

Close relationship partners of impartial altruists do not report diminished relationship quality and are similarly altruistic

Corresponding Author: Ms Paige Amormino

This file contains all editorial decision letters in order by version, followed by all author rebuttals in order by version.

Version 0:

Decision Letter:

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Dear Ms Amormino,

Thank you for submitting your manuscript titled "Does impartial altruism incur costs in close relationships?" to Communications Psychology. We have given the paper our careful consideration and find it of potential interest. However, due to certain shortcomings we are concerned that sending the current manuscript out to review could lead to unnecessary delays and quite possibly an undesirable outcome of the review process.

In particular,

1. Please ensure you follow our statistical guidelines when reporting statistics (<https://www.nature.com/commpsychol/submit/submission-guidelines#statistical-guidelines>). Please note in particular our requirements for the reporting and interpretation of null-results. Non-significant findings derived from null-hypotheses significance tests should be reported in full, but may not be interpreted. Where you interpret null results, this interpretation must be based on Bayes Factors or equivalence tests. Please add Bayes Factors or equivalence tests in regards to your null findings on favoritism.

2. We noticed that Supplemental Table 8 appears to be a copy of Supplemental Table 9. Please check and fix any reporting errors.

We would therefore like to invite you to revise your manuscript to address these concerns before we make a final determination on whether to send your manuscript for external review.

We shall hope to receive your revised version as soon as you are able to complete the suggested revisions. If something similar is published in the interim we will have to consider the impact it has on the novelty of a revised manuscript.

If you anticipate a delay of more than four weeks, please let us know. Should your manuscript be substantially delayed without notifying us in advance and your article is eventually published, the received date may be that of the revised, not the original, version.

We also ask that you ensure your manuscript complies with our editorial policies and reporting requirements.

To that end, we require revised manuscripts to be accompanied by two completed items: a reporting summary that collects information on study design and procedure, and an editorial policy checklist that verifies compliance with all required editorial policies.

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- <https://www.nature.com/documents/nr-reporting-summary.zip> p">Nature Research Reporting Summary

- <https://www.nature.com/documents/nr-editorial-policy-checklist.pdf>">Editorial Policy Checklist

All points on the policy checklist must be addressed. Your revised manuscript can only be sent to referees if these checklists

are completed and uploaded with the revision.

If you are not interested in submitting a suitably revised manuscript in the future please let me know immediately so we can close your file. If you have any questions, please contact me.

Please use the link below when you are prepared to resubmit.

Link Redacted

Thank you for your interest in Communications Psychology.

Best regards,

Jennifer Bellingtier

on behalf of

Mael Lebreton, PhD
Editorial Board Member
Communications Psychology
orcid.org/0000-0002-2071-4890

Version 1:

Decision Letter:

**** Please ensure you delete the link to your author homepage in this e-mail if you wish to forward it to your coauthors ****

Dear Ms Amormino,

Thank you for your patience during the peer-review process. Your manuscript titled "Does impartial altruism incur costs in close relationships?" has now been seen by 2 reviewers (numbered #2 and #3), and I include their comments at the end of this message. They find your work of interest but raised some important points. We are interested in the possibility of publishing your study in Communications Psychology, but would like to consider your responses to these concerns and assess a revised manuscript before we make a final decision on publication.

We therefore invite you to revise and resubmit your manuscript, along with a point-by-point response to the reviewers. Please highlight all changes in the manuscript text file.

Editorially, we recognize that both reviewers seem generally enthusiastic about your study, but nonetheless make sensible suggestions that should be taken into account in your revision. Most importantly, we want to raise your attention to the fact that several of your key results (notably the claim that there is no relationship between impartial altruism and social relationship quality) rely on accepting the null hypothesis. For manuscripts where the key results are null results, we require the following:

- Evidence that the study is sufficiently powered to detect the smallest theoretically or pragmatically meaningful effect. - Bayes Factors or equivalence tests to interpret the null results.

Please note that the journal guidelines on the requirement for the use, interpretation, and reporting of statistics are detailed in the attached checklist and on our webpage <https://www.nature.com/commpsychol/submit/submission-guidelines#statistical-guidelines>.

We are committed to providing a fair and constructive peer-review process. Please don't hesitate to contact us if you wish to discuss the revision in more detail.

I am attaching an Editorial Requests Table that details critical reporting requirements for the revised manuscript. Please attend to each item and ensure your manuscript is fully compliant. We are requesting that your manuscript aligns with these requirements as this facilitates the evaluation of your manuscript, reducing delays in re-review and potential future acceptance. If your revised manuscript is not aligned with these requests on major issues, such as those concerning statistics, it may be returned to you for further revisions without re-review. Additional information can be found in our style and formatting guide Communications Psychology formatting guide.

Please use the following link to submit your

- revised manuscript,
- point-by-point response to the referees' comments,
- cover letter (as a separate document),
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- the Reporting Summary (see below), and
- the completed Editorial Request Table (attached):

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We hope to receive your revised paper within 8 weeks; please let us know if you aren't able to submit it within this time so that we can discuss how best to proceed. If we don't hear from you, and the revision process takes significantly longer, we may close your file. In this event, we will still be happy to reconsider your paper at a later date, provided it still presents a significant contribution to the literature at that stage.

We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning.

We look forward to seeing the revised manuscript and thank you for the opportunity to review your work.

