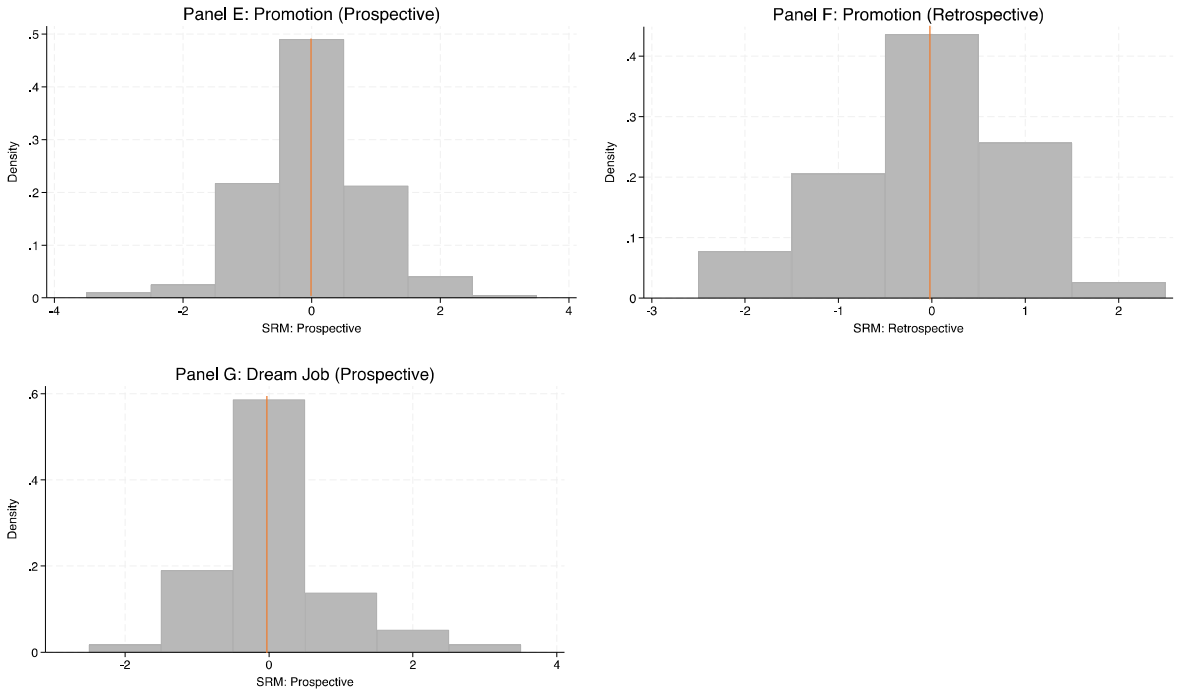


Figure 3: CDFs for Number of Heads (Actual, Reported, and Predicted)

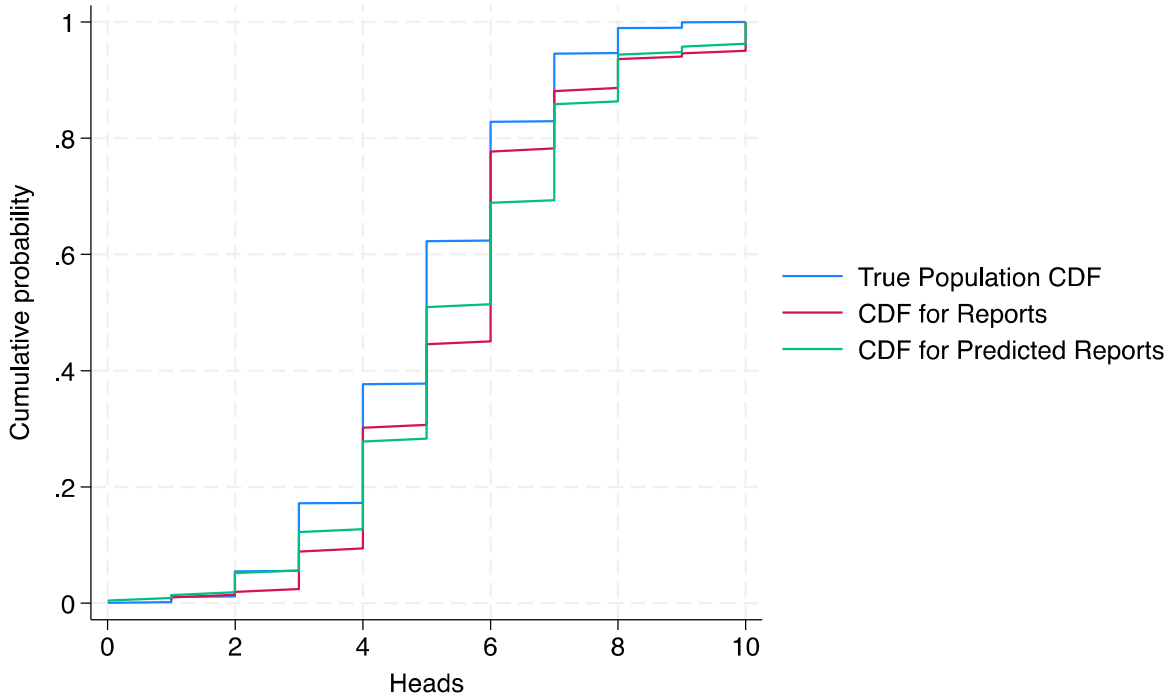


Table 1: Pooled Regressions for Expected Happiness and Expected Reports of Happiness

	Event	Temporal Perspective	Report		ReportBeforeFeel × Report		ReportBeforeFeel		Constant		Observations
					Negative Events:						
(1)	Breakup	Prospective	0.57***	(0.10)					3.32***	(0.19)	932
(2)	Breakup	Prospective	0.77***	(0.14)	-0.39**	(0.19)	0.36**	(0.14)	3.13***	(0.20)	932
(3)	Breakup	Retrospective	0.61***	(0.23)					2.76***	(0.41)	180
(4)	Breakup	Retrospective	0.77***	(0.29)	-0.41	(0.47)	-0.04	(0.34)	2.69***	(0.42)	180
(5)	Loss of Loved One	Prospective	0.49***	(0.12)					3.06***	(0.21)	480
(6)	Loss of Loved One	Prospective	0.52***	(0.17)	-0.07	(0.23)	0.33**	(0.16)	2.83***	(0.23)	480
(7)	Loss of Loved One	Retrospective	0.37	(0.23)					3.31***	(0.40)	120
(8)	Loss of Loved One	Retrospective	0.43	(0.32)	-0.13	(0.46)	0.50	(0.33)	3.09***	(0.43)	120
					Positive Events:						
(9)	Promotion	Prospective	0.01	(0.12)					4.92***	(0.20)	396
(10)	Promotion	Prospective	-0.32**	(0.16)	0.67***	(0.23)	-0.58***	(0.16)	5.22***	(0.21)	396
(11)	Promotion	Retrospective	-0.05	(0.27)					5.31***	(0.50)	78
(12)	Promotion	Retrospective	-0.09	(0.37)	0.09	(0.55)	0.13	(0.40)	5.20***	(0.56)	78
(13)	Dream Job	Prospective	0.07	(0.26)					4.76***	(0.42)	116
(14)	Dream Job	Prospective	-0.27	(0.36)	0.70	(0.52)	0.00	(0.37)	4.68***	(0.48)	116

Note: All regressions include controls for gender, religiosity, and survey wave.

Table 2: Regressions of Current Happiness on Contingent Expectations

	Current Surveyed Happiness						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Event	Breakup		Loss of a loved one		Promotion		Dream job
Temporal Perspective	Prospective	Retrospective	Prospective	Retrospective	Prospective	Retrospective	Prospective
Feel (β+γ)	0.06	0.24**	0.20***	0.03	0.41***	0.62***	0.20
	(0.04)	(0.09)	(0.07)	(0.14)	(0.07)	(0.14)	(0.16)
Report (γ)	0.08**	0.06	0.22***	0.25	0.34***	-0.08	0.53***
	(0.04)	(0.08)	(0.07)	(0.15)	(0.08)	(0.14)	(0.17)
Gender	-0.25***	0.01	-0.01	-0.15	0.07	-0.06	-0.18
	(0.08)	(0.19)	(0.13)	(0.29)	(0.12)	(0.24)	(0.23)
Religiosity	0.13***	0.23***	0.10***	0.07	-0.01	0.13*	0.01
	(0.02)	(0.08)	(0.03)	(0.07)	(0.03)	(0.07)	(0.07)
wave2	-0.05	0.07					
	(0.09)	(0.24)					
Constant	4.38***	2.95***	3.18***	4.08***	0.72**	1.95***	0.51
	(0.21)	(0.48)	(0.30)	(0.63)	(0.36)	(0.71)	(0.64)
Observations	466	90	240	60	198	39	58

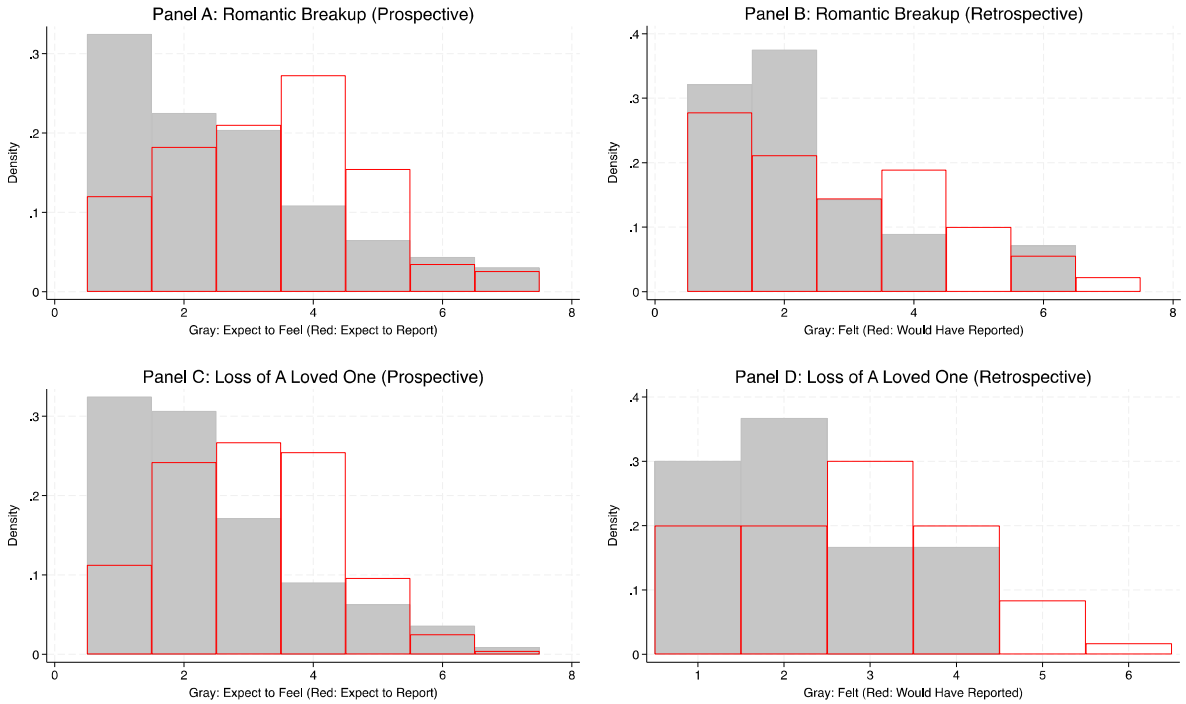
Table 3: Measured Projection Bias

		Romantic Breakups	Loss of A Loved One	Promotion
Ex Post Report	Mean	4.23	4.82	5.15
	SD	1.17	1.10	0.99
	N	90	60	39
Report of Ex Ante Anticipation	Mean	2.79	2.58	4.97
	SD	1.56	1.34	1.22
	N	466	240	198
Ex Post Report - Report of Ex Ante Anticipation	Diff	1.44	2.23	0.18
	SE	0.14	0.17	0.18
	N1	90	60	39
	N2	466	240	198

Appendix A: Additional Results

Figure A.1: Expected Happiness versus Expected Reports of Happiness, Feel Before Report

