

Response to Reviewers' Comments on the Manuscript SORO-D-23-00204R1

Dear Reviewers and Editor,

We thank you for the opportunity to revise our manuscript and for the valuable feedback, which has significantly helped us improve the clarity, organization, and overall quality of our paper. Based on the concerns reflected in the comments, we would first like to offer a general response:

(1) Following the recommendations from Reviewer 2, we have refined the language and figures. Furthermore, we have rerun the analysis for section 2.4, ensuring that all significant findings are now fully detailed. In addition, we rerun the EDA section as suggested to subtract the baseline to take the individual differences into account.

(2) In response to the insights from Reviewer 4, we have enriched the manuscript with additional details regarding the experimental procedures and potential study limitations. Regarding the concerns raised about the analytical approach, there was an initial misnaming of the statistical test employed. It was, in fact, a two-way mixed ANOVA, not a repeated measures ANOVA as previously stated. We have amended this inaccuracy within the document and have revised the relevant sections of the analysis for enhanced clarity.

Response to Reviewer 1,

We are very grateful for your insightful input, which helped us improve our paper a lot, and we sincerely thank you for your time and effort on our paper.

Comment 1: I thank the authors for submitting this revision. I appreciated their responses to all my concerns. I was curious. In their responses they mentioned that the changes were highlighted in blue in the manuscript and I couldn't find these. Perhaps this was an issue on my end, but I checked all documents uploaded and couldn't locate it anywhere. Highlighting the changes made would have been very helpful.

Response 1: We apologize for not indicating the location of the highlighted revisions in the manuscript clearly, and we regret any inconvenience caused. Thank you for taking the time to review the manuscript without the highlighted changes. Please be aware that a hyperlink to the highlighted version can be found on the last page of the submission document. Additionally, for added assurance, we have uploaded the response letter and highlighted version of the manuscript to GitHub. You can access it using the link provided below:

Comment 2: In reference to response 10 - actually in the literature these are called "user-initiated touch" and "robot-initiated touch," there is a wonderful Charlie Kemp paper about this in a nursing context re: clinical vs. affective touch that may be of interest to the authors. I did appreciate the addition of the section on active and passive tactile interactions to explain the challenges. I also appreciated that in the discussion/limitations section the authors mentioned the feeling of responsibility when the robot was placed on their lap. I think

this is a real thing. In this condition because the robot was placed on their lap this is more robot-initiated touch, which often is associated with less enjoyment of social touch throughout the literature. In social interactions it is almost always preferred when the initiation comes from the user.

Response 2: Thank you for sharing your thoughts on this wonderful paper. It's intriguing to learn that participants tend to favour robot-initiated touch when it's seen as practical rather than emotional. This may reflect the common view of robots as tools or functional devices, rather than partners capable of providing emotional support. In our study, we also discovered that people place greater trust in a robot's performance than in its morale. Engaging in tactile or verbal interactions could enhance emotional communication between participants and robots. Additionally, the perceived social role of the robot could alter expectations, influencing whether participants see the robot more as a functional tool or as an emotionally supportive companion. It would be worthwhile to further investigate this by maintaining the same experimental setup but introducing interactions with participants before the experiment in the future. Thanks for your recommendation.

Comment 3: On page 3, I would appreciate if the authors could add a citation for the sentence "touch serves as our primary non-verbal means of expressing intimate emotions and is crucial for our physical and emotional well-being" in Section 1.3 Social nature of tactile interaction.

Response 3: Thanks for your suggestions; we have included the reference below in Section 1.3 Social nature of tactile interaction, on page 3.

[1] Van Erp, J.B., Toet, A.: Social touch in human–computer interaction. *Frontiers in digital humanities* 2, 2 (2015)

Comment 4: Also on page 3, but the second column, the last paragraph of section 1.3. I would appreciate if the authors could include the Section numbers which each description in the overview.