Best regards,

Mael Lebreton

Mael Lebreton, PhD
Editorial Board Member
Communications Psychology
orcid.org/0000-0002-2071-4890

REVIEWER REPORTS:

Reviewer #2 (Remarks to the Author):

The present manuscript investigated people's altruistic behavior on a classic social discounting task and also a novel third-party social discounting task. The researchers recruited kidney donors and their close others in addition to demographically similar controls and their close others. The paper found that generally altruists were more altruistic in both a classic social discounting task and also the third-party one. The researchers also found that moral beliefs did not predict altruism and also that altruism did not negatively impact close social relationships. Finally, the authors also found that close others of altruists were more impartial themselves.

Generally speaking, I very much enjoyed reading this article and want to commend the authors for a well-written manuscript that straightforwardly addresses a relevant theoretical and practical question. I also found the sample population especially interesting. There were a few small comments that arose while I read the manuscript, which I outline below in no specific order.

1. At first, I found the use of the $N = 2, 10$, etc. confusing starting around line 172. The reason being is that N typically refers to sample sizes, and N had not been properly explained in the text prior to line 172 (my apologies if I missed it!). I recognize that N is defined in the Method, but someone skimming the article may find the results confusing, nonetheless.
2. Figure 2 could be clearer in terms of what the solid and dashed lines mean. I realize this information is in the note, but maybe the difference could be indicated in the legend somehow. Same with Figure 3. I think all that really matters is that the colors are different, but I was searching to see if the dashed versus solid lines meant anything, but I don't think they do.
3. I think it's interesting that impartiality doesn't seem to affect social relationships. I did wonder, though, because this is a null effect whether the authors could remark on power to detect this particular effect. The sample sizes are a bit small, which makes sense because the study is recruiting a very specific population. Nonetheless, it would be perhaps beneficial to acknowledge this and provide some context for whether this effect. A power analysis is mentioned on line 494, but I am sort of confused regarding what effect the study was powered to detect.
4. Throughout the manuscript, I was thinking about the limitation raised by the authors on line 459 regarding a demand effect. I appreciate that the authors raised this concern and also appreciate their refutation of this point. Unfortunately, I still have lingering concerns. Is there anything in the researchers' data specifically that could be used to argue that there isn't a demand going on here? For example, perhaps that altruists' moral beliefs do not predict their altruism speaks to this point that they aren't replying in an especially socially desirable way. I also think that line 462 may have a typo.

Reviewer #3 (Remarks to the Author):

This is an impressive paper. Thank you for the opportunity to review the work. I appreciate the authors' theoretical and methodological rigor in presenting evidence that impartial altruists do not suffer relational costs. I include suggestions below

on how the authors can strengthen the manuscript. Beyond these modifications, I recommend the paper for publication.

(1) On Line 121, the authors describe their method as “compar[ing] altruists and their close others... as they completed a standard social discounting task.” While this is true, the authors also deployed a novel social discounting task. I would make this distinction clearer throughout the paper (e.g., terming one as “standard social discounting” or “first person social discounting” — as they do on Line 165 — and the other as “third-party social discounting” or “novel social discounting”). Beyond consistent terminology, I also would suggest (for example, on Line 100) that the authors describe the method as including both of these tasks — and further emphasize the benefits of the novel task, in addition to the standard paradigm. One of the strengths of the methodology is that the researchers implemented the standard and improved paradigm, finding converging results across both.

(2) On Line 157, the authors mention two alternative hypotheses. The authors should motivate each of these to some extent, prior to transitioning into the results. Further, I found myself wanting deeper discussion of why consensus bias would predict “first-person social discounting to predict allocation... in the third-party task” (Line 337).

(3) I found myself wanting to know more about the recruitment of the altruistic sample. The authors mention a “database of verified altruistic kidney donors,” but I still wonder about selection bias. Could there be any differences, even within this database, concerning which people would be most likely to participate?

(4) Income predicted altruism across many of the results reported in the paper (e.g., Line 217, 235, 308, 314), but the authors do not discuss this finding at all within the manuscript. I’d recommend that they say something about why this might be the case in their discussion of the findings.

(5) Formatting error: No closed parenthetical on Line 199.

(6) One methodological limitation that the authors do not address is whether the “social distance” scale actually matches social closeness across participants. For example, if I set my friend John as $N = 20$, how do we know that this matches the closeness for $N = 20$ across all other participants? I would like to know more about whether the authors did anything to validate this metric, as it is the cornerstone of the paper.

(7) On Line 558, the authors begin a description of the payout scheme for participants. I found the lack of deception (and ties to actual social networks) to be compelling, and I would recommend that the authors further emphasize the ecological validity of both of these aspects of their paradigm in the primary text. This is a tremendous improvement over, for example, self-report (e.g., “Imagine making an allocation between a friend vs. a stranger”).

(8) The authors mention that there is no existing work surrounding value homophily and relationship quality. There are papers that seem relevant for inclusion in this discussion (e.g., the effect of similarity in social value orientation on dyadic cooperation, Zhang, Hu, Li, & Wang, 2023; the effect of personal value similarity on relationship satisfaction, Leikas, Ilmarinen, Verkasalo, Vartanen, & Lönnqvist, 2018; see Wolf, Hanel, & Maio, 2021 for further review of related work).

(9) Given that the authors did not find that “explicit beliefs about impartiality or morality [as predicting] impartial altruism” (Line 403), what are the downstream implications of this? I think it’s unlikely that the “impartial beneficence” component of the Oxford Utilitarianism Scale captures only impartiality for self-directed behavior (rather than third-party behavior, particularly given that the phrasing of the items themselves aren’t in the first-person— e.g., “If the only way to save another person’s life during an emergency is to sacrifice one’s own leg, then one is morally required to make this sacrifice”). Could these data demonstrate something like a judgment-behavior gap?