Negative events: Panels A through D



Positive events: Panels E through G

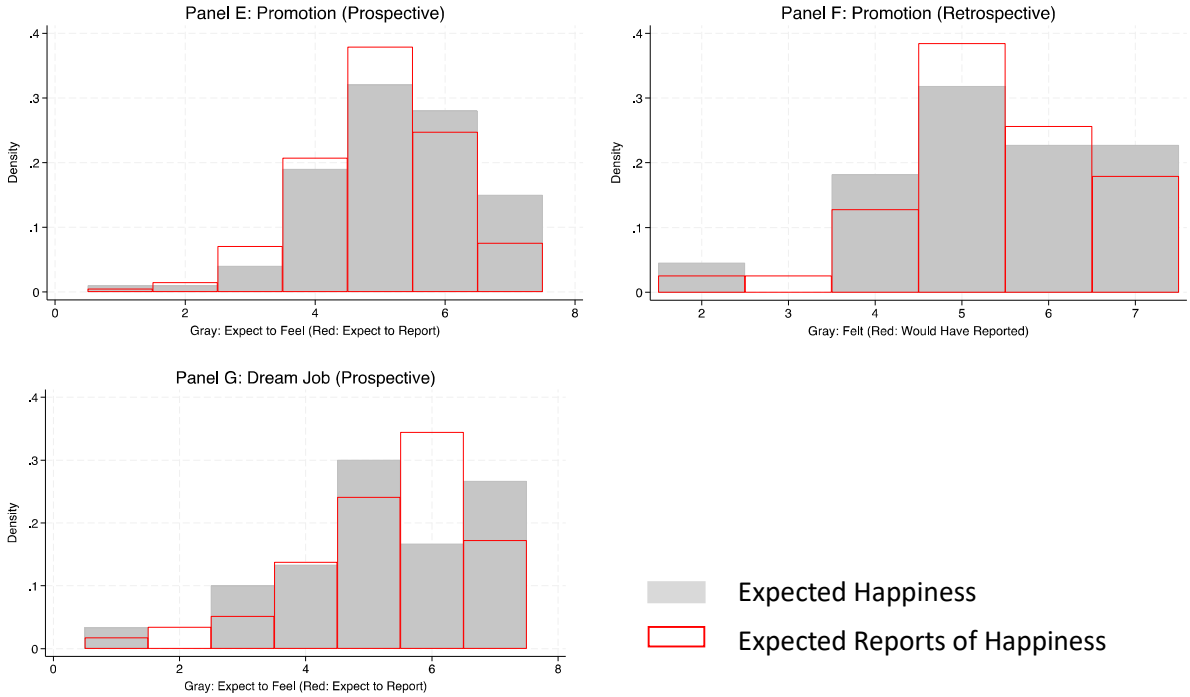
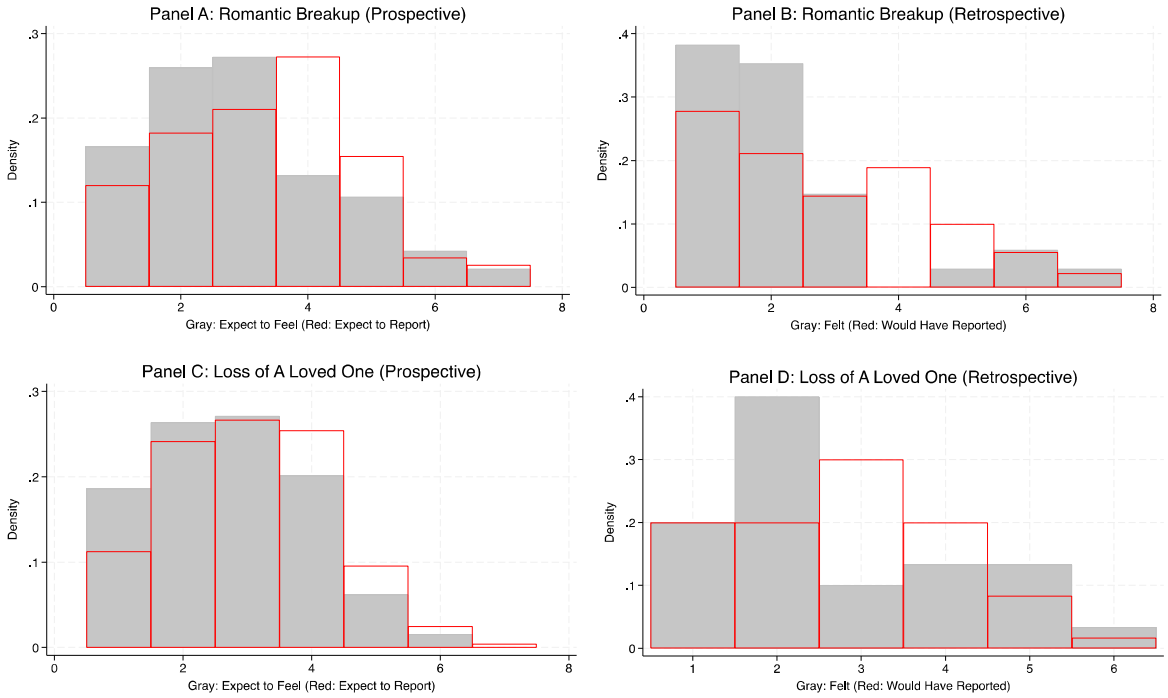
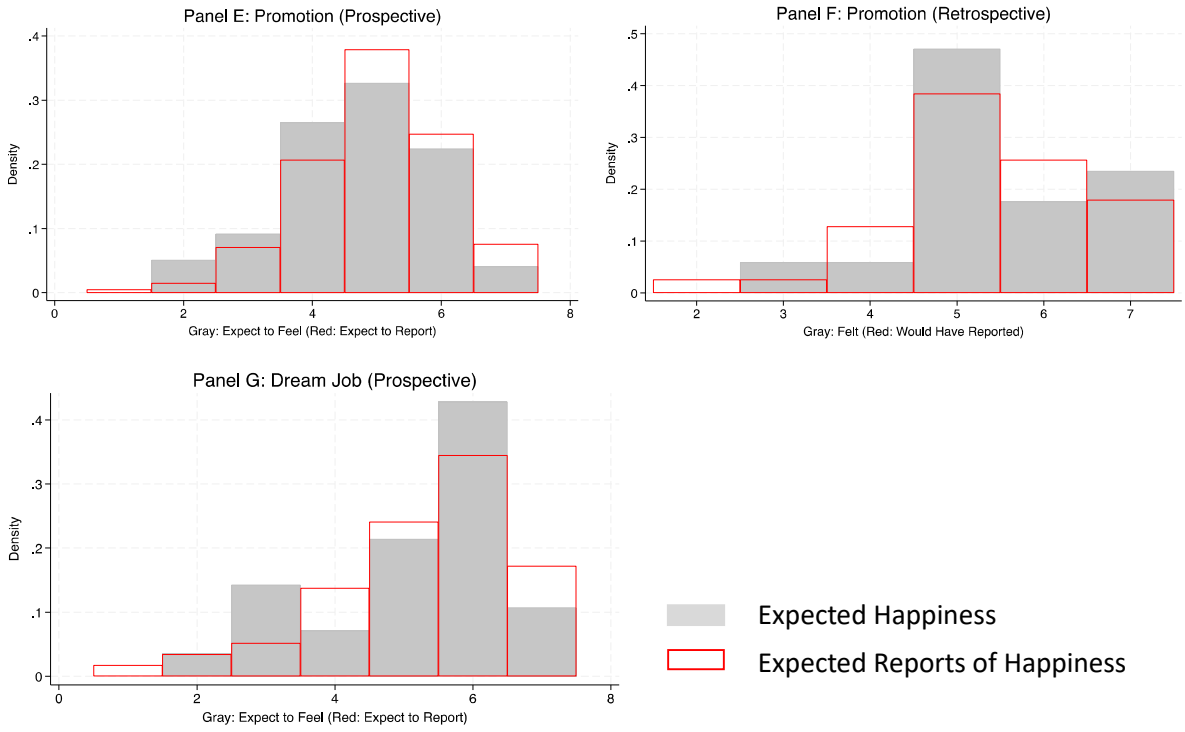


Figure A.2: Expected Happiness versus Expected Reports of Happiness, Report Before Feel

Negative events: Panels A through E

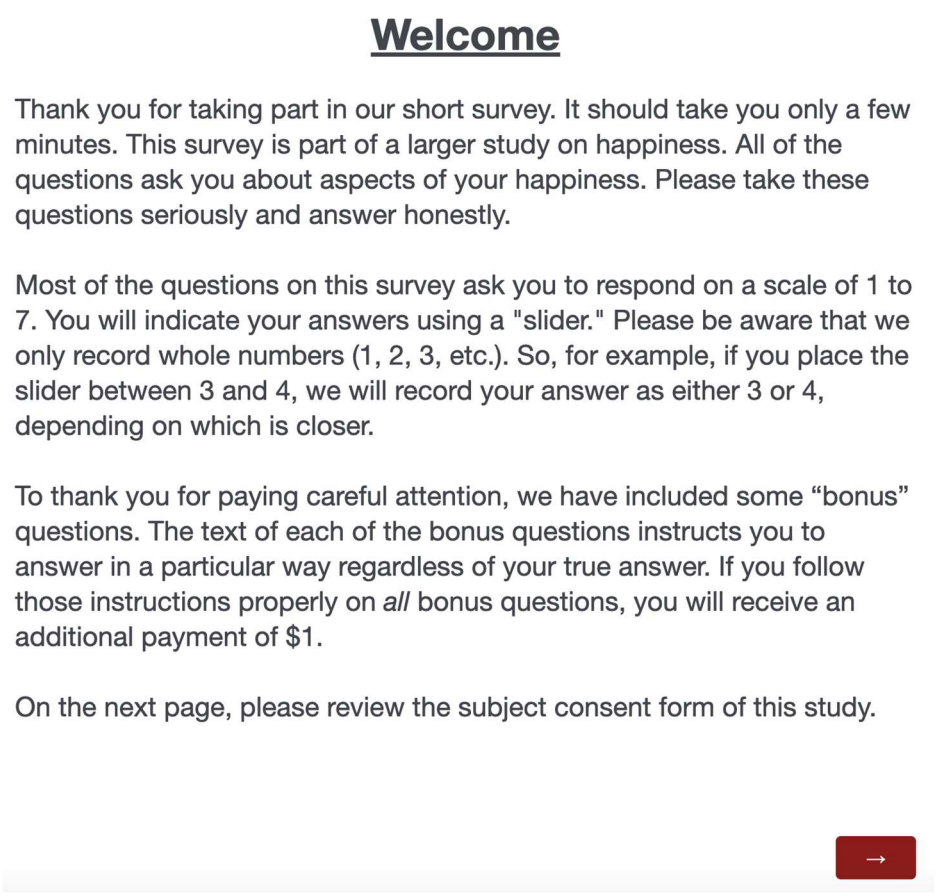


Positive events: Panels E through G



Appendix B: Survey Instruments, Main Experiment

For all subjects and all surveys:



Welcome

Thank you for taking part in our short survey. It should take you only a few minutes. This survey is part of a larger study on happiness. All of the questions ask you about aspects of your happiness. Please take these questions seriously and answer honestly.

Most of the questions on this survey ask you to respond on a scale of 1 to 7. You will indicate your answers using a "slider." Please be aware that we only record whole numbers (1, 2, 3, etc.). So, for example, if you place the slider between 3 and 4, we will record your answer as either 3 or 4, depending on which is closer.

To thank you for paying careful attention, we have included some “bonus” questions. The text of each of the bonus questions instructs you to answer in a particular way regardless of your true answer. If you follow those instructions properly on *all* bonus questions, you will receive an additional payment of $1.

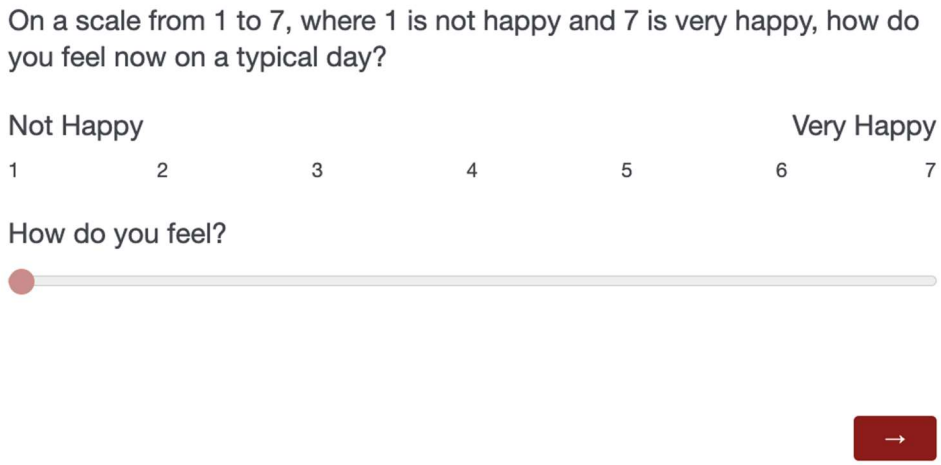
On the next page, please review the subject consent form of this study.

→

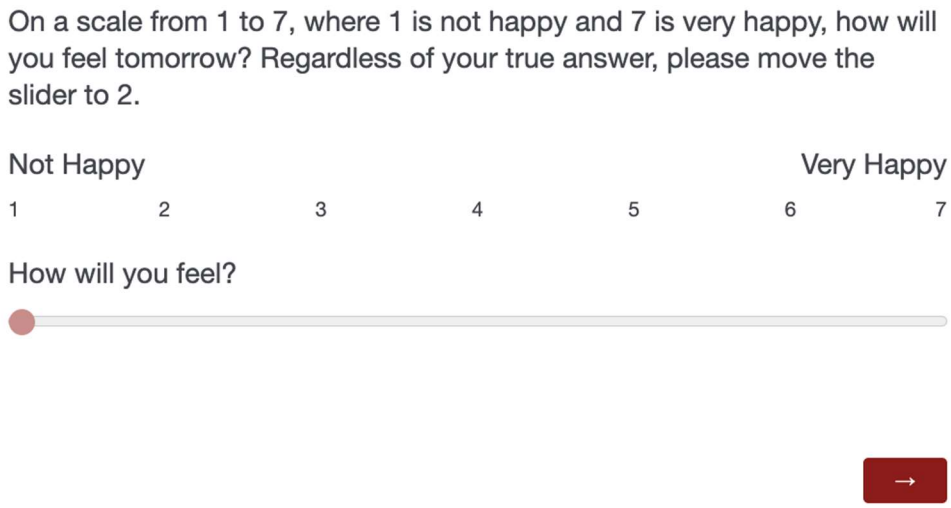
For all subjects and all surveys:

IRB regulatory information
Protocol Director: Professor B. Douglas Bernheim
Protocol Number: IRB-42264
IRB Approval Date: 10/28/22
Expiration Date: Does Not Expire
DESCRIPTION: You are invited to participate in a research study on decision-making. You will be asked to read several pages of instructions. Then you will be asked to make several choices that will determine the precise amount you will be paid, and then possibly answer several survey questions.
RISKS AND BENEFITS: We cannot and do not guarantee or promise that you will receive any benefits from this study. There are no risks associated with this study.
TIME INVOLVEMENT: Your participation in this experiment will take approximately as long as is indicated in the advertisement.
PAYMENTS: You will be compensated at the advertised rate.
SUBJECT'S RIGHTS: If you have read this form and have decided to participate in this project, please understand your participation is voluntary and you have the right to discontinue participation at any time without penalty or loss of benefits to which you are otherwise entitled. You have the right to refuse to answer particular questions. Your individual privacy will be maintained in all published and written data resulting from the study.
CONTACT INFORMATION:
Questions, Concerns, or Complaints: If you have any questions, concerns or complaints about this research study, its procedures, risks and benefits, you should ask the experimenter, Tingyan Jia, tingyan@stanford.edu, or (650) 387-9919.
Independent Contact: If you are not satisfied with how this study is being conducted, or if you have any concerns, complaints, or general questions about the research or your rights as a participant, please contact the Stanford Institutional Review Board (IRB) to speak to someone independent of the research team at (650)-723-2480 or toll free at 1-866-680-2906. You can also write to the Stanford IRB, Stanford University, Stanford, CA 94305-5401.
By continuing with this study, you are consenting to participate.
Please make a copy of this consent form for your own records. You can do so by right-clicking and selecting "print" in most browsers. If you cannot do so on your browser, please contact the experimenter for a copy of the consent form.

