Dear Dr. Dehghani,

Thank you for submitting your manuscript to Journal of Experimental Social Psychology. As well as reading your paper myself, I have been fortunate to solicit very helpful and considered reviews from three experts in the relevant fields including charitable donation, moral psychology, and social influence. Their reviews are appended to this letter. Their recommendations diverged, with Reviewer 1 being favorably disposed toward the paper and suggesting a revise-resubmit, whereas Reviewers 2 and 3 recommended rejection.

Although their recommendations are divergent and they make several rather divergent comments, you will also see that they converged on key points as well. Notably, all three reviews appreciated your methodological approach and sophistication, but all three raised concerns about the contribution to our theoretical knowledge of donation. Beyond this very broad summary, I shall attempt no further summary or reiteration of their comments, because I think they are already made very clearly. But alas, taking their comments into account, together with my own reading, I am afraid that I cannot accept the manuscript for publication. Nor do I think the path to revising the paper to make it suitable for this journal is clear enough for me to invite you to embark upon it.

Most important, I found the case for your research agenda to be well-argued. I agree that the dynamics of online communication on social media and microblogging sites are important to understand as powerful engines of donation, and that the psychological mechanisms at play are likely to be different than those in other contexts. On the other hand, I shared the reviewers’ concern that it is hard to make theoretical sense of the results. All three reviewers express dissatisfaction with the theoretical motivation of your research and its contribution to theory. This is not only because results were mixed, but also because of the way the research was conceptualized and presented. Allow me to add some specific observations that I think contribute to the lack of theoretical clarity in the paper. Although you are unlikely to agree with all of these observations, I hope that at least some of them are useful for you to consider.

For me, many of the problems stem from your literature review. Many potential variables that influence donation are mentioned, but the relation between these variables, which often seem to overlap with each other, was not made clear. It helps to impose some simplicity, for example by organizing them into superordinate clusters (even if horizontal relations between constructs within a cluster are not precisely specified). However, too often, I found their hierarchical organization unclear (e.g., on p. 5, are group affiliation, perceived instrumental value, and disaster awareness “individual-level factors”, falling under the remit of the topic sentence in the previous paragraph, or did you mean to confine this factors like “empathy” and “perspective taking”, mentioned in the previous paragraph). Elsewhere, I found them debatable: are moral foundations values, as implied by their placement under the heading? And is a sense of obligation or responsibility a value? Also note that there are powerful critiques of the assumption in MFT that harm and fairness values are individualizing foundations, whereas purity, loyalty and authority are binding foundations (published for example by Matthew Kugler, and Ronnie Janof-Bulman).
Another factor that undermined the theoretical contribution is to be found in the statement of aims. Crucially, the key theoretical argument used to motivate the present research, as stated on p. 7, is in need of elaboration:

This raises the question of if and to what extent moral values influence charitable donation through the mechanism of solicitation framing. That is, while we know that individuals’ moral values are associated with their donation behavior, little is known about how expressions of moral values in solicitation frames may influence charitable donation.

Another issue that derogates the theoretical contribution of your research is that your decision to privilege MFT (over the several other mentioned approaches to moral values – if indeed the foundations are moral values) is not justified. Further, the review of MFT is scant, and does not allow you to draw out which results would be effectively confirmatory of what we know about moral foundations as predictive individual differences, and which would be new.

Further, it is not clear why you would expect any deviation from (or similarity to) the relation between individual differences in moral values and donation in the effect of moral values when employed in solicitation frames. You have set up an essentially communicative research question, and have justified your research powerfully by arguing that communication dynamics are poorly understood but likely potent drivers of donation. Thus it seems a pity that your explicitly communication research agenda is not informed by a close reading of communication theory. Inviting communicative processes into the literature on charitable donation also invites consideration of communicative factors such as cognitive tuning, common ground, shared reality, reactance or interpretation of communicative intent (the communication game)– none of which are reviewed in your introduction. The literature on solicitation of donations is reviewed only in one paragraph and reads rather as a list of findings, which are not mapped onto moral values.