EDITORIAL POLICIES

We ask that you ensure your manuscript complies with our editorial policies and reporting requirements.

To that end, we require revised manuscripts to be accompanied by two completed items: a reporting summary that collects information on study design and procedure, and an editorial policy checklist that verifies compliance with all required editorial policies.

- <https://www.nature.com/documents/nr-reporting-summary.zip> Nature Research Reporting Summary
- <https://www.nature.com/documents/nr-editorial-policy-checklist.pdf> Editorial Policy Checklist

All points on the policy checklist must be addressed. Your revised manuscript can only be sent back to the referees if these checklists are completed and uploaded with the revision.

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Version 2:

Decision Letter:

** Please ensure you delete the link to your author homepage in this e-mail if you wish to forward it to your coauthors **

Dear Ms Amormino,

Thank you for submitting a revised version of your manuscript titled "Does impartial altruism incur costs in close relationships?". After careful consideration and discussion with my colleagues, we would like to ask you to incorporate additional evidence supporting the power of your study.

Currently, your power analysis is based on a single published paper, which is not advisable as effect sizes in the literature are likely to be inflated. On the other hand, we appreciate that your sample size was greater than what was indicated by the power analysis. Therefore, we ask that you add a sensitivity analysis to your manuscript. Please do not conduct a post-hoc power analysis based on the observed effect size in your study (cf. Lakens, 2022, <https://doi.org/10.1525/collabra.33267>). We ask for a sensitivity analysis to be added at this point as we intend to request comments from the reviewers on whether they consider the study sufficiently sensitive (i.e., this effect size sufficiently small) to be informative.

We hope to receive your revised version as soon as possible. If you anticipate a delay of more than three months, however, please let us know. We will be happy to consider your revision so long as nothing similar has been accepted for publication at Communications Psychology or published elsewhere.

We understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

If you are not interested in submitting a suitably revised manuscript in the future please let me know immediately so we can close your file. If you have any questions, please contact me.

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Best regards,

Jennifer Bellington

on behalf of
Mael Lebreton, PhD
Editorial Board Member
Communications Psychology
orcid.org/0000-0002-2071-4890

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Version 3:

Decision Letter:

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Dear Ms Amormino,

Your manuscript titled "Does impartial altruism incur costs in close relationships?" has now been seen by our reviewers, whose comments appear below. In light of their advice I am delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Psychology.

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers and a list of editorial requests. At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Jennifer Bellingtier

Jennifer Bellingtier, PhD
Senior Editor
Communications Psychology

Mael Lebreton, PhD
Editorial Board Member
Communications Psychology
orcid.org/0000-0002-2071-4890

REVIEWERS' EXPERTISE:

Reviewer #1 Moral psychology

Reviewer #2 Moral psychology

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

I appreciate the authors' thoughtful replies to my comments and the other reviewer's comments as well. The only thing I noticed is that the power analysis on lines 198 -206 are still a little confusing:

- a. G*power is a computer program for calculating power. 'pwr' is an R program for reporting power. Which did the authors use? Both? The writing could be clearer in terms of clarifying what exactly the authors did.
- b. Is the power analysis reported on lines 200-201 from the Vekaria et al. paper? I feel like those sentences could be written more clearly in terms of clarifying that the power analysis is from a different study and then saying that you multiplied the amount by two to determine the number of individuals to test in the current work.

Otherwise, nice job! I think these findings are impactful and important.

Reviewer #2 (Remarks to the Author):

Thank you again for the opportunity to review this work. I appreciate the effort that the authors have taken in this revision, and I note a few remaining questions and comments that arose for me in reading the rebuttal letter and updated manuscript.

- (1) The authors discuss value homophily in terms of social discounting --- I wonder if there was also marked similarity in terms of participants' and close others' responses (vs. controls) concerning other value-oriented measures from the research (e.g., relativism, moral tolerance). This kind of analysis may potentially bolster their argument concerning homophily and

relationship quality.

(2) I wasn't able to view the preregistration on the OSF (which may have been due to a server error on the OSF's end). I would recommend that the authors ensure that these documents are publicly visible before the publication of the work.

(3) There seems to be a typo on Page 15 ("others whom altruist have") and perhaps on Pages 12 (i.e., "non-impartial" instead of "partial") and 17 ("participants are aware that their decisions are yoked to real money" vs. "participants were aware...").

(4) I also wonder if the point being made on Page 15 is too strong --- is it right to say that moral judgment self-reports lack "all the salient moral features, external pressures, and other context relevant for real-world moral decisions and actions"? There's certainly some correlation between moral self-report and behavior, and I worry that this point is being made a bit too decisively. (Though I appreciate the authors' addition in discussing a potential judgment-behavior gap!)

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Authors' Responses to R&R

The authors would like to thank the reviewers for taking the time to review and improve our work.

Reviewer 2's Review (in black) and Authors' Responses (in red):

1. At first, I found the use of the $N = 2, 10$, etc. confusing starting around line 172. The reason being is that N typically refers to sample sizes, and N had not been properly explained in the text prior to line 172 (my apologies if I missed it!). I recognize that N is defined in the Method, but someone skimming the article may find the results confusing, nonetheless.

Thank you for this helpful note. We now introduce social distance (N) in the second paragraph of the Results section for clarity: "Therefore, we obtained "amount willing to forgo" (v) values corresponding to each of the social distance (N) combinations (e.g., self vs $N=2$, $N=10$ vs $N=50$, etc.)."

2. Figure 2 could be clearer in terms of what the solid and dashed lines mean. I realize this information is the note, but maybe the difference could be indicated in the legend somehow. Same with Figure 3. I think all that really matters is that the colors are different, but I was searching to see if the dashed versus solid lines meant anything, but I don't think they do.