For all subjects and all surveys:

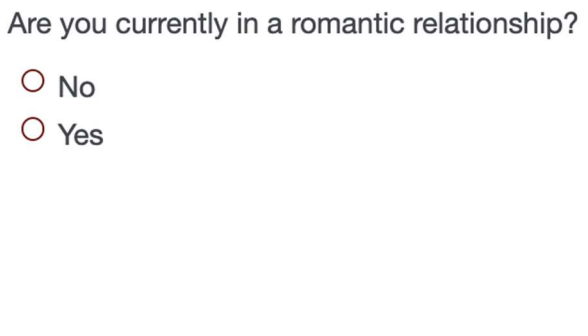


For all subjects and all surveys:



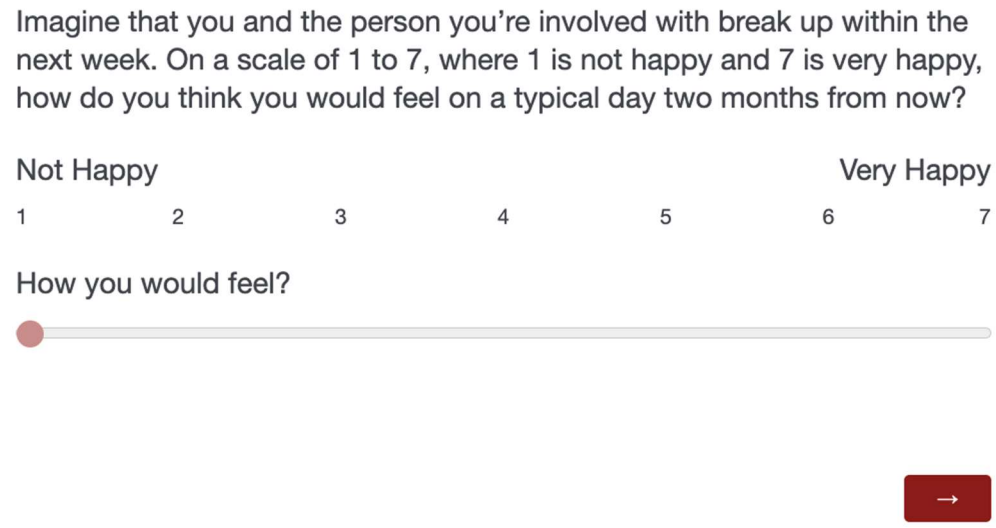
The Romantic Breakup Survey:

For all subjects:



If they answer "Yes" to the relationship question:

--Half of the subjects are asked "Feel" before "Report". The "Feel" question:

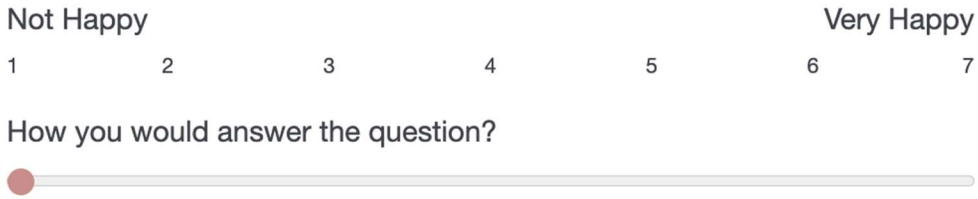


If they answer "Yes" to the relationship question:

--Half of the subjects are asked "Feel" before "Report". The "Report" question:

Now that you've told us how you would **ACTUALLY** feel two months after a breakup, we'd like to know what you think you would **SAY IF ASKED** on a typical day two months after a breakup to rate your overall happiness.

Continue to imagine that you and the person you're involved with break up within the next week. Two months later, you participate in a survey, which asks you how happy you feel on a typical day using a scale from 1 to 7, where 1 is not happy and 7 is very happy. How do you think you would *answer* that question at that time?

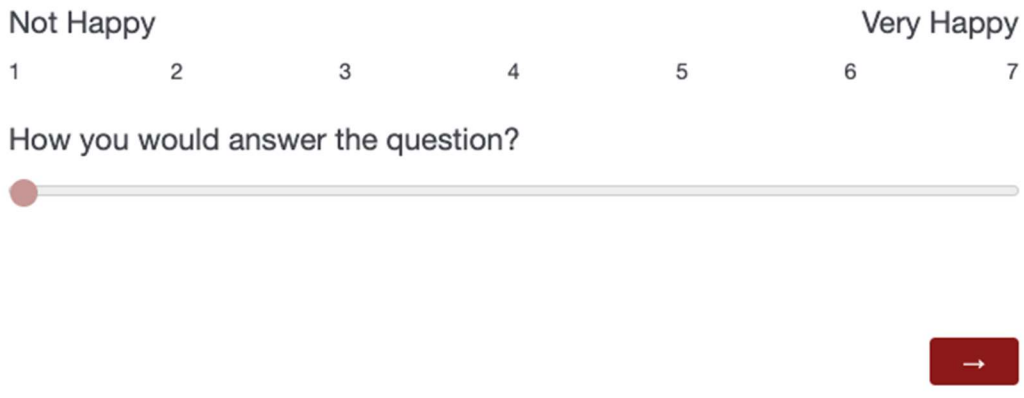


If they answer "Yes" to the relationship question:

--The other half of the subjects are asked "Report" before "Feel". The "Report" question:

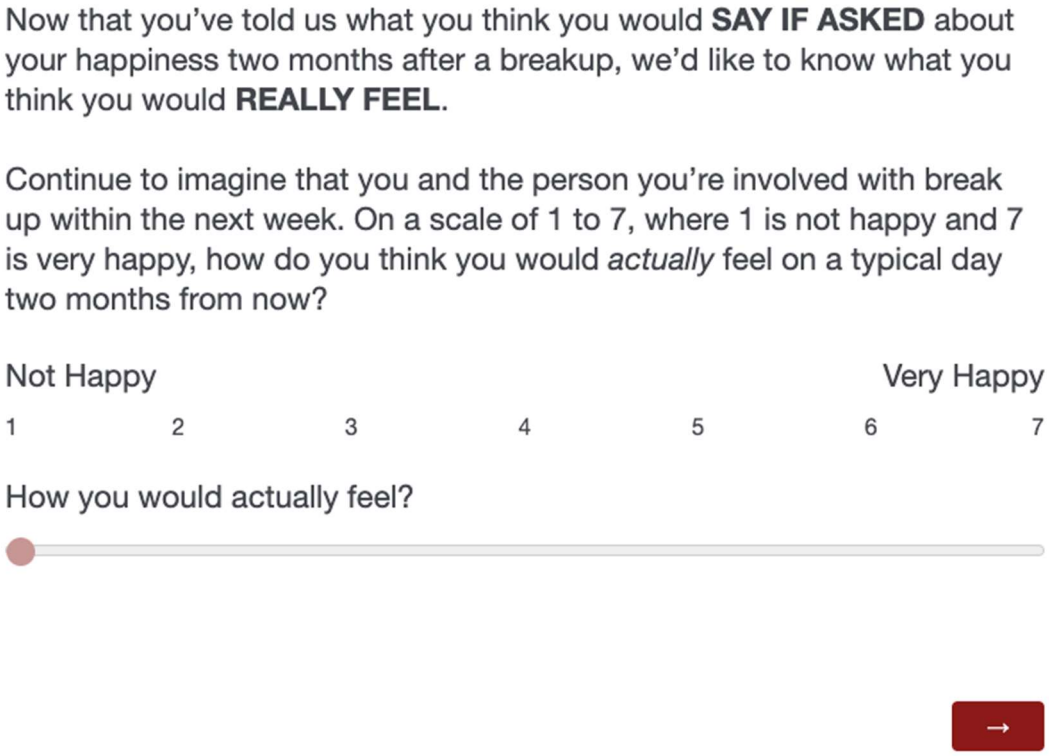
Imagine that you and the person you're involved with break up within the next week. Two months later, you participate in a survey, which asks you how happy you feel on a typical day using a scale from 1 to 7, where 1 is not happy and 7 is very happy. How do you think you would *answer* that question at that time?

Important: We are **NOT** asking you how you would **ACTUALLY** feel. Our question is what you think you would **SAY IF ASKED** on a typical day two months after a breakup to rate your overall happiness.

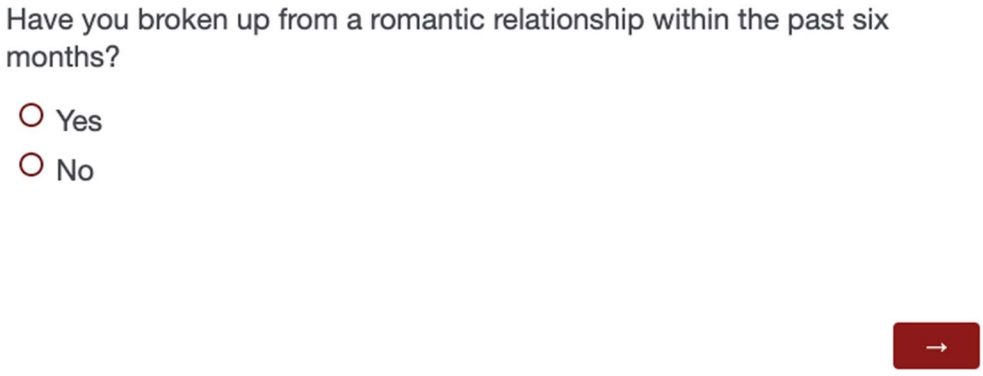


If they answer "Yes" to the relationship question:

--The other half of the subjects are asked "Report" before "Feel". The "Feel" question:



If they answer "No" to the relationship question:



If they answer "Yes" to the breakup question:

You said you had a breakup within the past 6 months. When did it happen?

- ○ About 1 week ago
- ○ About 1 month ago
- ○ About 2 months ago
- ○ About 3 months ago
- ○ About 4 months ago
- ○ About 5 months ago
- ○ About 6 months ago

→

For those who had a recent breakup, half of them are asked "Felt" before "Would Have Reported". The "Felt" question:

On a scale to 1 to 7, where 1 is not happy and 7 is very happy, how happy did you feel on a typical data shortly after your breakup?

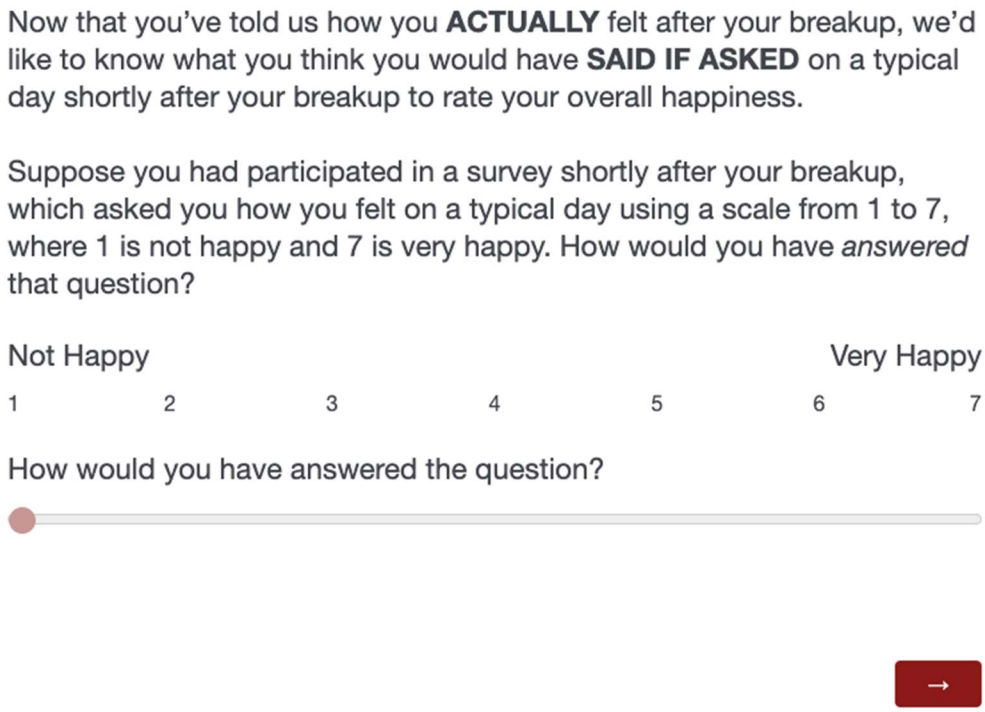
Not Happy Very Happy

1 2 3 4 5 6 7

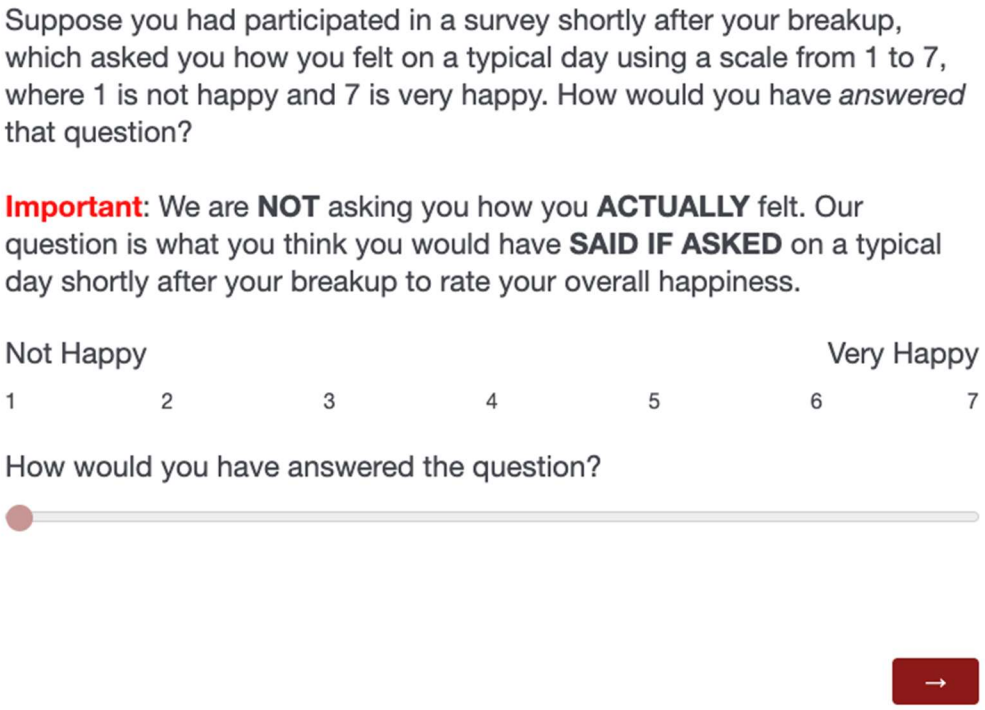
How did you feel then?