Response 4: Thanks for your suggestion; we have added the section numbers, specifically in Section 1.3 Social nature of tactile interaction, on page 3 of the manuscript.

Comment 5: Figure 1 on page 4. I believe the authors should swap sub figures a and b so the subfigures are shown in decreasing order of touch intensity. I also believe it would help the reader if sub-figure c's caption was renamed to match the condition naming convention in section 2.1.1 to be something more like "robot present no touch" (the condition is "no touch condition"). Right now the repetition of "no robot" between subfigure c. and d. is a little confusing.

Response 5: We appreciate your suggestions. Acknowledging the potential for reader confusion, we have rearranged the subfigures within Figure 1 as you recommended, ensuring they are presented in descending order of touch intensity. Additionally, we have updated the labels for all subfigures of Figure 1 at the top of page 4 to ensure consistency with the condition names in Section 2.1.1.

Comment 6: In section 2.1 when the authors mention "in the baseline condition" I think it would be great if they could add a reference to [Fig. 1.d] already so the reader understands, and later when they mention "in the presence of a robot" they can add a reference to [Fig.1.a-c].

Response 6: Thanks for your suggestion; we added the reference to the subfigures in section 2.1 on page 4 to make sure the readers can understand better, which is shown below:

"In the baseline condition, participants complete the BART task alone, as referred to Fig. 1.d. In the experimental condition, which is shown in Fig. 1.a-c"

Comment 7: Also in section 2.1 on the other column, the authors mention the participants have a tactile interaction "with varying intensity". I wonder if the authors could change this to be "with various intensity" or "with preset intensity" to indicate that the intensity of the interaction is not changing during each condition.

Response 7: Thanks for your suggestions. We changed the "with varying intensity" to "with the intensity of the tactical interaction depending on the condition" in Section 2.1 Methodology and materials on page 4 to make sure our statement is clear.

Comment 8: Finally in section 2.1 when the authors reference a "non-social device (plastic box)" please add a reference to [Fig. 2].

Response 8: Following your suggestion, we have included a reference to [Fig. 2] when discussing the "non-social device." The relevant reference appears below:

"while interacting with a non-social object: a plastic box having the same texture as the surface of a Nao robot, as shown in the Fig 2."

Comment 9: Figure 2: I am still struggling to see a difference between sub-figure a and sub-figure b. After quite a while looking at the images and reading through the paper, I don't believe there is supposed to be a difference between these images. Therefore, may I make two suggestions. Either: combine sub-figure a and b into one image and call it "affective and neutral tactile interaction with the box" OR: add speech bubbles or a message on the computer screen to sub-figure a and b that hint at the difference (i.e. for affective it would say

"don't worry, we'll do better next time" and for neutral "can you touch the box"). I think this will help clarify the difference.

Response 9: Thanks for your suggestions; it is indeed helpful; we add speech bubbles on the computer screen to the subfigures of Figure 2 on page 5 to make sure the readers can hint at the difference easily.

Comment 10: Also Figure 2 - caption. The first parenthesis for Figure (a) is facing the wrong way (Figure)a). There is also some awkward wording in sub-caption a and b "slightly intensity... tactile interaction". I don't think the wording is correct. Perhaps the authors meant "slightly intense affective/neutral tactile interaction" or "slight intensity affective/neutral tactile interaction"? However, I believe this caption would make more sense without the phrase entirely just "affective tactile interaction" and "neutral tactile interaction" as these are the phrases used in the paper.

Response 10: Thanks for the suggestion; we removed all the 'slightly' in the caption, and now the caption is: a) Affective tactile interaction with box, b) Neutral tactile interaction with the box, c) No tactile interaction

Comment 11: In section 2.2. the authors mention 38 participants in Study 1 and 36 participants in study 2, I was curious if the authors kept track or asked if any of the participants from Study 1 also participated in Study 2, if so, how many? Or if they were all new participants? I think this would be important information to mention in the participants section. For example, if any participants had already experienced the first study, they may have some insight into the researcher's questions and could be subject to the demand effect.