This is perhaps a missed opportunity, because they could be so mapped: for example the work on framing donations as economic transactions was situated in Dale Miller’s theorizing about on the myth of self-interest, which is a deep-seated cultural value. In this way, the literature review does not succeed in imposing sufficient order on the literature to allow for a feasibly orderly empirical investigation of the phenomenon – even an exploratory investigation. Although I am all for open-ended, exploratory work, and your approach to confirmatory experiments given the dearth of discoveries in psychology (see Rozin, 2009), I think the literature could already could furnish you with meaningful, theoretically grounded hypotheses that you could test at the same time as your explorations.

So again, for me and I think the reviewers, a factor that limited the contribution of the research was that you did not delineate what hypotheses can be inferred from theory from those that are truly exploratory.

The final factor that I will mention is picked up explicitly by Reviewer 3 (and less explicitly by Reviewer 2) and undermines the theoretical contribution of the research and the coherence of the empirical program. That is, Study 1 examines whether donation sentiment is associated with lexical indicators of moral foundations in tweets. These tweets may or may not be solicitations to donate, and their impact is not studied. Studies 2-5 instead seem to examine the effectiveness of solicitations. It just seems to be a different research question, and it is not clear how these experiments confirm (or otherwise) the results of Study 1.

In sum, I think it is clear that we all – the reviewers and I – truly appreciated your methodological innovation and rigour. It’s rare to see, and methodologically there is little doubt that your paper clears the bar for JESP (notwithstanding the important methodological observations made by the reviewers). What prevents these studies from being published here is their limitations considered collectively, rather than individually, and their unclear relation to theoretical frameworks in social psychology. Study 1 could make an excellent contribution on its own to journals concerned with online communication. The rest of the studies may not sit so easily with Study 1, and perhaps you could think about designing experiments that are truly confirmatory (e.g., when asked to construct messages that are pro donation vs. not, do people invoke moral foundations). These experiments, by manipulating communicative intent orthogonally to participants’ pre-experimental moral foundations or private attitudes to donation, might present a cleaner conceptual departure from the individual differences research that has been conducted to date. The existing studies 2-5 may comprise the basis of an interesting paper on the effectiveness of different messages. Of course, a holy grail in this line of research would be to find a way to examine (observationally, in your naturalistic data) which tweets were effective in soliciting donations.

We appreciate your submitting your manuscript to this journal and for giving us the opportunity to consider your work. Thanks again to the reviewers for their excellent, detailed and constructive commentaries ranging from sharp and observant edits (Reviewer 1) to observations about results.
(Reviewer 2) and methods (Reviewer 3). Finally, I apologize for the delay in getting this decision to you and thank you for your grace and patience. I wish you well in developing this highly promising, sophisticated - and socially important – line of research.

Kind regards,

Robbie Sutton

Associate Editor

Journal of Experimental Social Psychology

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**C:P Editor Decision for Version 1**

**Editor:** Brent Donnellan  
**Affiliation:** Michigan State University, US  
**Editor decision:** Revisions Required  
**Decision date:** 03 January 2018

Dear Morteza Dehghani,

Thank you for submitting your work to Collabra: Psychology under our streamlined review process. I first read your paper, then the previous reviews, and the cover letter. I generally like to see authors respond point-by-point to each issue raised in the prior review process but I did not detect this issue until after I read the paper. I also took some time off for the end of the year holidays so I did not want to further delay a decision. Thus, I went ahead and made a decision but I will ask you to this kind of response for the revision to be consistent with how I want to handle streamlined papers at Collabra in the future.