→

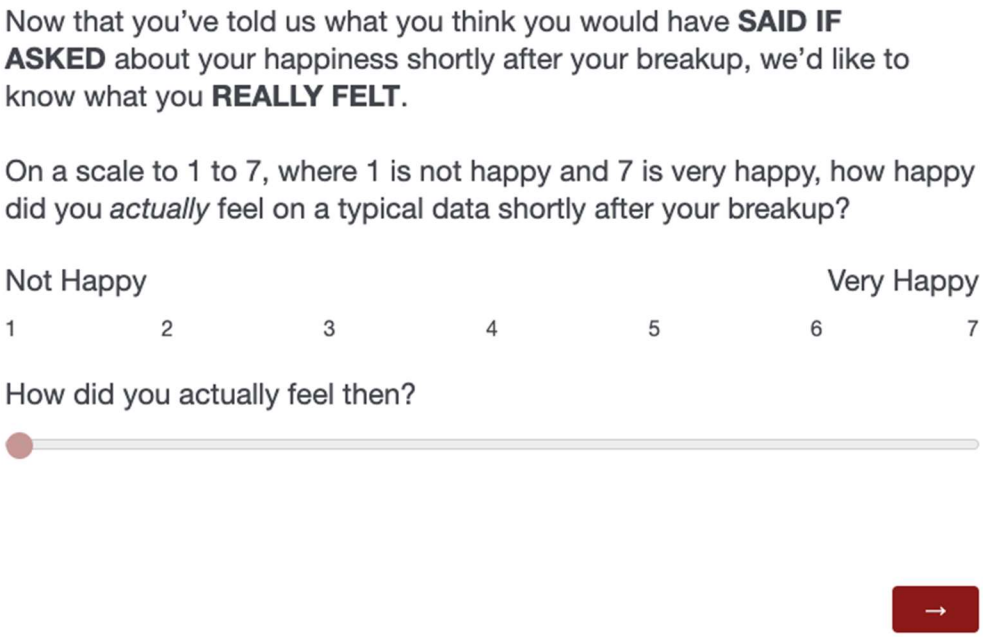
For those who had a recent breakup, half of them are asked "Felt" before "Would Have Reported". The "Would Have Reported" question:



For those who had a recent breakup, the other half of them were asked "Would Have Reported" before "Felt". The "Would Have Reported" question:

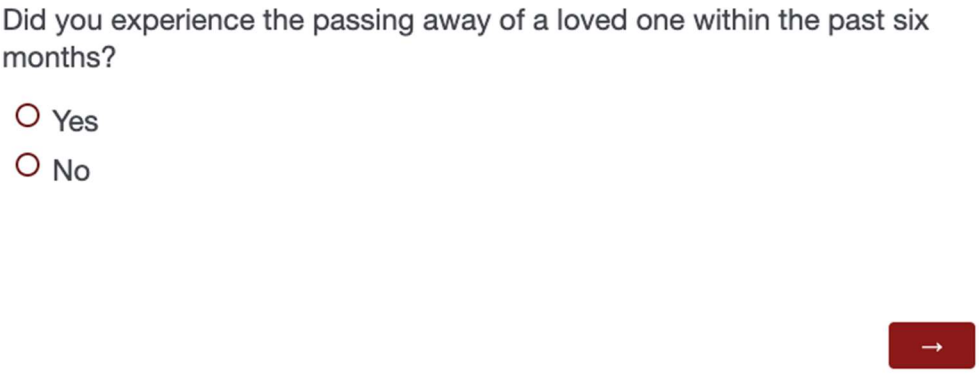


For those who had a recent breakup, the other half of them were asked "Would Have Reported" before "Felt". The "Felt" question:



<u>The "Loss of a Loved One" Survey:</u>

For all subjects:



If they answer "Yes" to the loss of a loved one question:

You said a loved one passed away within the past 6 months. When did it happen?

- ○ About 1 week ago
- ○ About 1 month ago
- ○ About 2 months ago
- ○ About 3 months ago
- ○ About 4 months ago
- ○ About 5 months ago
- ○ About 6 months ago

→

If they answer "Yes" to the loss of a loved one question:

--Half of the subjects are asked "Felt" before "Would Have Reported". The "Felt" Question:

On a scale to 1 to 7, where 1 is not happy and 7 is very happy, how happy did you feel on a typical data shortly after your loved one passed away?

Not Happy Very Happy

1 2 3 4 5 6 7

How did you feel then?

→

If they answer "Yes" to the loss of a loved one question:

--Half of the subjects are asked "Felt" before "Would Have Reported". The "Would Have Reported" Question:

Now that you've told us how you **ACTUALLY** felt after your loved one passed away, we'd like to know what you think you would have **SAID IF ASKED** on a typical day shortly after your loved one passed away to rate your overall happiness.

Suppose you had participated in a survey shortly after your loved one passed away, which asked you how you felt on a typical day using a scale from 1 to 7, where 1 is not happy and 7 is very happy. How would you have *answered* that question?

Not Happy Very Happy

1 2 3 4 5 6 7

How would you have answered the question?

→

If they answer "Yes" to the loss of a loved one question:

--The other half of the subjects are asked "Would Have Reported" before "Felt". The "Would Have Reported" Question:

Suppose you had participated in a survey shortly after your loved one passed away, which asked you how you felt on a typical day using a scale from 1 to 7, where 1 is not happy and 7 is very happy. How would you have *answered* that question?

Important: We are **NOT** asking you how you **ACTUALLY** felt. Our question is what you think you would have **SAID IF ASKED** on a typical day shortly after your loved one passed away to rate your overall happiness.

Not Happy Very Happy

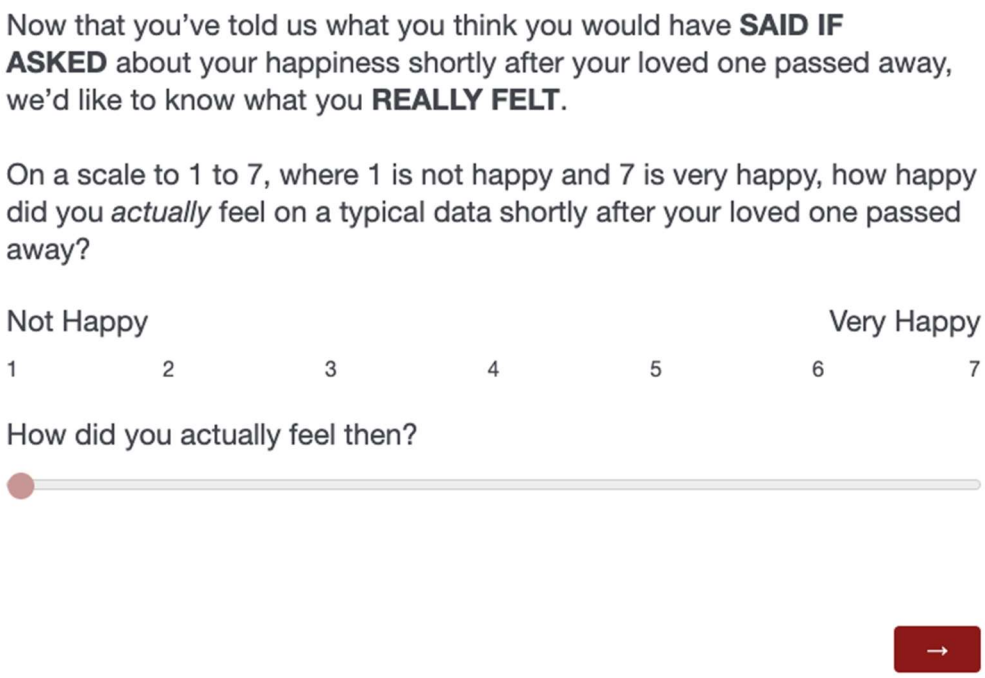
1 2 3 4 5 6 7

How would you have answered the question?

→

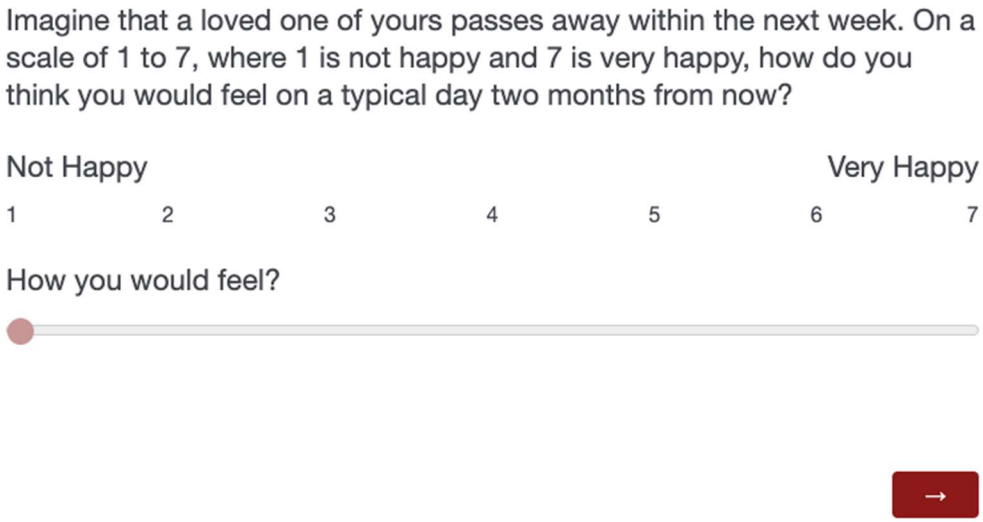
If they answer "Yes" to the loss of a loved one question:

--The other half of the subjects are asked "Would Have Reported" before "Felt". The "Felt" Question:



If they answer "No" to the loss of a loved one question:

--Half of the subjects are asked "Feel" before "Report". The "Feel" Question:



If they answer "No" to the loss of a loved one question:

--Half of the subjects are asked "Feel" before "Report". The "Report" Question:

Now that you've told us how you would **ACTUALLY** feel two months after a loved one of yours passes away, we'd like to know what you think you would **SAY IF ASKED** on a typical day two months after a loved one of yours passes away to rate your overall happiness.

Continue to imagine that a loved one of yours passes away within the next week. Two months later, you participate in a survey, which asks you how happy you feel on a typical day using a scale from 1 to 7, where 1 is not happy and 7 is very happy. How do you think you would *answer* that question at that time?

Not Happy Very Happy

1 2 3 4 5 6 7

How you would answer the question?

If they answer "No" to the loss of a loved one question:

--The other half of the subjects are asked "Report" before "Feel". The "Report" Question:

Imagine that a loved one of yours passes away within the next week. Two months later, you participate in a survey, which asks you how happy you feel on a typical day using a scale from 1 to 7, where 1 is not happy and 7 is very happy. How do you think you would *answer* that question at that time?

Important: We are **NOT** asking you how you would **ACTUALLY** feel. Our question is what you think you would **SAY IF ASKED** on a typical day two months after a loved one of yours passes away to rate your overall happiness.

Not Happy Very Happy

1 2 3 4 5 6 7

How you would answer the question?

→

If they answer "No" to the loss of a loved one question:

--The other half of the subjects are asked "Report" before "Feel". The "Feel" Question:

Now that you've told us what you think you would **SAY IF ASKED** about your happiness two months after a loved one of yours passes away, we'd like to know what you think you would **REALLY FEEL**.

Continue to imagine that a loved one of yours passes away within the next week. On a scale of 1 to 7, where 1 is not happy and 7 is very happy, how do you think you would *actually* feel on a typical day two months from now?

Not Happy Very Happy

1 2 3 4 5 6 7

How you would actually feel?

→

"Job Promotion" Survey:

Are you currently employed?

- ○ No
- ○ Yes

→

If they answer "Yes" to the employment question:

Did you receive a promotion at your job within the past six months?

- ○ Yes
- ○ No

→

If they answer "Yes" to the promotion question:

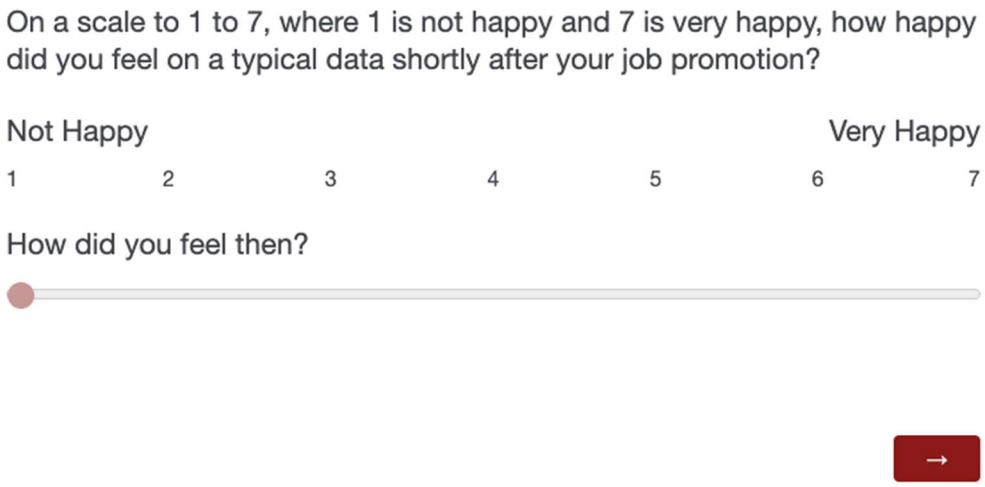
You said you received a promotion within the past 6 months. When did it happen?

- ○ About 1 week ago
- ○ About 1 month ago
- ○ About 2 months ago
- ○ About 3 months ago
- ○ About 4 months ago
- ○ About 5 months ago
- ○ About 6 months ago

→

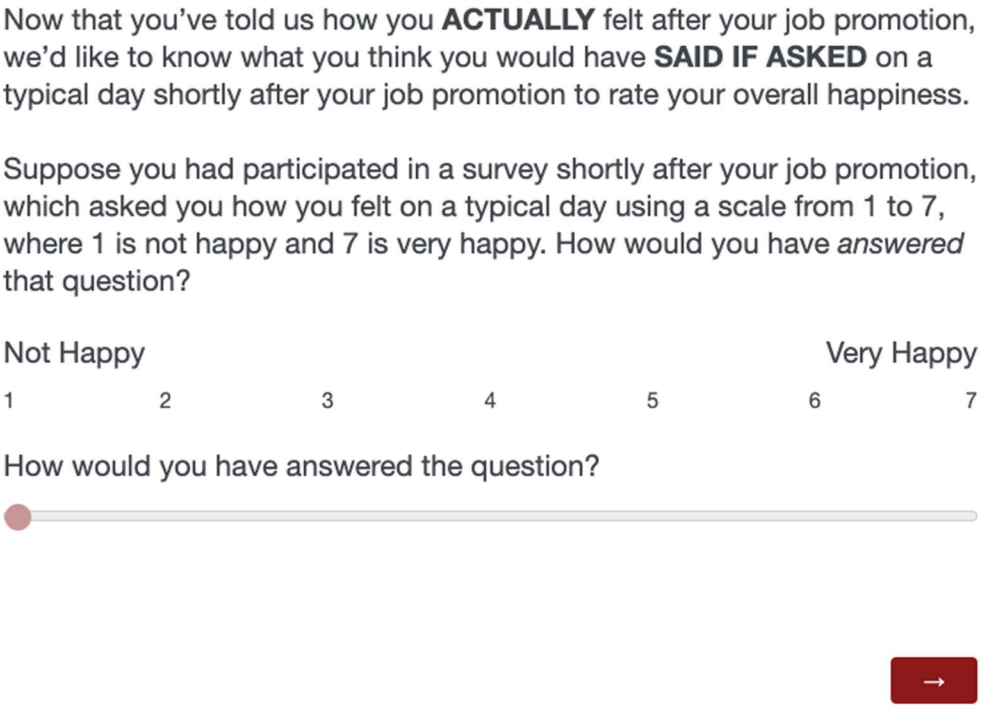
If they answer "Yes" to the promotion question:

--Half of the subjects were asked "Felt" before "Would Have Reported". The "Felt" Question:



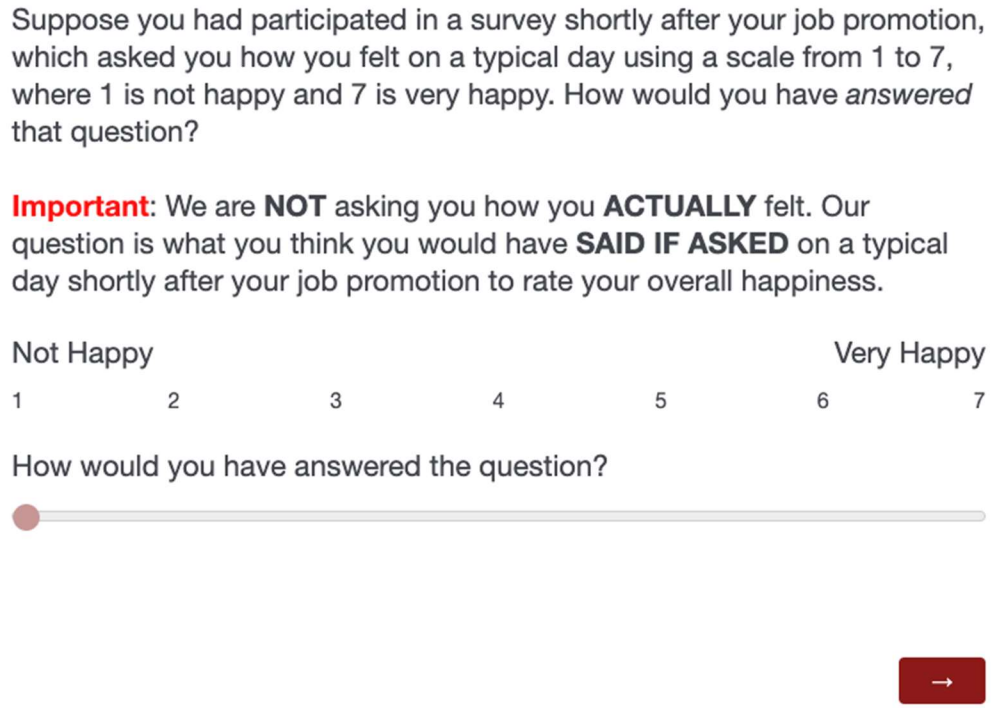
If they answer "Yes" to the promotion question:

--Half of the subjects were asked "Felt" before "Would Have Reported". The "Would Have Reported" Question:



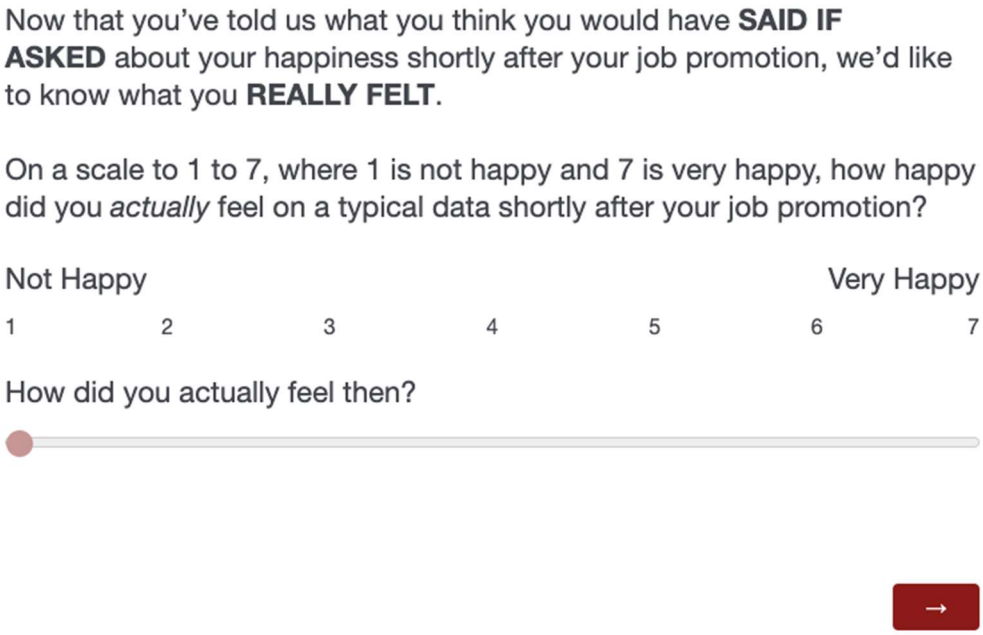
If they answer "Yes" to the promotion question:

--The other half of the subjects were asked "Would Have Reported" before "Felt". The "Would Have Reported" Question:



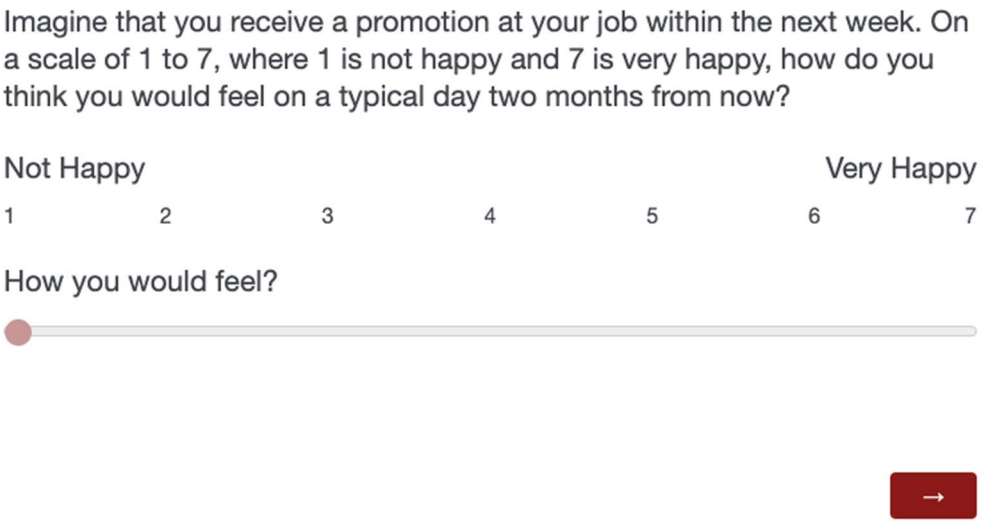
If they answer "Yes" to the promotion question:

--The other half of the subjects were asked "Would Have Reported" before "Felt". The "Felt" Question:



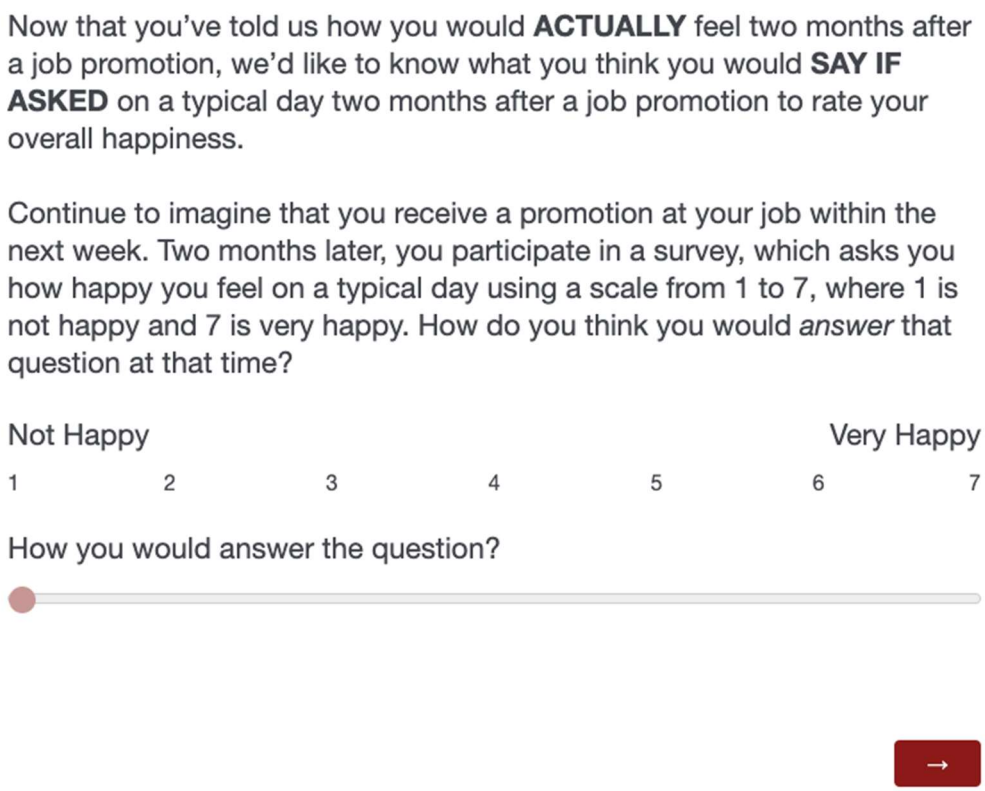
If they answer "Yes" to the employment question and "No" to the promotion question:

--Half of the subjects were asked "Feel" before "Report". The "Feel" question:



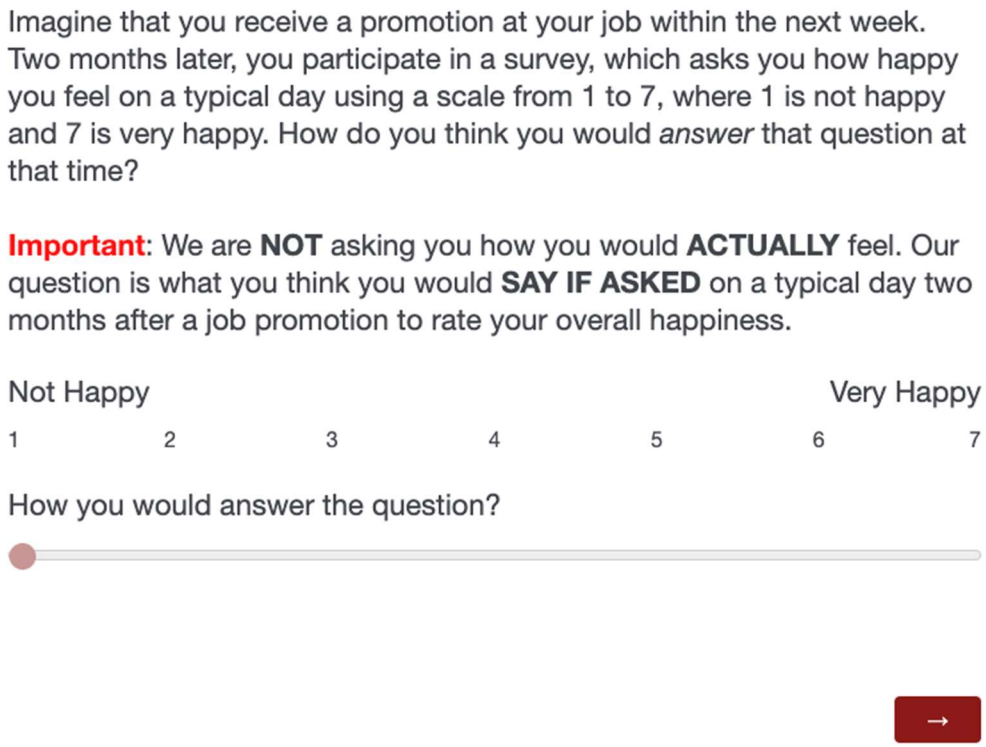
If they answer "Yes" to the employment question and "No" to the promotion question:

--Half of the subjects were asked "Feel" before "Report". The "Report" question:



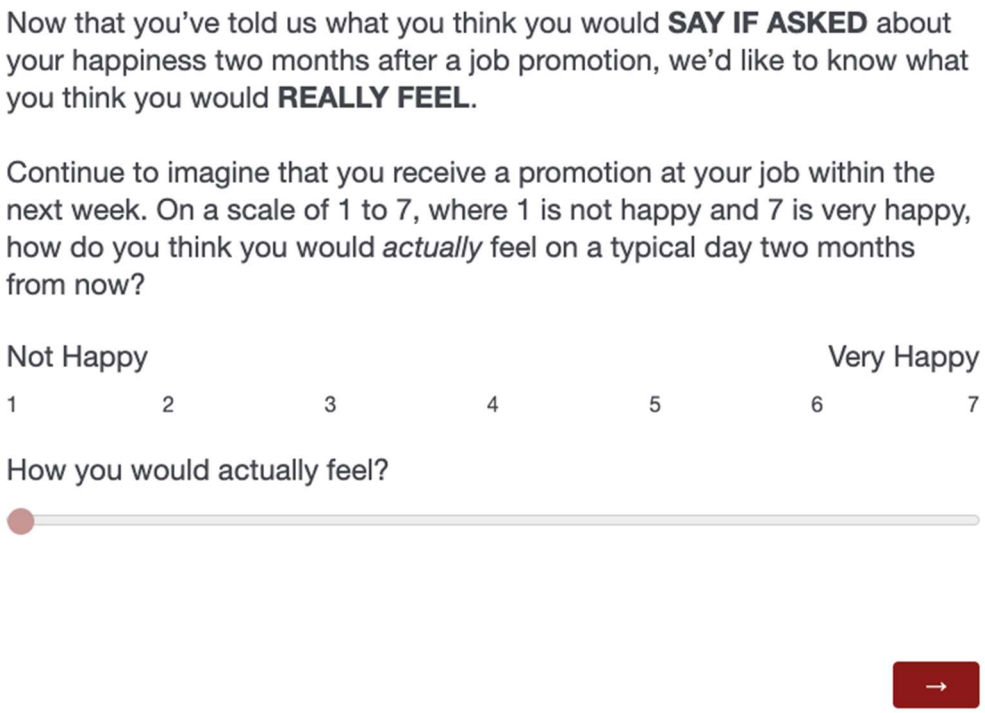
If they answer "Yes" to the employment question and "No" to the promotion question:

--The other half of the subjects were asked "Report" before "Feel". The "Report" question:

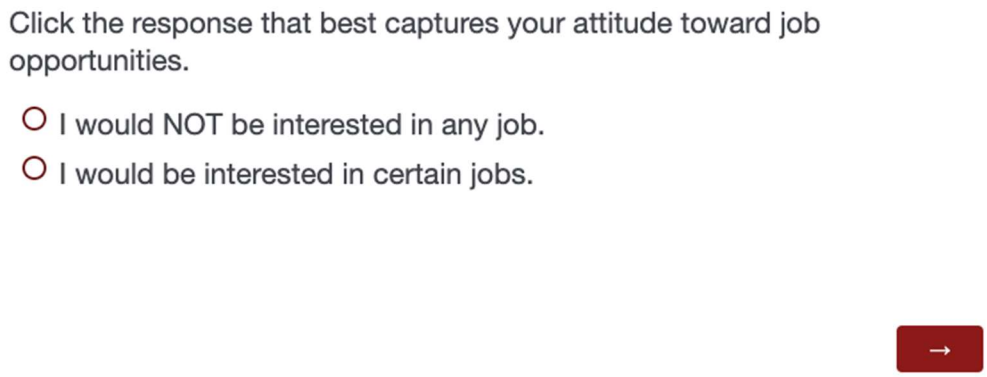


If they answer "Yes" to the employment question and "No" to the promotion question:

--The other half of the subjects were asked "Report" before "Feel". The "Feel" question:

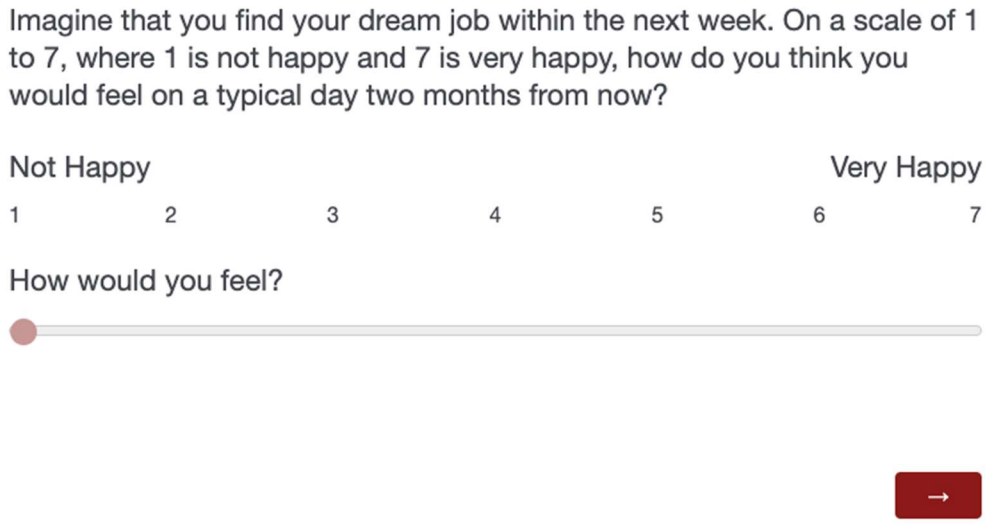


If they answer "No" to the employment question:



If they answer "Yes" to the job opportunity question:

--Half of the subjects were asked "Feel" before "Report". The "Feel" question:



If they answer "Yes" to the job opportunity question:

--Half of the subjects were asked "Feel" before "Report". The "Report" question:

Now that you've told us how you would **ACTUALLY** feel two months after you find your dream job, we'd like to know what you think you would **SAY IF ASKED** on a typical day two months after you find your dream job to rate your overall happiness.