Response 11: Thanks for your suggestions; they were all new participants and ran at different times; the human-robot interaction experiment is around three months earlier than the human-object experiment. We mentioned that the participants for the two experiments are from different participant pools under Section 2.2 Participants on page 5.

Comment 12: In section 2.3 I was curious why the authors mentioned that they only had participants fill out the MDMT v2 after the first robot interaction, but unfortunately I didn't find out the answer until section 2.6.5. I think it would be great if the authors could move this information earlier to when the question first appears in the reader's head.

Response 12: Thanks for your suggestions. We moved this information to section 2.3 Procedure on page 6 to avoid confusion for the readers.

Comment 13: In section 2.6.3.1, I would appreciate if the authors could kindly define any acronyms the first time they appear before using them. SNS is never defined (sympathetic

nervous system) and PNS (parasympathetic nervous system) is only defined the second time it appears in relation to the PNS index.

Response 13: We appreciate your suggestions. We defined the SNS and PNS in section 2.6.3.1 Heart rate variability on page 8.

Comment 14: On page 9, Section 2.6.6. Relationship between trust, risk-taking behavior, and arousal: I am concerned the authors do not fully understand the literature they are citing. In reading the sentence, "perceived trust is related to perceived risk" I thought about this and was concerned, because I believe they are inversely related. I then went and read the paper the authors cited [61], and indeed, I believe I am correct. On page 9 of the cited paper, under subsection "Subjective trust and perceived risk as probabilities" the authors mention that subjective trust is basically the probability of receiving a positive outcome. Conversely, the perceived risk is the probability of receiving an unfavorable outcome. Therefore, I recommend the authors rephrase that sentence to say "inversely related." My second issue with this sentence is that the second half of the sentence was copied word-for-word from the abstract of the paper cited, but was not put in quotes "so the causal relationship between subjective and behavioral trust is similar to perceived risk and risk-taking." Please either include this sentence in quotations to show that it is not the authors' of this manuscripts own words, or rewrite in your own words.

Response 14: Thank you for highlighting this issue. We have revised the sentence and rephrased the latter half as indicated below in the Section 2.6.6 Relationship between trust, risk-taking behaviour and arousal on page 9:

"Perceived trust is inversely related to perceived risk, and behavioural trust is related to risk-taking, The relationship between subjective trust and behavioural trust parallels the relationship between perceived risk and the risk-taking behaviours."

Comment 15: In section 3.1, on page 10 under header "Participants' perception of Nao's emotional state" all the 1st quotations in the section, related to 'wants to be touched' are going the wrong direction.

Response 15: We appreciate your comment. We corrected the direction of the first quotations related to 'wants to be touched' in Section 3.1 Perception of the interaction of Nao on page 10.

Comment 16: Section 3.2.2 and Figure 6, the authors in several places refer to "explosion times," but as a reader, this sounds like the duration of an explosion, whereas the section title more clearly describes what the authors mean "number of explosions." Therefore, for clarity, I would appreciate if the authors could replace any instance of "explosion times" with "number of explosions."

Response 16: Thanks for your suggestions; we replaced all instances of “explosion times” with “number of explosions” by highlighting them in section 3.2.2 Number of Explosions for the convenience of review on page 11.

Comment 17: It appears to me that the authors have not referenced Figures 7 or 8. It is important that every figure that is included in a manuscript is referenced somewhere in the text of that manuscript. Figure 7 should be referenced in section 3.2.3 Number of pumps. Additionally, the authors describe posthoc analyses showing 4 significant differences (LI sig higher than NT, HI sig higher than NT, LI sig higher than NR, and HI sig higher than NR), but Figure 7 only shows one significance bar - which one is correct? Finally, I found the phrasing in the text to be confusing as the directions were reported differently within the same sentence, please report consistently (rather than a sig. Decrease from here to here and then these sig. more than those). Please add a reference to Figure 8 in Section 3.2.4 profits.