The bottom line is that I believe a revised version of the paper would be a good fit for this journal. First and foremost, I thought this was an interesting package of studies that blended exploratory and confirmatory analyses. I did not have the concerns about HARKing that were raised by Reviewer #3 (and described globally in your letter). I think this paper is a useful model for how to package exploratory and confirmatory studies together when investigating novel ideas. I thought the preregistrations made sense and the work was transparent. Ultimately, I think the work is described adequately enough for readers to make up their minds about the strength of the case being advanced in the paper. Thus, I believe the work has the necessary characteristics for being eventually published in this outlet. Accordingly, I would like you to make a final revision of this paper for ultimate consideration at Collabra: Psychology. I ask that you detail how you responded to the previous reviews in a point-by-point response and address the few issues I noted below. I will then make a final up or down decision.

Below are the issues I noted in reading the work. You might disagree with some (or all) of the points so feel free to push-back against any suggestions you believe will harm your work. Just describe your counterpoints in the letter.

1. I checked the preregistrations for the studies. It was noted in Study 2 that one-tailed tests would be used. This is fine with me but please note this for readers. (And I apologize if I missed this in the text). It would also be good then to note if the planned contrasts were one versus two-tailed throughout the paper.

2. I thought the Introduction was clear but I thought it might make sense to streamline it a bit further (especially in light of the JESP reviews). I am not sure that you needed to spend so much time...
on other individual differences effects and this could save space to spend a bit more time discussing
MFT in the context of charitable giving and message framing. There was also some discussion about
higher-order structure of moral foundations (individuating versus binding) that appears on page 9. I
did not see that this issue was a consistent thread throughout the rest of your work so perhaps this
distinction might be worth returning to in the Discussion or it might even be cut from the Introduc-
tion if it is not relevant for understanding or interpreting the current work. If this is a big idea,
it might be good to report some exploratory summary analyses using composites for individuating
versus binding foundations.

3. There is a citation to the WEIRD paper on page 11. I did not think your work did anything to
address WEIRD samples given that MTurk workers are probably mostly WEIRD in some fashion or
another (e.g., most are educated). Thus, you might want to change that citation to something more
classic about college sophomores in the laboratory.

4. I think I understood all of the rationales for winnowing the tweets described on page 15 but
I think a couple more sentences might help readers even better understand the decisions and the
ramifications.

5. I think you could report more d-metric effects for Study 2. Unless I missed it, there were only
95% confidence intervals being reported. You might also clarify in the text how exactly the d-metric
effects were computed for each study (the big issue is the denominator, I believe).

6. There were 81 dropped participants in Study 3 (p. 26). Is this a high number relative to previous
work? I doubt it matters but I did wonder if dropping people was the best way to handle this kind of
missing data.

7. I might have missed this but were hypothetical donations and real donation correlated in Study 5?
Were the data in Study 5 a good fit with parametric ANOVAs?

To access your submission account, follow the below instructions:
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Please ensure that your revised files adhere to our author guidelines, and that the files are fully
copyedited/proofed prior to upload. Please also ensure that all copyright permissions have been
obtained. This is the last opportunity for major editing; therefore please fully check your file prior
to re-submission.

If you have any questions or difficulties during this process, please do contact us. My direct email is
donnel59@msu.edu in case you want to contact me.

Please could you have the revisions submitted by 3 March 2018? If you cannot make this deadline, please let us know as early as possible.

Kind regards,

Brent Donnellan

Michigan State University
donnel59@msu.edu

Author’s Response to Review Comments for Version 1

Author: Morteza Dehghani, PhD
Affiliation: Assistant Professor, Psychology, Computer Science, University of Southern California, US
Revision submitted: 15 February 2018

Dear Dr. Donnellan,

First and foremost, we would like to thank you and the reviewers for considering our manuscript and giving us such constructive feedback. We would also like to thank you for allowing us to resubmit the paper. We have carefully revised our paper and have addressed all the issues discussed. We have also added a point-by-point response to the issues raised in the prior review process. Below we quote the entirety of your and the reviewers’ comments (in italics) and detail how we modified the paper to respond to each point. Please note that a pdf version of this letter is both attached to this email and uploaded as supplementary material.