Continue to imagine that you find your dream job within the next week. Two months later, you participate in a survey, which asks you how happy you feel on a typical day using a scale from 1 to 7, where 1 is not happy and 7 is very happy. How do you think you would *answer* that question at that time?

Not Happy Very Happy

1 2 3 4 5 6 7

How you would answer the question?

→

If they answer "Yes" to the job opportunity question:

--The other half of the subjects were asked "Report" before "Feel". The "Report" question:

Imagine that you find your dream job within the next week. Two months later, you participate in a survey, which asks you how happy you feel on a typical day using a scale from 1 to 7, where 1 is not happy and 7 is very happy. How do you think you would *answer* that question at that time?

Important: We are **NOT** asking you how you would **ACTUALLY** feel. Our question is what you think you would **SAY IF ASKED** on a typical day two months after you find your dream job to rate your overall happiness.

Not Happy Very Happy

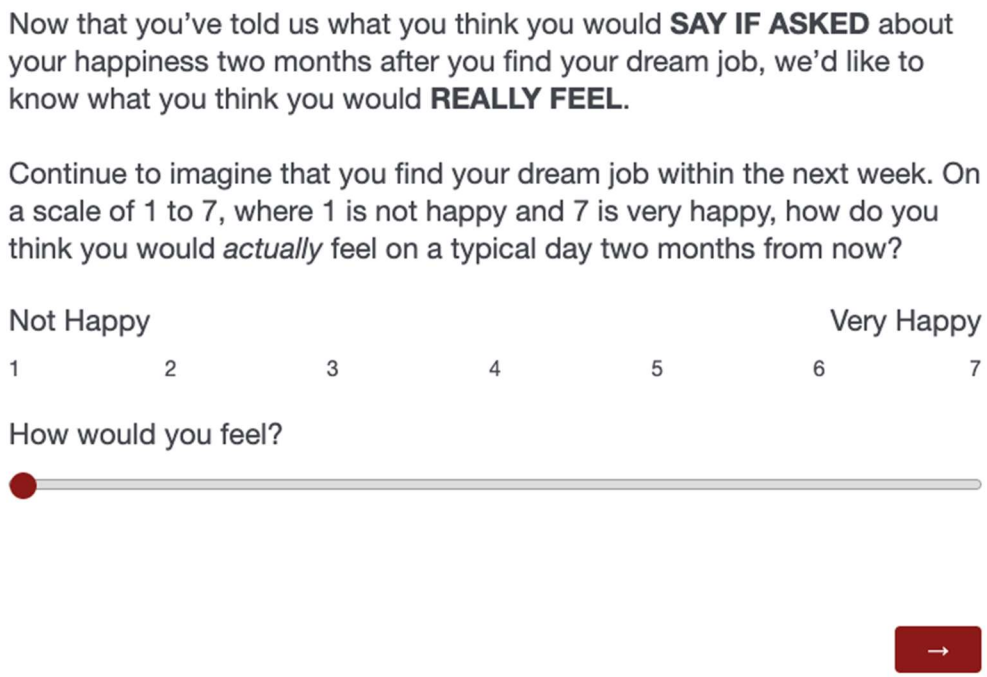
1 2 3 4 5 6 7

How you would answer the question?

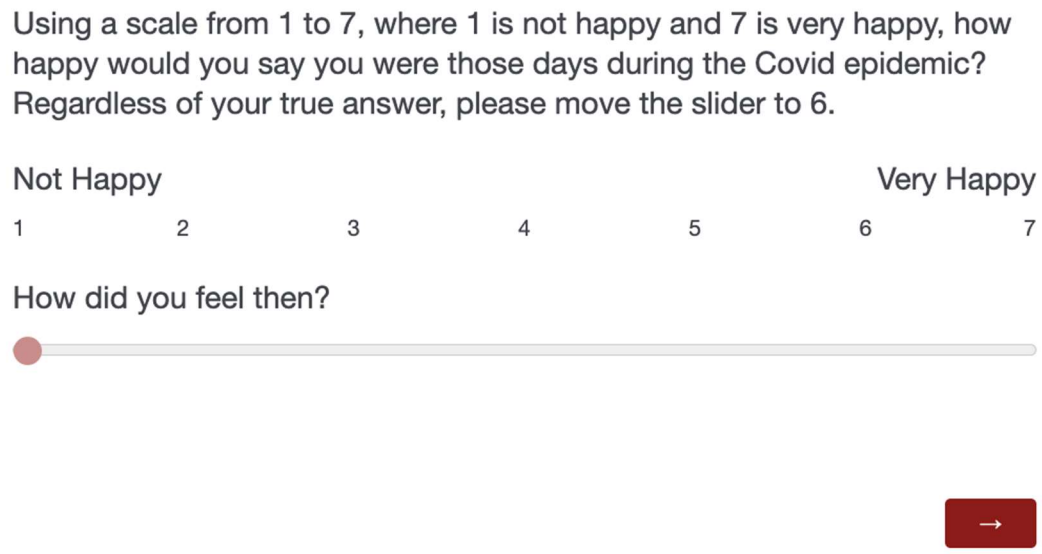
→

If they answer "Yes" to the job opportunity question:

--The other half of the subjects were asked "Report" before "Feel". The "Feel" question:



For all subjects and all surveys:



Three demographic questions for all subjects and all surveys:

How old are you this year?

- ○ 18
- ○ 19
- ○ 20
- ○ 21
- ○ 22
- ○ 23
- ○ 24
- ○ 25
- ○ Other, please specify:
- ○ Decline to state

What is your gender?

- ○ Male
- ○ Female
- ○ Non-binary
- ○ Decline to state

Using a scale from 1 to 7, where 1 is not religious and 7 is very religious, how religious do you consider yourself?

Not religious Very religious

1 2 3 4 5 6 7

How religious are you?

→

Appendix C: Survey Instruments, Supplementary Experiment (Section 7)

First Component Survey:

For All Subjects:

Please enter your Prolific ID:

→

Welcome

Thank you for taking part in our short survey. It should take you only a few minutes.

Throughout the study, there will be attention checks. You'll recognize them when you see them. You will receive a bonus of $0.50 for passing all of the attention checks and correctly answering a comprehension question about the instructions. To make sure you are paying attention now, please click on the first word of this paragraph.

On the next page, please review the subject consent form of this study.

→

IRB regulatory information

Protocol Director: Professor B. Douglas Bernheim

Protocol Number: IRB-42264

IRB Approval Date: 10/28/22

Expiration Date: Does Not Expire

DESCRIPTION: You are invited to participate in a research study on decision-making. You will be asked to read several pages of instructions. Then you will be asked to make several choices that will determine the precise amount you will be paid, and then possibly answer several survey questions.

RISKS AND BENEFITS: We cannot and do not guarantee or promise that you will receive any benefits from this study. There are no risks associated with this study.

TIME INVOLVEMENT: Your participation in this experiment will take approximately as long as is indicated in the advertisement.

PAYMENTS: You will be compensated at the advertised rate.

SUBJECT'S RIGHTS: If you have read this form and have decided to participate in this project, please understand your participation is voluntary and you have the right to discontinue participation at any time without penalty or loss of benefits to which you are otherwise entitled. You have the right to refuse to answer particular questions. Your individual privacy will be maintained in all published and written data resulting from the study.

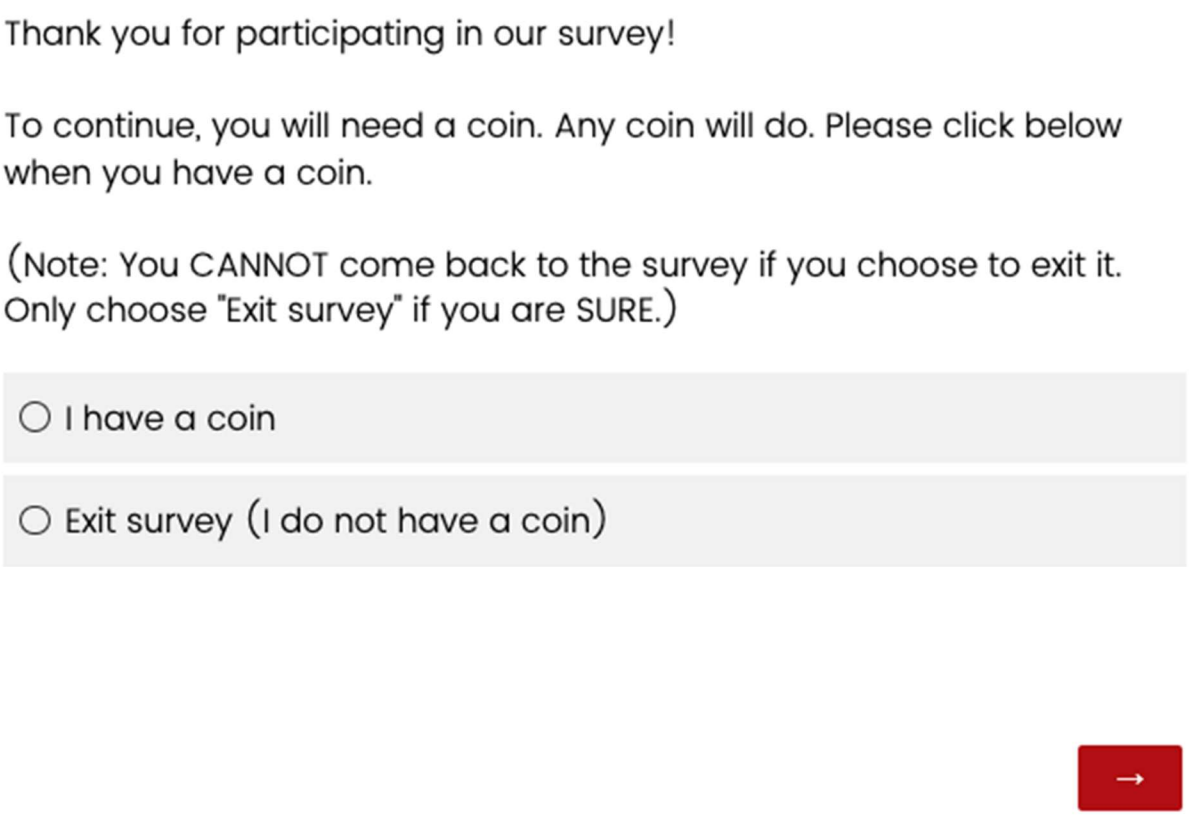
CONTACT INFORMATION:

Questions, Concerns, or Complaints: If you have any questions, concerns or complaints about this research study, its procedures, risks and benefits, you should ask the experimenter, Tingyan Jia, tingyan@stanford.edu, or (650) 387-9919.

Independent Contact: If you are not satisfied with how this study is being conducted, or if you have any concerns, complaints, or general questions about the research or your rights as a participant, please contact the Stanford Institutional Review Board (IRB) to speak to someone independent of the research team at (650)-723-2480 or toll free at 1-866-680-2906. You can also write to the Stanford IRB, Stanford University, Stanford, CA 94305-5401.

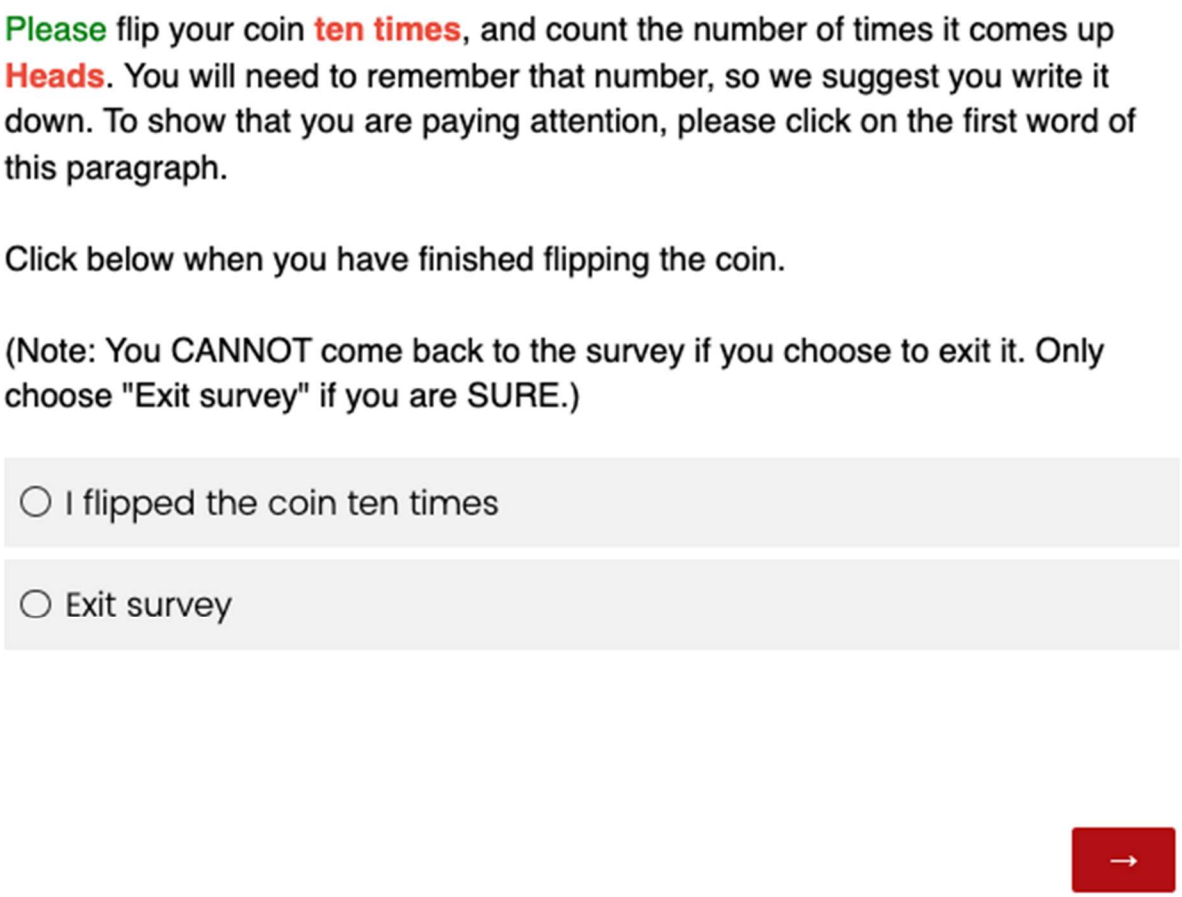
By continuing with this study, you are consenting to participate.