Response 17: We appreciate your suggestion. To address your concern, we first add the reference to Figure 7 in Section 3.2.3 Number of pumps and reference to Figure 9 in Section 3.2.4 Profits. Secondly, we corrected the significance bars in Figure 7. Thirdly, we rephrase the sentence to make sure that the reported direction is consistent in the same sentence in section 3.2.3 Number of pumps on page 11.

Comment 18: In section 3.4 Gender differences: The "w" in "when examining gender as a main effect" begins the sentence but it is not capitalized. It should be.

Response 18: Thank you for bringing this to our attention. We have capitalized the "W" in "when examining gender as a main effect" in Section 3.4 on page 13.

Comment 19: Also in section 3.4 on page 12 the authors mention "less fluctuation in pressure across four conditions" and later at the end of the section on page 14 mention "experienced lower pressure levels." I am concerned the authors are not fully aware of what they are measuring. HRV measures the fluctuation in time between heartbeats, it does not measure any sort of pressure. When the high frequency component of HRV is decreased, it can be due to a time pressure, an emotional strain, or some anxiety. Furthermore, the higher aroused we are (e.g., nervous or with exercise), the intervals between our successive heartbeats become more regular (often, but not always) because our heart beats faster (they are often, but not always, inversely related to each other), so HRV will decrease with feeling an increase in pressure, anxiety, or emotional strain. Conversely, if someone feels relaxed or is resting, they may have a lot of variability in their heartbeats and thus will have high HRV. I would appreciate if the authors could kindly improve their wording in these two locations because the "pressure" they are referring to is unclear.

Response 19: Thanks for your suggestions; we have removed the sentence regarding "less fluctuation in pressure across four conditions" from page 12. Additionally, we have

revised the wording on page 14. The sentence now reads: "Females exhibited lower HRV levels, potentially indicating higher emotional strain, stress, or anxiety among participants."

Comment 20: Section 3.5. EDA I am curious why the authors did not provide a figure for the EDA data. I think this would have been very helpful. Especially because there were significant differences to be seen, and they chose to include figures with non-significant differences, I think this would have been great to see. I was also curious if the authors normalized the EDA data before performing their analyses? Perhaps I missed it in my reading, but I don't remember reading about subtracting a baseline recording of each person's EDA to account for individuals being generally sweatier than others?

Response 20: We are grateful for your feedback. Initially, we did not normalize the EDA data before the analysis because we conducted measurements for participants across all conditions, aiming to discern differences between these conditions. Additionally, we initiated the experiments after a 15-minute period to ensure participants started from a baseline, thereby minimizing potential influences. But overlooking the possibility that individual baseline differences could affect the results. In response, we normalized the EDA data by subtracting each participant's baseline EDA recording; we rerun the analysis in Section 3.5 on Electrodermal Activity, allowing us to account for individual differences. Furthermore, we've included a plot of the related-samples Friedman's Two-way Analysis of Variance by Ranks to illustrate our findings as you suggested.

Comment 21: In section 3.6 I had a lot of questions regarding the statistical analyses conducted. First, I was curious why the authors conducted a paired t-test only for the Benevolent subscale? In looking at Figure 12, I wonder if the authors also conducted a paired t-test for the other subscales and are only reporting the significant result they found? Did they also conduct a paired t-test for any of the comparisons on Figures 11, 13, or 15? If so, I didn't read about the authors using an alpha correction to account for the multiple comparisons. I will assume they conducted their analyses correctly, but I wanted to mention this because it wasn't very clear why it was presented this way.