Response to Dr. Donnellan:

Thank you for submitting your work to Collabra: Psychology under our streamlined review process. I first read your paper, then the previous reviews, and the cover letter. I generally like to see authors respond point-by-point to each issue raised in the prior review process but I did not detect this issue until after I read the paper. I also took some time off for the end of the year holidays so I did not want to further delay a decision. Thus, I went ahead and made a decision but I will ask you to this kind of response for the revision to be consistent with how I want to handle streamlined papers at Collabra in the future.

The bottom line is that I believe a revised version of the paper would be a good fit for this journal. First and foremost, I thought this was an interesting package of studies that blended exploratory and confirmatory analyses. I did not have the concerns about HARKing that were raised by Reviewer #3 (and described globally in your letter). I think this paper is a useful model for how to package exploratory and confirmatory studies together when investigating novel ideas. I thought the preregistrations made sense and the work was transparent. Ultimately, I think the work is described adequately enough for readers to make up their minds about the strength of the case being advanced in
the paper. Thus, I believe the work has the necessary characteristics for being eventually published in this outlet. Accordingly, I would like you to make a final revision of this paper for ultimate consideration at Collabra: Psychology. I ask that you detail how you responded to the previous reviews in a point-by-point response and address the few issues I noted below. I will then make a final up or down decision.

We thank Dr. Donnellan for his thoughtful and encouraging comments. We hope our responses address the issues noted.

Below are the issues I noted in reading the work. You might disagree with some (or all) of the points so feel free to push-back against any suggestions you believe will harm your work. Just describe your counterpoints in the letter.

1. I checked the preregistrations for the studies. It was noted in Study 2 that one-tailed tests would be used. This is fine with me but please note this for readers. (And I apologize if I missed this in the text). It would also be good then to note if the planned contrasts were one versus two-tailed throughout the paper.

Thank you for catching this omission. We now explicitly note that the null hypothesis tests for these t-tests were one-tailed. However, the planned contrasts throughout the rest of the paper are tested with two-tailed tests; we now clarify this with a footnote (pg 24).

2. I thought the Introduction was clear but I thought it might make sense to streamline it a bit further (especially in light of the JESP reviews). I am not sure that you needed to spend so much time on other individual differences effects and this could save space to spend a bit more time discussing MFT in the context of charitable giving and message framing. There was also some discussion about higher-order structure of moral foundations (individuating versus binding) that appears on page 9. I did not see that this issue was a consistent thread throughout the rest of your work so perhaps this distinction might be worth returning to in the Discussion or it might even be cut from the Introduction if it is not relevant for understanding or interpreting the current work. If this is a big idea, it might be good to report some exploratory summary analyses using composites for individuating versus binding foundations.

To an extent, we agree that we did not need to spend as much time reviewing individual difference effects; however, the consensus among the previous reviewers regarding the perceived paucity of our literature review led us to substantially restructure and expand the review. Ultimately, we hope that this review, which we wrote while trying to optimize concision and breadth, will be useful for others. There are only a few papers that offer a broad summary of donation effects and we hope that our review might make this literature more accessible for others.

Regarding cutting the supraordinate MFT categories, we thank you for this suggestion. As you note, that distinction is not relevant to the current work; rather, we mentioned in passing only to familiarize people with that view of MFT. Given the responses of the previous reviewers and your observations, we fully agree that a discussion of binding vs individualizing values just distracts from the primary foci of the paper.
3. There is a citation to the WEIRD paper on page 11. I did not think your work did anything to address WEIRD samples given that MTurk workers are probably mostly WEIRD in some fashion or another (e.g., most are educated). Thus, you might want to change that citation to something more classic about college sophomores in the laboratory.

Thank you for this suggestion. We agree that neither MTurk nor Yourmorals.org samples are likely to be robust against WEIRD biases. We have thus removed this citation.