Thank you for your feedback regarding the figures of social discounting rates (we believe the reviewer meant to refer to Figures 1&3, not 2&3, based on their suggestions). We have updated the figures such that dashed lines represent third-party social discounting and solid lines represent first-person social discounting. Additionally, warm colors represent altruistic pairs and cool colors represent control pairs.

3. I think it's interesting that impartiality doesn't seem to affect social relationships. I did wonder, though, because this is a null effect whether the authors could remark on power to detect this particular effect. The sample sizes are a bit small, which makes sense because the study is recruiting a very specific population. Nonetheless, it would be perhaps beneficial to acknowledge this and provide some context for whether this effect. A power analysis is mentioned on line 494, but I am sort of confused regarding what effect the study was powered to detect.

Thank you for this note. We now include this language around our power analysis in the beginning of the results section:

"We calculated sample size in *R* using G*Power from the 'pwr' package²⁸ using the social discounting effects published by Vekaria and colleagues⁹, which features samples of both altruistic kidney donors and demographically-matched controls (Cohen's $d = 0.80$, $\alpha = 0.05$, target power = 0.80, computation type = two-tailed comparison of means). Power analysis results suggested a total of 26 subjects per group in order to detect a large effect size ($>.79$).

We then multiplied by 2 in order to account for the nesting within the model, a recommendation by Aiken and West ²⁹ suggested by Schönbrodt, Humberg, and Nestler ³⁰ as appropriate for dyadic analysis. Thus, we aimed to recruit a minimum total of 52 dyads per group, or a total of 208 participants, with a stopping rule (due to funding limitations) of 260 participants.”

We are thus confident that the 260 participants we ultimately recruited for this study is sufficient to reject the null hypothesis.

4. Throughout the manuscript, I was thinking about the limitation raised by the authors on line 459 regarding a demand effect. I appreciate that the authors raised this concern and also appreciate their refutation of this point. Unfortunately, I still have lingering concerns. Is there anything in the researchers’ data specifically that could be used to argue that there isn’t a demand going on here? For example, perhaps that altruists moral beliefs do not predict their altruism speaks to this point that they aren’t replying in an especially socially desirable way. I also think that line 462 may have a typo.

Thank you for your note regarding concerns about demand effects. We agree this is an important consideration that merits more discussion.

Our prior work has accumulated several lines of evidence against altruists’ social discounting choices reflecting demand effects. The first is the reliable discrepancy we observe between how people expect altruists to respond in laboratory tasks and how altruists actually respond. Whereas most people expect altruists will show more of every socially desirable trait (be more agreeable, more conscientious, lower in negative emotionality, etc.) altruists’ actual responses reflect a more limited set of differences related to valuing others’ welfare and high levels of honesty-humility (Rhoads et al., 2023). Second, altruists are less likely to differ from controls on tasks that are more transparent and susceptible to demand characteristics (perhaps related to high levels of humility). For example, they do not self-report themselves to be unusually altruistic (Brethel-Haurwitz et al., 2016; Brethel-Haurwitz et al., 2018) or empathic (Rhoads et al., 2023) despite clear evidence from implicit measures that have stronger empathic responses to strangers’ pain and fear. We believe one strength of the social discounting task relative to, e.g., a dictator game, is that it is more complicated, obscuring to participants what it is measuring and reducing potential demand effects. Finally, our prior work finds that neither altruists’ behavioral nor neural response patterns are consistent with their social discounting responses reflecting an effort to override selfish response patterns, but rather appear to reflect genuine differences in the valuation of others’ outcomes. Thus, we are confident that altruists’ response patterns in a standard first-person social discounting task reflect their social preferences.

It is also worth noting that only the first-person social discounting has a “selfish” option versus a more socially desirable generous options. The third-party social discounting task only features generous options, such that there is no obviously more socially desirable choice. If anything, prior work indicates that most people view impartiality as *less* socially desirable than favoritism for close others. Particularly given that participants were completing the task with their N=1, any

potential demand effects would be as likely to push them toward favoritism than toward impartiality.

We now add more to the Discussion paragraph on demand effects and edit out the typos, so it now reads:

It could be argued that the behavior of altruists in this study may reflect their awareness that they were recruited for a study on altruism, introducing an experimental demand bias. Several pieces of evidence argue against this possibility. First, altruists do not score highly on self-report altruism or empathy scales more directly related to altruism and more susceptible to demand effects ^{10,45}. Second, prior work finds that the traits and behaviors that most reliably distinguish altruists from controls are not those predicted by the average person, suggesting that altruists are not simply conforming to stereotype-consistent behaviors ¹⁰. Prior behavioral and neuroimaging work also find no evidence that altruists' reduced social discounting reflects effortfully overcoming selfishness. Instead, neural activation during the task in regions that include amygdala and anterior cingulate cortex correspond to the subjective valuation of others' welfare predicted by the social discounting model ¹². Lastly, the task's real-world payout component further reduces demand effects, as participants are aware that their decisions are yoked to real money.

Reviewer 3's Review (in black) and Authors' Responses (in red):

This is an impressive paper. Thank you for the opportunity to review the work. I appreciate the authors' theoretical and methodological rigor in presenting evidence that impartial altruists do not suffer relational costs. I include suggestions below on how the authors can strengthen the manuscript. Beyond these modifications, I recommend the paper for publication.