My second question was with the authors conducting a one-way ANOVA on the scores of each subscale of the MDMT v2 for 3 experimental conditions at 2 different time points. To me, this does not sound like a one-way ANOVA. I believe conducting the three experimental conditions at a single time point and evaluating the subscales would be a one-way ANOVA. However, the fact that the authors are taking multiple time points into account, I believe this makes this a repeated measures ANOVA problem because you are resampling from the same participant pool. In this case, the time factor will only have 2 levels, and the experimental condition factor has 3 levels. But I still believe this would need repeated measures.

My last question related to this section, was why the authors seemed to have duplicate analyses for the Benevolent subscale? The paired t-test seems to be conducted just on the LI condition. However, the ANOVA should be taking into account all the experimental

conditions. The ANOVA with post hoc multiple comparisons is typically preferred over a t-test because it provides a more comprehensive analysis and better controls for Type I errors when dealing with multiple comparisons.

Response 23: Thank you for your valuable suggestions. We concur that a two-way mixed ANOVA is the appropriate analysis method, considering the time factor will have two levels, serving as the within-subjects factor. At each time point, we assess the outcomes across the five trust subscales. Moreover, given that all participants were measured for trust only after the first condition, we treat the experimental condition as a between-subjects factor for the trust measurement. Consequently, we have applied a two-way mixed ANOVA for our analysis and used the Bonferroni correction for all necessary pairwise comparisons. In response to your feedback, we have revised Section 3.6 to accurately report all significant findings discovered, as well as to include significant outcomes across different subscales—a component previously unexplored in our analysis.

Comment 24: On page 15, on the right hand column, there appears to be a very similar sentence written three times in a row. It says something to the effect of "negative attitudes towards robots leads to lower trust in robots" in a variety of ways. I think writing this sentence only once would be sufficient.

Response 24: Thanks for your suggestions; we deleted the similar sentences on page 16.

Comment 25: On page 16, the authors report positive correlations of $r = 0.52$, $p < 0.001$, but I can't seem to find this anywhere. It's not in Table 7 or 8. Can the authors kindly point me to where this number came from? Did they actual experience or have an instance of this in their experiment? If so, please include this data in the manuscript.

Response 25: Thanks for your suggestions, the correlation analysis is for the overall trust after the first interaction and the overall BART scores, but we only measured the trust before and after the first interaction. Therefore, we deleted this statement.

Comment 26: Section 4.1 BART performance, I also had concerns regarding the statistical analyses. The authors reported that there were no significant differences found in the BART scores among the three conditions, and then proceeded to perform and report post hoc tests. Post hoc tests are only necessary if a significant difference is found. If there is not one found in the main ANOVA, there will never be significant differences found in post hoc tests. I am slightly concerned the authors are not thinking critically about the analyses they are conducting.

Response 26: Thanks for your suggestions; we agree that the post-hoc test is not necessary if there is no significant difference in the main effect. Therefore, we deleted the post-hoc analysis part in section 4.1 BART performance.

Comment 27: In section 4.2 HRV the authors ran a repeated measures ANOVA, I was curious how many levels they used for time here? Did they use the same 2 time periods they have been using for the other analyses (T0 and T1)? I assume they were recording HRV continuously, so how did the authors bracket or segment HRV into sections and how many did they use for their analyses? I believe this information should be provided in the manuscript to the reader.

Response 27: Thanks for your suggestions. In section 4.2 HRV, we used three levels of HRV as there are three interaction periods; we recorded the HRV continuously for each interaction period. This study lasted an average of 45 minutes. Specifically, AT, NAT, and NT conditions lasted on average, 9 minutes, 9 minutes, and 6 minutes, respectively. Participants saw all three conditions in a random balanced order. IBI and EDA measurements were continuously recorded during the BART task in periods P1, P2, and P3, and the baseline of the IBI and EDA data was obtained at P0. There was a one-minute break between conditions. The first two minutes of physiological data in the P1, P2, and P3 were not included in the analysis to prevent the impact of the previous interaction. As for T0 and T1, which refer to the end of the interaction in baseline and first interaction. We asked the participants to fill out the trust questionnaire at these two time points in the first study, we did not ask the participants to fill out the trust questionnaire in the second study human-object study. For the Bart game, HRV, and EDA measurement, we used the same period for analysis. Trust measurements are obtained at T0 and T1 in the first study. To address your concern, we clarified these details in Section 2.3 Procedure on page 6.