Please make a copy of this consent form for your own records. You can do so by right-clicking and selecting "print" in most browsers. If you cannot do so on your browser, please contact the experimenter for a copy of the consent form.



For those who selected “I have a coin”:

(Those who selected “Exit survey (I do not have a coin)” left the study at this point.)



For those who selected “I flipped the coin ten times”:

(Those who selected “Exit survey” left the study at this point.)

We would now like you to report the number of times the coin came up **Heads** out of the ten times you just flipped it.

You will receive a bonus payment in this survey that depends on the number of times the coin came up **Heads**. The table below shows the size of your bonus.

Number of times the coin flip was **Heads**	Your bonus
0	0
1	$0.10
2	$0.20
3	$0.30
4	$0.40
5	$0.50
6	$0.60
7	$0.70
8	$0.80
9	$0.90
10	$1.00

We would appreciate an honest answer.

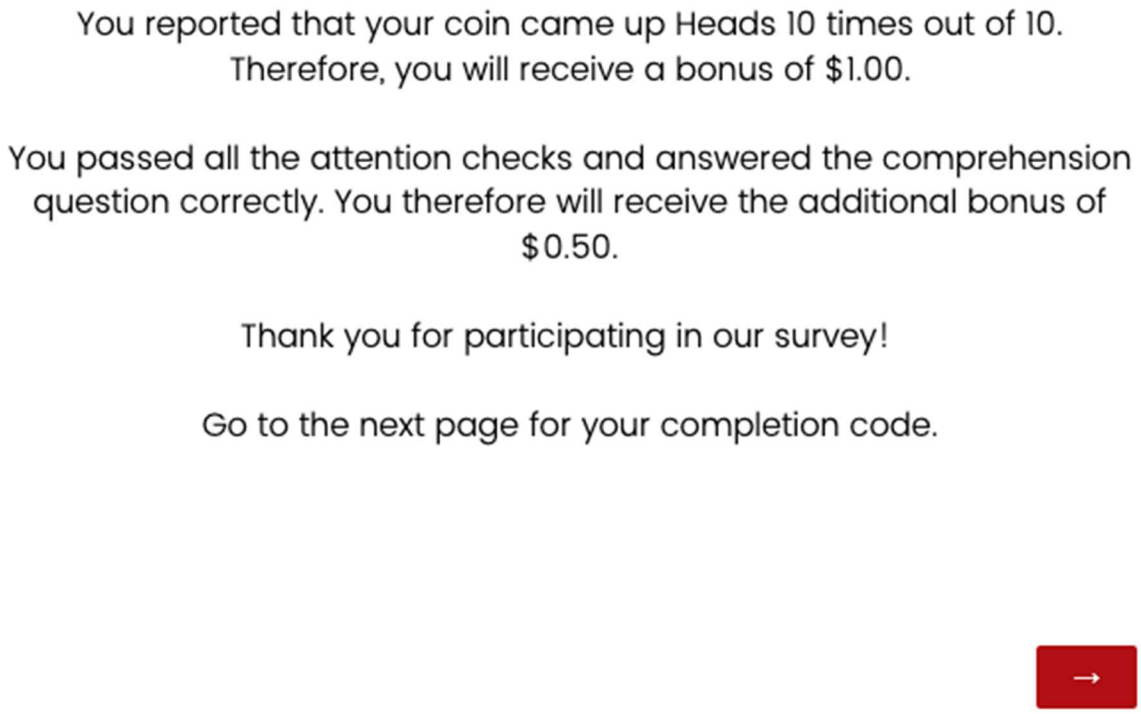
Number of times the coin came up **Heads**, out of 10:

To see whether you understand how the bonus will be paid, please answer the following question:

How much bonus will someone receive if they report they flipped 10 times and had 9 heads?

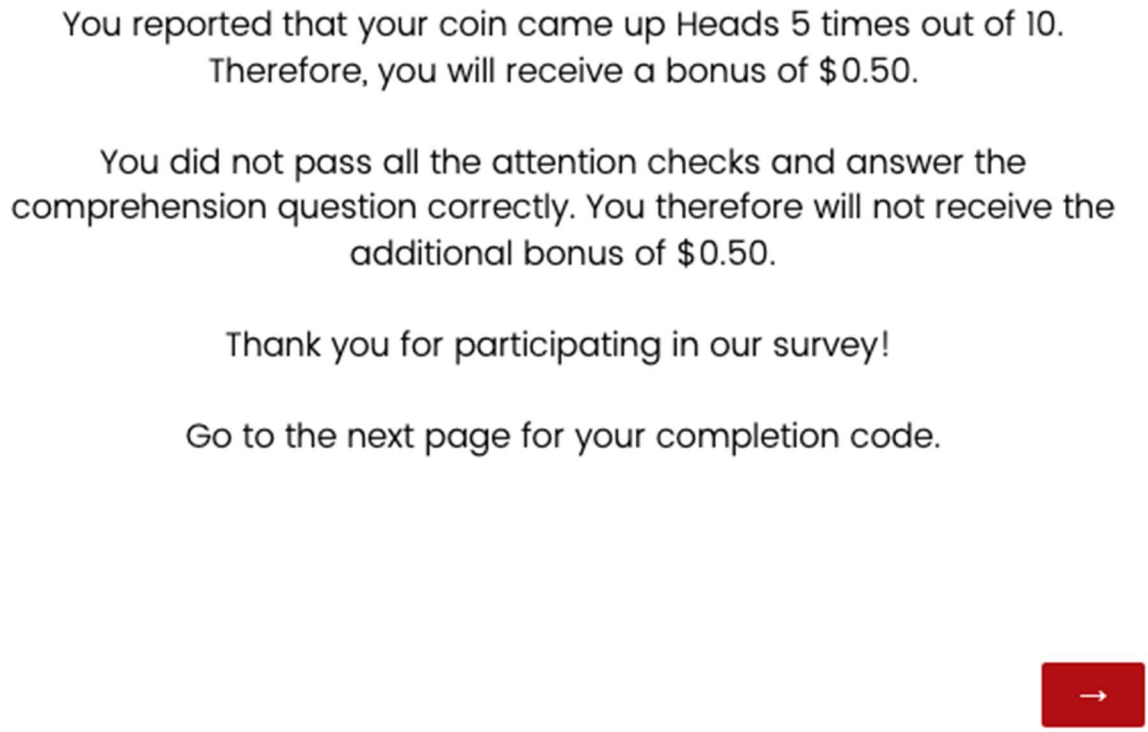
→

If they passed all the attention checks and answered the comprehension question correctly, they would see:



(In the screenshot, it says “You reported that yoru coin came up Heads X times out of 10. Therefore, you will receive a bonus of $0.10*X.” Where X=10. For each subject, X is replaced with their reported number of heads.)

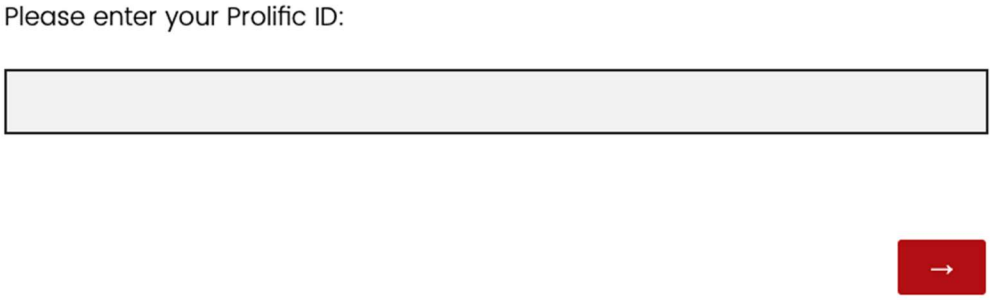
If they did not pass all the attention checks or answer the comprehension question correctly, they would see:



(In the screenshot, it says "You reported that yoru coin came up Heads X times out of 10. Therefore, you will receive a bonus of $0.10*X." Where X=5. For each subject, X is replaced with their reported number of heads.)

Second Component Survey:

For All Subjects:



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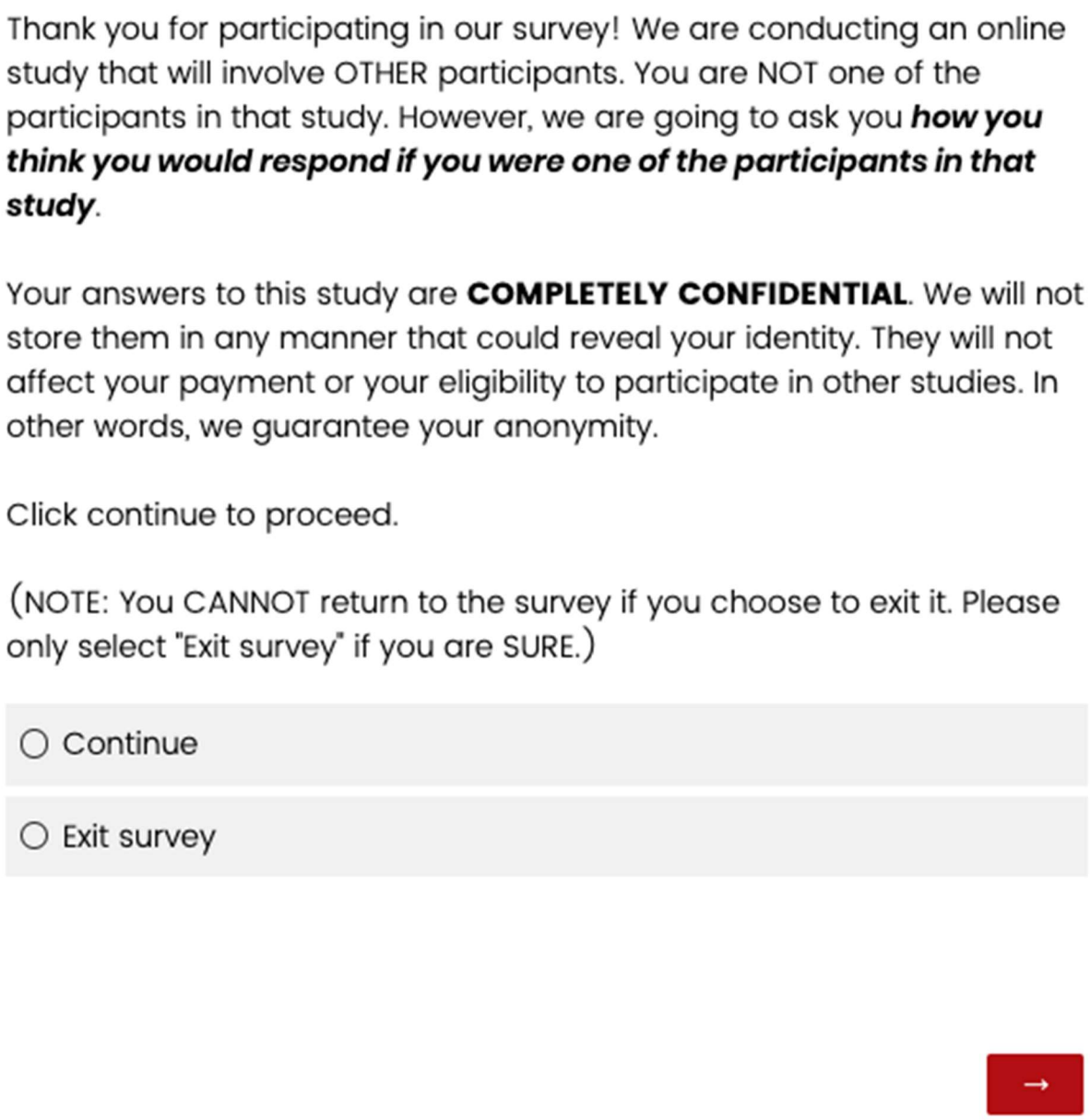
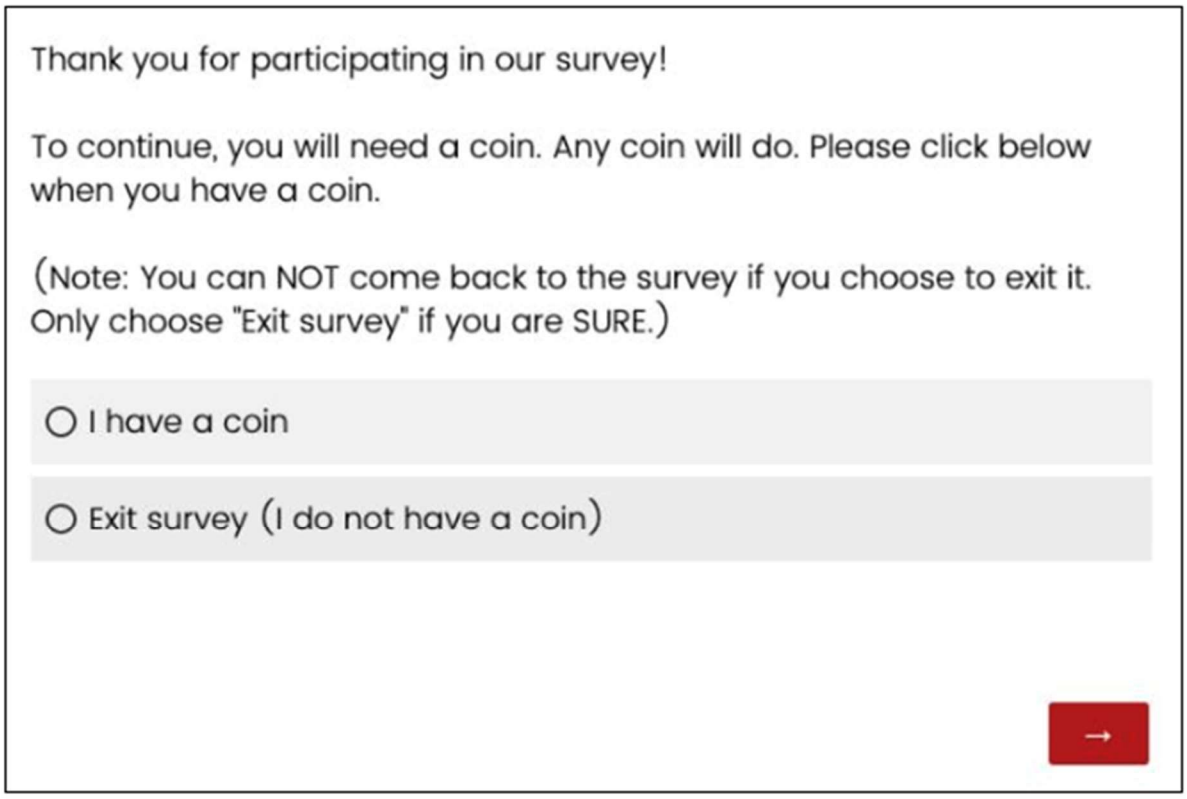