(1) On Line 121, the authors describe their method as “compar[ing] altruists and their close others... as they completed a standard social discounting task.” While this is true, the authors also deployed a novel social discounting task. I would make this distinction clearer throughout the paper (e.g., terming one as “standard social discounting” or “first person social discounting” — as they do on Line 165 — and the other as “third-party social discounting” or “novel social discounting”). Beyond consistent terminology, I also would suggest (for example, on Line 100) that the authors describe the method as including both of these tasks — and further emphasize the benefits of the novel task, in addition to the standard paradigm. One of the strengths of the methodology is that the researchers implemented the standard and improved paradigm, finding converging results across both.

Thank you so much for the recommendation to keep the language regarding these two tasks consistent. We have revised manuscript language to the more standardized terminology of “first-person social discounting” and “third-party social discounting” throughout. We also added more

language introducing the novel third-party social discounting in the second paragraph of the introduction:

“We developed a novel impartial decision-making task to behaviorally assess impartial altruism. We adapted the first-person social discounting paradigm (wherein participants elect to keep money for themselves or split it between themselves and a social other) to assess third-party decision-making (wherein participants elect to keep money for a close social other or split it between a close social other and a more distant social other) to determine how people make valuations between close and distant others. We administered both the first-person social discounting paradigm and the novel third-party social discounting paradigm to pairs of close others.”

(2) On Line 157, the authors mention two alternative hypotheses. The authors should motivate each of these to some extent, prior to transitioning into the results. Further, I found myself wanting deeper discussion of why consensus bias would predict “first-person social discounting to predict allocation... in the third-party task” (Line 337).

Thank you for this suggestion. We now elaborate on the motivation behind these hypotheses in the introduction:

“As for why altruists exhibit increased impartial altruism in the third-party social discounting task, we hypothesized and tested three potential psychological mechanisms: One hypothesis is that impartial decision-making is driven by a generally heightened concern for impartial beneficence—that is, altruists’ increased impartial generosity could be significantly predicted by a generally heightened concern for impartial beneficence (as measured through the Oxford Utilitarianism Scale ²⁵); a second, related hypothesis is that impartial third-party social discounting is driven by a lack of moral tolerance for social others who might prefer favoritism (on their own behalf) over impartiality—that is, no matter what social others might prefer, impartial decision-making is determined by the decision maker’s moral dogma of impartiality; lastly, the third hypothesis is that individuals use their own first-person social discounting tendencies as a cognitive anchor ²⁶ when engaging in third-party social discounting, such that impartial decision-making is driven by an overreliance on how oneself would socially discount and a failure to properly adjust to social others’ differing preference sets ²⁶, resulting in a form of consensus bias, or the implicit view that others share one’s own beliefs and preferences ²⁷. We tested the first two hypothesized mechanisms by investigating the predictive roles of impartial beneficence and moral tolerance/relativism on third-party social discounting behavior; and we tested the last hypothesized mechanism by investigating the predictive role of first-person social discounting behavior on third-party social discounting behavior.”

(3) I found myself wanting to know more about the recruitment of the altruistic sample. The authors mention a “database of verified altruistic kidney donors,” but I still wonder about selection bias. Could there be any differences, even within this database, concerning which people would be most likely to participate?

Thank you for raising this concern. One way to consider this question is to consider how the demographics of our sample compare to the demographics of non-directed donors on average. Thus, we compared national demographic data from the Organ Procurement and Transplantation Network (OPTN) to our sample, similar to methods from Vekaria and colleagues' (2017) kidney donor comparisons. We cannot (in a straightforward manner) compare age, as donors continue to age after transplant, while the OPTN only shows age at time of donation. That said, we did look at race, ethnicity, and gender between our sample and the national database. 59.2% of living kidney donors in the national database identify as female, which is a similar gender ratio to our sample (62.7% female). Our sample is also similarly majority white (93.2%) compared to the national database (91.4%). We discuss this lack in racial and ethnic diversity in our discussion: "One limitation is that efforts to recruit controls who were demographically matched with altruists resulted in a sample that was relatively racially and ethnically homogenous and on average, richer and more educated than the average American. This in part reflects the selection criteria applied to living kidney donors, who must typically meet stringent health criteria before donating, including low risk for kidney disease, which disproportionately affects lower-income, Black, and Hispanic adults in the United States. However, it is reasonable to assume our findings would apply to more demographically representative samples, as prior research has found that more diverse altruistic samples, such as bone marrow donors and heroic rescuers,^{16,43,44} display similar reductions in first-person social discounting as altruistic kidney donors¹⁰ as well as other similar personality and behavioral traits."

Another consideration is whether non-directed donors who opted to participate in this study are more altruistic and/or impartial than donors on average. We do not believe this is true, in part because the positive response rate is extremely high among altruistic kidney donors (41% of emailed individuals responded to the recruitment emails). If anything, it is likely a bigger problem that our control sample (which we should note was also recruited from a database, as in all online studies) is more altruistic than the population average. Prior research finds that psychology research participants are unusually prosocial (Van Lange et al., 2011). This is logical, as participating in research studies is a form of time and energy-consuming volunteering that yields no direct benefit to participants (as IRBs require participants be informed during consenting). Thus, if anything our observed group differences are likely smaller than they would be if we were to test truly random samples from the population.

(4) Income predicted altruism across many of the results reported in the paper (e.g., Line 217, 235, 308, 314), but the authors do not discuss this finding at all within the manuscript. I'd recommend that they say something about why this might be the case in their discussion of the findings.

Thank you for highlighting this. We have added a note about this to our Discussion: "It is also worth noting that income was associated with altruist status in our models. This is likely due to sampling, as demographically similar controls were on average lower income in our sample (see Table S1 in the Supplemental Materials). This group difference is consistent with prior work

showing reliable positive associations between altruism and various measures of objective and subjective well-being, including wealth (Rhoads & Marsh, 2024).”