Comment 28: Also in this section 4.2 there is a repeat sentence "However, the outcomes did not reveal any significant differences among the three conditions." Appears twice in a row. Please remove one.

Response 28: Thanks; we removed the repeat sentence in section 4.2 on page 17.

Comment 29: Section 5.1.1 "In contrast, interaction with the robot without touching the robot" I think would be clearer if it was written as "in contrast, interaction without touching the robot" - the repetition of the word "robot" was confusing.

Response 29: We rewrite it to "in contrast, interaction without touching the robot" in section 5.1.1 Risk-taking behaviour on page 18.

Comment 30: Citation #65 does not look complete or correct. There are three question marks, which the authors were probably using as placeholders. Please find the correct information and fill it in.

Response 30: We corrected the citation 65.

Response to Reviewer 2,

We appreciate your insightful input, which helped us to make our paper better, we addressed your concerns below one by one.

Comment 1: The paper lacked a clear description about the actual study procedure. The authors describe they measured IBI and EDA at time points T1, T2, T3 and T4, but never explained when those time points are. Besides the overall time, there was no indication on how long each condition were? Similarly, there were no mention of familiarization or breaks between conditions.

Response 1: Thanks for your suggestion, for human-robot interaction experiment, we incorporated the periods for IBI and EDA measurement, the breaks between conditions and overall time consumption for conditions in Section 2.3 Procedure on page 6.

For Human-robot interaction:

Measurements of IBI and EDA were sampled continuously during periods P1, P2, P3, and P4, the BART task's four conditions. Baseline IBI and EDA data are obtained at P0, before the task start. The time points T0, T1, T2, T3, and T4 refer to the end of the interaction in the baseline, first, second, third, and fourth periods, respectively. During P0 we ask participants to sit quietly for 15 minutes to obtain a physiological baseline measurement. After the first interaction, the participants complete the above-mentioned questionnaire, which takes 5 to 12 minutes to finish. There is one minute of breaks between the conditions. The first two minutes of physiological data in the P1, P2, P3, and P4 periods were not included in the analysis to prevent the previous interaction influencing the current one. The entire experiment lasted on average 1 hour and 10 minutes. Specifically, LI, HI, NR, and NT conditions lasted on average around 15 minutes, 10 minutes, 6 minutes, and 8 minutes, respectively.

For Human-object interaction:

This study lasted an average of 45 minutes. Specifically, AT, NAT, and NT conditions lasted on average, 9 minutes, 9 minutes, and 6 minutes, respectively. Participants saw all three conditions in a random balanced order. IBI and EDA measurements were continuously recorded during the BART task in periods P1, P2, and P3, and the baseline of the IBI and EDA data was obtained at P0. There was a one-minute break between conditions. As before, the first two minutes of physiological data in the P1, P2, and P3 were not included in the analysis to prevent the impact of the previous interaction.

Comment 2: It was unclear how the study and incentives were presented to the participants. In 2.2, the authors mention participants were paid 5 Euro for participating in the study. For BART to work, participants should believe their payment is tied to their performance for them to have skin in the game. The authors should clarify whether this was a deception study and what participants were told.

Response 2: It is not a deception study. We informed the participants that their goal was to achieve higher profits during the experiments, and then we rewarded them with 5 euros as

participation compensation. Participants were engaged in the experiment who were aware their scores were being recorded and aimed to outperform their peers. In fact, some participants inquired about their standings relative to others post-experiment. However, we recognize that participant engagement might have been further enhanced by offering real monetary rewards. Therefore, we have noted this consideration as a limitation in our study, which has been noted in Section 5.2 Limitation on page 20.