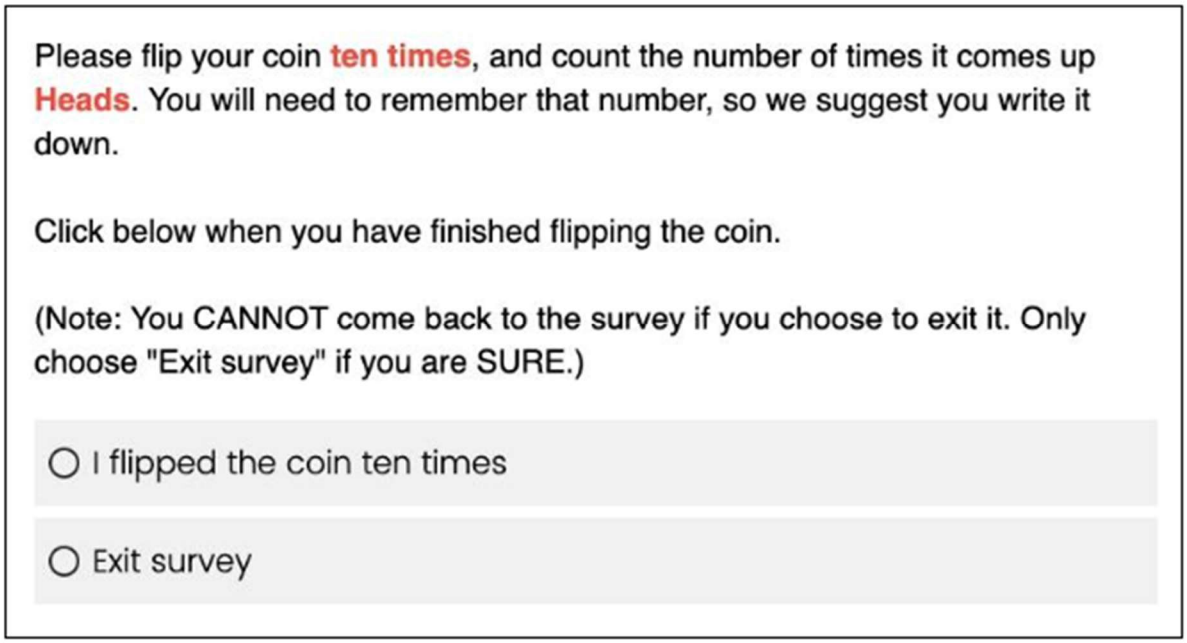
For those who selected “Continue”:

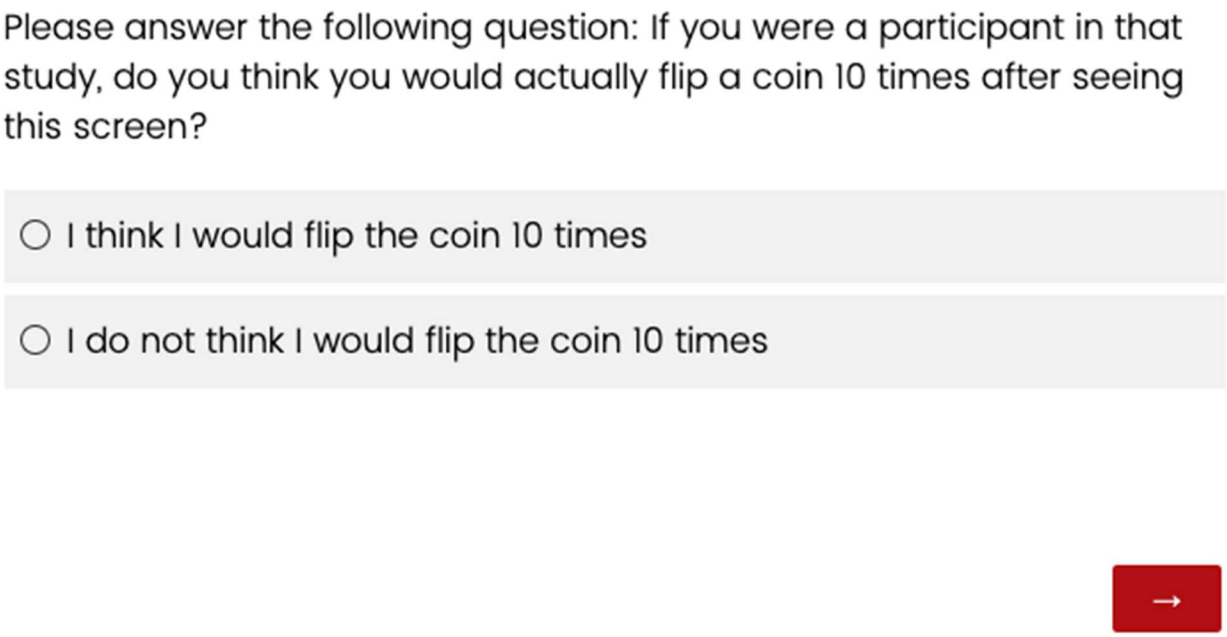
(Those who selected “Exit survey” leave the study at this point.)

When the participants start our online study, they will first see the following screen:



Once they click "I have a coin," they will see the next screen:





Regardless of whether they say they would flip the coin 10 times, on the next screen, they will see:

Next, you would see the following screen. Please read it carefully. To make sure you are paying attention, please click on the first word of this paragraph.

We would now like you to report the number of times the coin came up **Heads** out of the ten times you just flipped it.

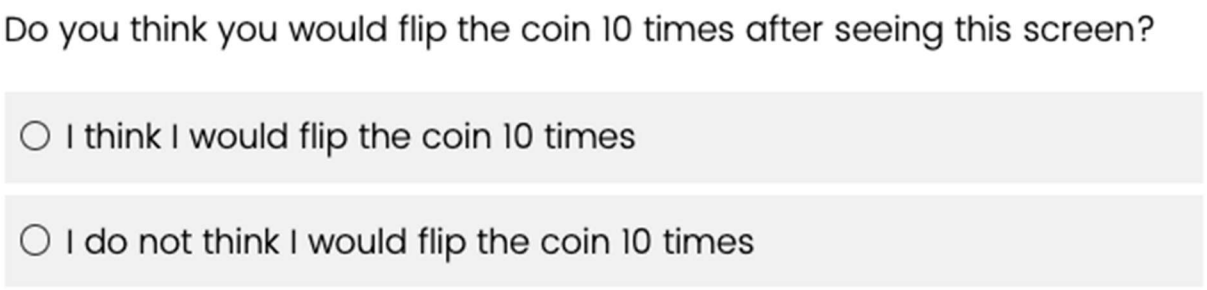
You will receive a bonus payment in this survey that depends on the number of times the coin came up **Heads**. The table below shows the size of your bonus.

Number of times the coin flip was **Heads**	Your bonus
0	0
1	$0.10
2	$0.20
3	$0.30
4	$0.40
5	$0.50
6	$0.60
7	$0.70
8	$0.80
9	$0.90
10	$1.00

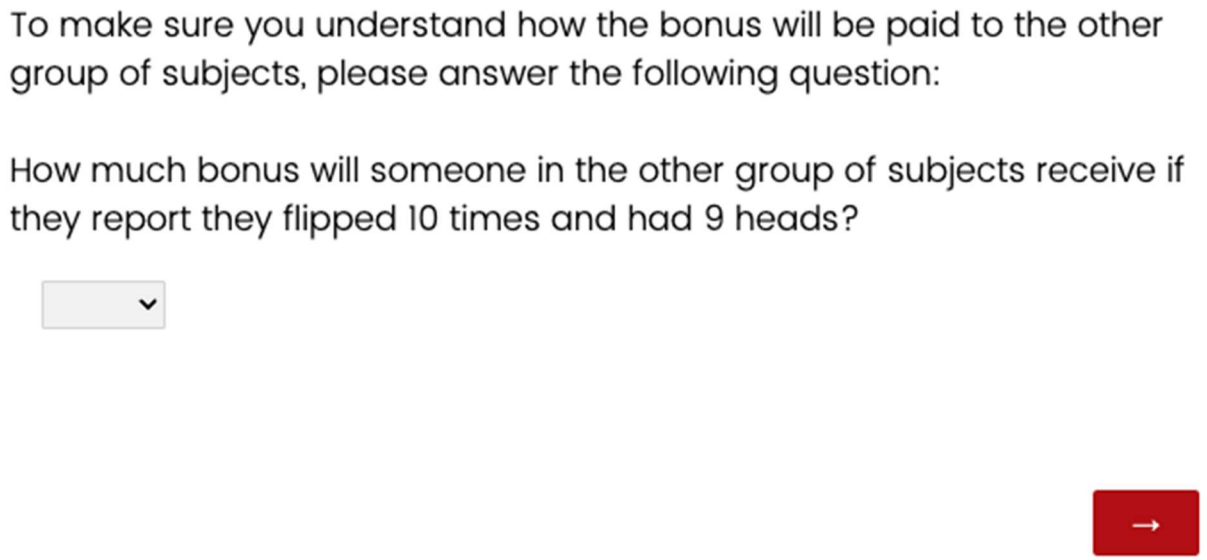
We would appreciate an honest answer.

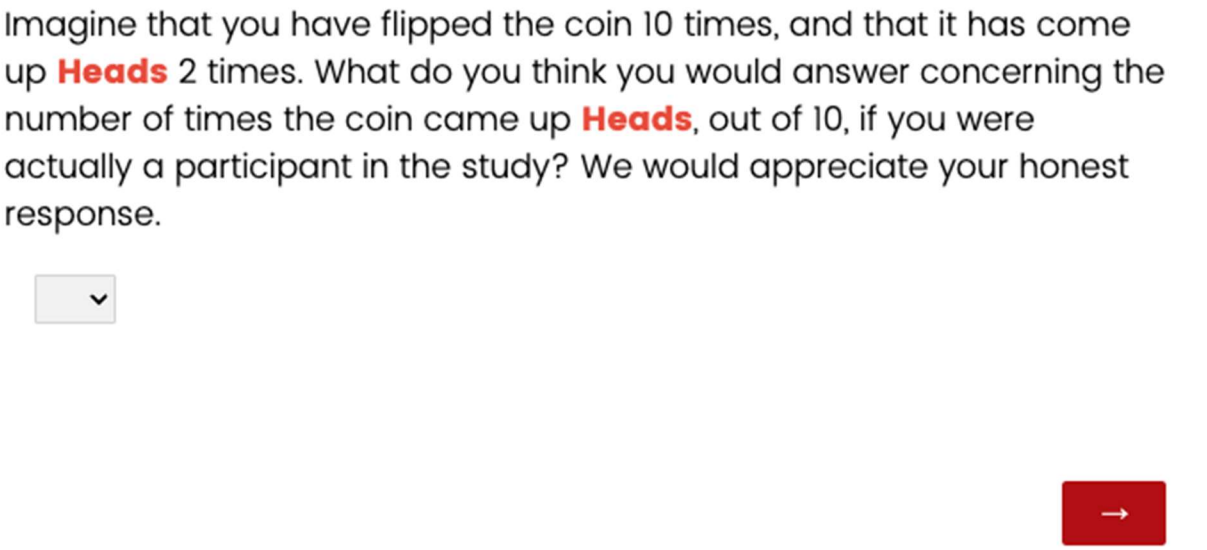
Number of times the coin came up **Heads**, out of 10:

If they say they would not flip the coin on the previous screen, here on this screen, they will be asked again whether they would flip a coin at this point:



All of them were also asked the comprehension check question on this screen:

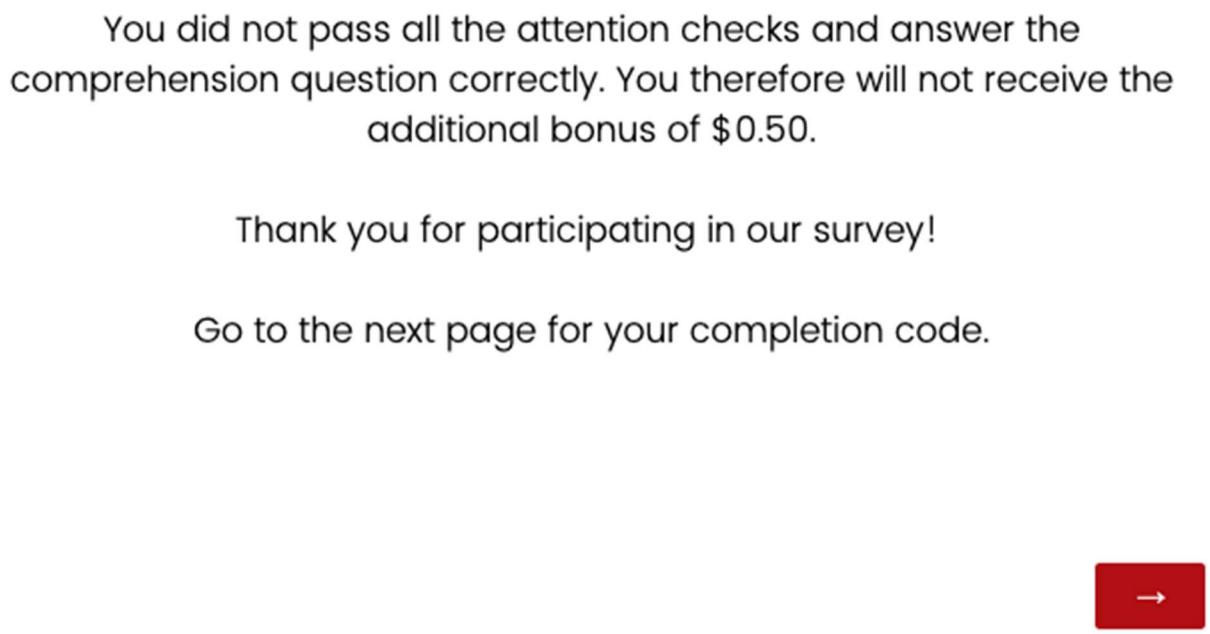




Note that in the question statement, it says "…and that it has come up Heads **X** times". In the screenshot,

X=2, but different subjects see different X's. X is randomly generated according to the true coin flipping population frequencies.

If they did not pass all the attention checks and answer the comprehension question correctly, they would see:



If they passed all the attention checks and answered the comprehension question correctly, they would see:

You passed all the attention checks and answered the comprehension question correctly. You therefore will receive the additional bonus of $0.50.

Thank you for participating in our survey!

Go to the next page for your completion code.

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