(5) Formatting error: No closed parenthetical on Line 199.

Thank you for raising this concern—we fixed this typo.

(6) One methodological limitation that the authors do not address is whether the “social distance” scale actually matches social closeness across participants. For example, if I set my friend John as N = 20, how do we know that this matches the closeness for N = 20 across all other participants? I would like to know more about whether the authors did anything to validate this metric, as it is the cornerstone of the paper.

Thank you for raising this concern. We collected participants’ reports of emotional closeness to each N and have now compared emotional closeness ratings from altruists and controls for six of the seven social distances (N=1,2,5,10,20,50) but not for the stranger (N=100) in a series of six t-tests, one for each social distance. We found no significant difference in emotional closeness between groups at any social distance:

N=1: No significant difference in N=1 emotional closeness scores between the altruist group (M = 12.43, SD = 1.00) and the control group (M = 12.44, SD = 1.01), $t(142.53)=-0.013$, $p=0.990$.

N=2: No significant difference in N=2 emotional closeness scores between the altruist group (M = 10.03, SD = 1.12) and the control group (M = 10.35, SD = 1.14), $t(139.61)=-0.556$, $p=0.579$.

N=5: No significant difference in N=5 emotional closeness scores between the altruist group (M = 8.96, SD = 1.15) and the control group (M = 8.28, SD = 1.12), $t(139.46)=1.160$, $p=0.248$.

N=10: No significant difference in N=10 emotional closeness scores between the altruist group (M = 7.21, SD = 1.08) and the control group (M = 6.83, SD = 1.15), $t(143.55)=0.757$, $p=0.450$.

N=20: No significant difference in N=20 emotional closeness scores between the altruist group (M = 5.84, SD = 1.20) and the control group (M = 6.02, SD = 1.18), $t(146.82)=-0.430$, $p=0.668$.

N=50: No significant difference in N=50 emotional closeness scores between the altruist group (M = 4.40, SD = 1.12) and the control group (M = 4.95, SD = 1.15), $t(137.85)=-1.392$, $p = 0.166$.

These findings are consistent with past work comparing extraordinary altruists to controls, which also has found that ratings of emotional closeness does not differ between altruists and controls in this task (Rhoads et al., 2023).

As we also reference in the methods section of the manuscript: “Altruists and controls did not differ significantly in relationship type (i.e., spouse, friend, roommate, etc.; see Figure S1 in the SI) across nominations.”

Along with the supplemental figure, we also note in the supplementary materials that: “To assess whether groups ranked people similarly across different social distances, participants

described their relationship with each N via a multiple-choice prompt, which included 14 relationship categories (see Figure S1). Chi-square tests of independence (2x14, where group = altruistic/control dyad) were performed at each social distance to detect any differences between groups and the participants' relationship with each N. Groups did not differ in the proportion of relationship types with N's across social distances (see Figure S1): At Social Distance #1, $\chi^2(30, N = 260) = 10.25, p = 1.00$; Social Distance #2, $\chi^2(33, N = 260) = 13.33, p = 1.00$; Social Distance #5, $\chi^2(42, N = 260) = 19.00, p = 1.00$; Social Distance #10, $\chi^2(39, N = 260) = 12.27, p = 1.00$; Social Distance #20, $\chi^2(33, N = 260) = 13.32, p = 1.00$; Social Distance #50, $\chi^2(33, N = 260) = 10.43, p = 1.00$. In line with prior work, this suggests that all groups similarly understood nomination instructions and similarly differentiate between nominated social target"

(7) On Line 558, the authors begin a description of the payout scheme for participants. I found the lack of deception (and ties to actual social networks) to be compelling, and I would recommend that the authors further emphasize the ecological validity of both of these aspects of their paradigm in the primary text. This is a tremendous improvement over, for example, self-report (e.g., "Imagine making an allocation between a friend vs. a stranger").

We thank you for your kind words regarding the task design! We now note the real-world stakes we use at the end of the introduction: "For the social discounting task to have real stakes, participants were informed at the start of the study that their decisions are yoked to real-world consequences wherein one of their choices will be randomly selected for a payout to the participants and/or their nominated social other(s)." and again in the discussion: "Lastly, the task's real-world payout component further reduces demand effects, as participants are aware that their decisions are yoked to real money."

(8) The authors mention that there is no existing work surrounding value homophily and relationship quality. There are papers that seem relevant for inclusion in this discussion (e.g., the effect of similarity in social value orientation on dyadic cooperation, Zhang, Hu, Li, & Wang, 2023; the effect of personal value similarity on relationship satisfaction, Leikas, Ilmarinen, Verkasalo, Vartanen, & Lönnqvist, 2018; see Wolf, Hanel, & Maio, 2021 for further review of related work).

Thank you so much for these helpful references. We have updated the introduction with this information:

"Homophily in both prosocial and antisocial values have been found to predict improved relationship quality, though not *all* values have such effects. Similarity in self-direction values (interest in independent thought and action, expressed in choosing, exploring, and creating ²²) has been found to predict higher relationship satisfaction in opposite-sex spousal pairs ²³. Meanwhile, other values more closely related to impartiality, such as benevolence (concern for the welfare of one's ingroup ²²), are not predictive of relationship quality. Interestingly, though same-sex spousal pairs exhibit similar levels of universalism (concern for the welfare of all

beings)²² to each other, this similarity is not predictive of relationship quality²³. Similar levels of some antisocial traits, such as narcissism and psychopathy have been found to predict increased relationship quality in heterosexual romantic pairs²⁴. Could similarity in impartiality serve as a similar protective factor for altruistic pairs?”