Comment 3: In 3.4 where authors attempt to measure the effect of gender, the authors say they used a two-way repeated measure ANOVA. This is the incorrect test since gender is a between-subject factor. A mixed ANOVA or other mixed methods analysis should be used instead. It was unclear if the authors just misnamed the test, or the wrong test was conducted. If the wrong test was run, it is unclear if the results will be the same. The authors should clarify this in future revisions.

Response 3: Thanks for pointing it out. We misnamed the test, and we indeed used a two-way mixed ANOVA for the analysis. We fixed the misnaming issue and rephrased Section 3.4 Gender differences in risk-taking performance and stress level during BART game on page 12 to make it more clear.

Comment 4: I'm concern about whether the results were caused by only the physical interaction manipulation. I wonder whether the differences between the physical interaction conditions and the verbal condition was that the robots in the physical interaction conditions were successful in getting the person's attraction. This probably can be address by giving more details about how the robot gave verbal encouragements.

Response 4: Verbal encouragement was paired with autonomous gestures, and additionally, whenever the participant's pump surpassed certain thresholds or wished to stop the pump before 50 pumps, pressing the "next balloon" button or when the balloon exploded, initiated an immediate response. At this point, the robot began to offer verbal encouragement along with gestures. After that, if the participants pressed the button again, it would move to the next balloon. In addition, if the balloon explodes, the robot will also give verbal encouragement. We believe that even in conditions only with verbal interaction, the robot still captures participants' attention. However, we observed that the participants will look at the robot more often during the tactile interaction. We acknowledge that the robot likely garners more interest from participants during tactile interactions. It will be interesting to compare to a scenario solely featuring tactile interaction without verbal encouragement. This juxtaposition might highlight the synergistic effect observed when verbal and tactile cues are combined. We have also acknowledged this aspect as a limitation in our study, suggesting that the integration of both verbal and tactile interactions may play a crucial role in the robot's ability to engage participants. To address your concern, we incorporate this in the Section 5.2.4 Active and passive tactile interaction and we give more details about how the verbal encouragement are given in Section 2.1.1 Study 1: Human-robot interaction.

Comment 5: I was curious why the second study did not completely mirror the study and have the box on the person's lap.

Response 5: We focused on replicating the low-intensity (LI) condition in the first study for the second study, as we observed that in the condition leads to a decrease in stress. In the LI condition, there were affective sentences and tactile interactions presented together. Our research question was piqued by whether there would be a discernible difference when combining an affective sentence with physical interaction versus a neutral sentence with physical interaction. Interestingly, no significant difference was found between these approaches, which might be attributed to the challenge of attributing social capabilities to an inanimate object like a box. It would be worthwhile to conduct a future study replicating this setup—neutral verbal communication paired with tactile interaction—using a robot instead. Additionally, exploring the dynamics of having the box placed on a participant's lap, given its lightweight nature and the reassurance that participants need not worry about its presence, could yield different results. These considerations and potential areas of further research have been duly noted in our study's limitations, located in Section 5.2.5 Human-robot interaction scenarios on page 21.

Comment 6: I found this paper to be more about persuasion than risk-taking, in some ways, the paper looks at ways for the robot to be more persuasive. I think a discussion about the differences will be interesting.

Response 6: Thank you for your suggestions. Indeed, the process appears more akin to persuasion, with risk-taking behaviour emerging due to the robot's persuasive influence. We added one subsection under section limitation to explore the correlation between persuasion and risk-taking behaviour. In addition, the persuasion effect might also stem from the tactile interaction with the robot, making participants feel a closer connection, thereby enhancing the persuasive impact and leading to increased risk-taking behaviour. We've highlighted this observation in Section 5.2.6 Persuasion and risk-taking behaviour, suggesting that the physical engagement with the robot potentially fosters a sense of closeness, which in turn, amplifies its persuasive capacity.