(9) Given that the authors did not find that “explicit beliefs about impartiality or morality [as predicting] impartial altruism” (Line 403), what are the downstream implications of this? I think it’s unlikely that the “impartial beneficence” component of the Oxford Utilitarianism Scale captures only impartiality for self-directed behavior (rather than third-party behavior, particularly given that the phrasing of the items themselves aren’t in the first-person— e.g., “If the only way to save another person’s life during an emergency is to sacrifice one’s own leg, then one is morally required to make this sacrifice”). Could these data demonstrate something like a judgment-behavior gap?

Thank you for introducing this idea as a potential explanation for our findings. We have amended this portion of the discussion to read: “Although past work has found impartial beneficence to predict extraordinary, selfless altruism¹³, the present study’s findings corroborate the presence of a moral judgment-behavior gap³³, perhaps because moral judgments and self-reported moral values lack all the salient moral features, external pressures, and other context relevant for real-world moral decisions and actions.”

Authors' Responses to R&R

The authors would like to thank the reviewers for taking the time to review and improve our work.

Reviewer #1 (Remarks to the Author):

I appreciate the authors' thoughtful replies to my comments and the other reviewer's comments as well. The only thing I noticed is that the power analysis on lines 198 -206 are still a little confusing:

a. G*power is a computer program for calculating power. 'pwr' is an R program for reporting power. Which did the authors use? Both? The writing could be clearer in terms of clarifying what exactly the authors did.

Thank you for this helpful note. We agree with the reviewer that this should be worded more clearly. While G*Power uses *R* statistical computing, it would be more straightforward to describe and cite G*Power directly. Thus, we have amended the language to say: "We calculated sample size in G*Power²⁸ based on the social discounting effects published by Vekaria and colleagues⁹, which features samples of both altruistic kidney donors and demographically-matched controls (Cohen's $d = 0.80$, $\alpha = 0.05$, target power = 0.80, computation type = two-tailed comparison of means)." We also make this same change in the Methods section when referencing the power analysis: "The sample size was determined through a power analysis conducted in G*Power²⁸."

b. Is the power analysis reported on lines 200-201 from the Vekaria et al. paper? I feel like those sentences could be written more clearly in terms of clarifying that the power analysis is from a different study and then saying that you multiplied the amount by two to determine the number of individuals to test in the current work.

Thank you for this helpful note. We agree with this sentiment and have reworded to the following: "The results of this power analysis suggested a total of 26 subjects per group in order to detect an effect of this size ($>.79$). We then multiplied this computed sample size by 2 in order to account for the nesting within the model, a recommendation by Aiken and West²⁹ suggested by Schönbrodt, Humberg, and Nestler³⁰ as appropriate for dyadic analysis."

Otherwise, nice job! I think these findings are impactful and important.
Thank you!

Reviewer #2 (Remarks to the Author):

Thank you again for the opportunity to review this work. I appreciate the effort that the authors have taken in this revision, and I note a few remaining questions and comments that arose for me in reading the rebuttal letter and updated manuscript.

(1) The authors discuss value homophily in terms of social discounting --- I wonder if there was also marked similarity in terms of participants' and close others' responses (vs. controls) concerning other value-oriented measures from the research (e.g., relativism, moral tolerance). This kind of analysis may potentially bolster their argument concerning homophily and relationship quality.

We appreciate this suggestion. We went back to the data and calculated difference scores for each dyad's area-under-the-curve (AUC) social discounting, Moral Tolerance and Moral Relativism scores (MTS and MRS), and Impartial Beneficence and Instrumental Harm as indexed by the Oxford Utilitarianism Scale (OUS-IB and OUS-IH). We also calculated the average relationship quality scores for each dyad as indexed by the McGill Friendship Questionnaire's Friendship Function and Respondent Affection subscales (MFQ-RA and MFQ-FF). Because of the lack of variance in the MTS and MRS across individuals and across groups, all follow-up models including the MTS or MRS were insignificant ($p > .15$). Additionally, because relationship quality scores are basically at ceiling across groups, all follow-up models predicting relationship quality from MRS, MTS, OUS-IB and OUS-IH were insignificant as well. That said, insignificant models' possessed directionality consistent with our hypotheses. This includes models trending in significance:

Linear regression predicting AUC difference scores from averaged MFQ-FF scores (Not trending in significance but included in our response for reader comparison with MFQ-RA)

	<i>b</i>	SE	p-value
Intercept	0.29	0.26	0.272
Friendship Function	-0.02	0.03	0.597
<i>Demographic Covariates</i>			
Age ^a	0.0007	0.0027	0.804
Gender ^a	-0.05	0.05	0.374
Household Income ^a	0.02	0.02	0.399
Age ^b	0.0025	0.0029	0.390
Gender ^b	0.0055	0.055	0.922
Household Income ^b	-0.03	0.02	0.239

$R^2 = .012, p = .31$

Note. SE = Standard Error.

^aClose-others' data

^bAltruists' and Controls' data

* $p \leq 0.05$, ** $p \leq 0.01$, *** $p \leq 0.001$

Linear regression predicting AUC difference scores from averaged MFQ-RA scores (Model is insignificant but predictor of interest is trending in the hypothesized direction)

	<i>b</i>	SE	p-value
Intercept	0.44	0.21	0.0387*
Respondent Affection	-0.07	0.04	0.0977
<i>Demographic Covariates</i>			
Age ^a	0.0006	0.0027	0.8286
Gender ^a	-0.05	0.05	0.3162
Household Income ^a	0.02	0.02	0.3238
Age ^b	0.0023	0.0028	0.4202
Gender ^b	-0.0047	0.0549	0.9320
Household Income ^b	-0.03	0.02	0.1842

$R^2 = -.01, p = .53$

Note. SE = Standard Error.

^aClose-others' data

^bAltruists' and Controls' data

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Linear regression predicting close-other's OUS-IB scores from altruists' and controls' OUS-IB scores (Model is non-significant but predictor of interest is trending in the hypothesized direction)

	<i>b</i>	SE	p-value
Intercept	2.76	0.56	<.001***
Impartial Beneficence	0.17	0.09	0.0663
<i>Demographic Covariates</i>			
Age ^a	0.0022	0.01	0.8296
Gender ^a	-0.13	0.19	0.4931
Household Income ^a	-0.07	0.08	0.4196
Age ^b	-0.006	0.01	0.5655
Gender ^b	-0.14	0.20	0.4867
Household Income ^b	0.08	0.08	0.3411

$R^2 = -.002, p = .46$

Note. SE = Standard Error.

^aClose-others' data

^bAltruists' and Controls' data

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Linear regression predicting close-other's OUS-IH scores from altruists' and controls' OUS-IH scores (Model is trending and predictor of interest is significant in the hypothesized direction)

	<i>b</i>	SE	p-value
Intercept	3.95	0.71	<.001***
Instrumental Harm	0.26	0.10	0.00817**
<i>Demographic Covariates</i>			
Age ^a	-0.0024	0.0117	0.8374
Gender ^a	0.12	0.23	0.5882
Household Income ^a	-0.06	0.10	0.5057
Age ^b	-0.0091	0.012	0.4495
Gender ^b	-0.26	0.23	0.2730
Household Income ^b	0.033	0.10	0.7301

$R^2 = .06, p = .055$

Note. SE = Standard Error.

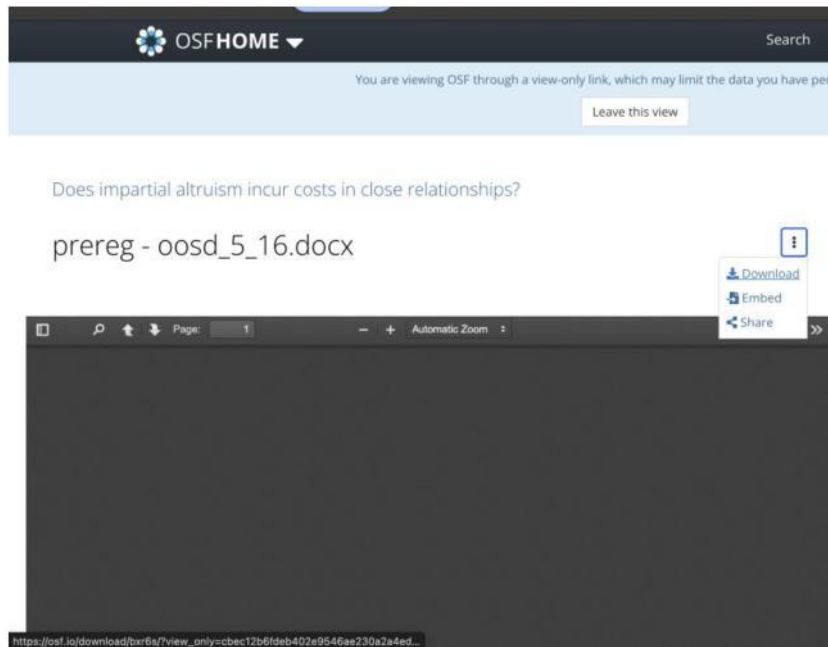
^aClose-others' data

^bAltruists' and Controls' data

* $p \leq 0.05$, ** $p \leq 0.01$, *** $p \leq 0.001$

(2) I wasn't able to view the preregistration on the OSF (which may have been due to a server error on the OSF's end). I would recommend that the authors ensure that these documents are publicly visible before the publication of the work.

Thank you for this note! Using the view-only link, it looks like the preview page might not appear depending on your device/application, but it is still downloadable:



(3) There seems to be a typo on Page 15 ("others whom altruist have") and perhaps on Pages 12 (i.e., "non-impartial" instead of "partial") and 17 ("participants are aware that their decisions are yoked to real money" vs. "participants were aware...").

Thank you for finding these. We reworded page 15's text to read less awkwardly: "People who view altruists' impartiality unfavorably may be less likely to form or maintain close relationships with them." We also changed "non-impartial" to "partial" on page 12 per your recommendation. Lastly, we fixed the tenses on page 17 to read: "Lastly, the task's real-world payout component further reduces demand effects, as participants were aware that their decisions were yoked to real money."

(4) I also wonder if the point being made on Page 15 is too strong --- is it right to say that moral judgment self-reports lack "all the salient moral features, external pressures, and other context relevant for real-world moral decisions and actions"? There's certainly some correlation between moral self-report and behavior, and I worry that this point is being made a bit too decisively. (Though I appreciate the authors' addition in discussing a potential judgment-behavior gap!)

Thank you for noting this. We agree with this observation, and have reworded "all the salient moral features..." to "some of the salient moral features" so that the argument in its entirety now reads: "Although past work has found impartial beneficence to predict extraordinary, selfless altruism¹³, the present study's findings corroborate the presence of a moral judgment-behavior gap³⁶, perhaps because moral judgments and self-reported moral values lack some of the salient moral features, external pressures, and other context relevant for real-world moral decisions and actions."