4. I think I understood all of the rationales for winnowing the tweets described on page 15 but I think a couple more sentences might help readers even better understand the decisions and the ramifications.

Thank you for this suggestion. We have revised and slightly extended our discussion of the exclusion process (pg 15). Specifically, we have tried to give a more specific example of the risks of our exclusion process, as well as clarify what it might afford us in terms of sample fidelity.

5. I think you could report more d-metric effects for Study 2. Unless I missed it, there were only 95% confidence intervals being reported. You might also clarify in the text how exactly the d-metric effects were computed for each study (the big issue is the denominator, I believe).

Again, thank you for this suggestion. Effect sizes and their CIs for the study 2 tests were meant to be included in the manuscript and their omission was accidental. We have added them to our revision and noted the software used to calculate them.

We also clarify how Cohen’s d was calculated for study 1 in a footnote on page 18. Specifically, we used the model estimates of the mean and its standard error to calculate: d = mean/sd, where sd = se*sqrt(n). As you noted, it was not clear in the text whether we calculated effects sizes based on sample statistics or model estimates. Our footnote aims to clarify that distinction.

6. There were 81 dropped participants in Study 3 (p. 26). Is this a high number relative to previous work? I doubt it matters but I did wonder if dropping people was the best way to handle this kind of missing data.

Thank you for this comment. We can certainly see why one would question listwise removal of 81 participants. However, relative to our other studies, we think this is a rather small exclusion n. For example, 386 participants were collected for study 2 and 73 participants were excluded due to attention check failures. In contrast, we collected 1,116 participants for study three and excluded only 8 more participants than in study 2, a substantially smaller proportion of exclusion.

That all said, we agree that listwise deletion is often not the best way to deal with missing data.
However, we elected to use (and pre-registered) this approach because it is simple, widely understood, and posed no foreseeable risks (in our opinion) within the context of the current work. Given that we were able to plan for such exclusions when determining our sample sizes, we feel that they pose to risk to the validity of our tests. That is, based on our power analyses, we oversampled based on the assumption that a small portion of participants would be excluded due to manipulation or attention check failures.

7. I might have missed this but were hypothetical donations and real donation correlated in Study 5? Were the data in Study 5 a good fit with parametric ANOVAs?

These are excellent questions and we have conducted several follow-up analyses to answer them.

First, the real and hypothetical donations are indeed correlated, rs > 0.65. We should have checked this, as it clearly shows the expected correspondence between real and hypothetical donation.

Second, we can say that the data were not a bad, per se, fit with parametric ANOVAs. The data could have been more normal and the variances could have been more equal, but no assumptions were flagrantly violated. Accordingly, we proceeded with our pre-registered analysis.

However, thanks to your comment, we have supplemented our original study 5 analyses with a set of robust analyses (robust ANOVAs with 20% trimmed means and no assumption of equal variance). From these models, we find no evidence for any effects on charitable donation, hypothetical or real.

We have updated our abstract, study 5 analyses, discussion of study 5, and conclusions to reflect this change in evidence. Ultimately, we feel that these null results make more sense and actually improve the quality and coherence of the paper. We thank you again for this suggestion.

Below you’ll find our comments to the original JESP review.

Again, we would like to thank you for your detailed and constructive feedback. We are confident that we have addressed all the issues raised in this round.

Sincerely,

Joe Hoover, Kate Johnson Grey, Reihane Boghrati, Jesse Graham & Morteza Dehghani

Morteza Dehghani, PhD
Assistant Professor
Psychology, Computer Science
University of Southern California
Dear Morteza Dehghani,

Thank you for submitting the revised version of your paper to Collabra: Psychology, "Moral Framing and Charitable Donation: Integrating exploratory social media analyses and confirmatory experimentation". I read the response letter and new version. I think this revision was thoughtful and responsive to feedback. Thus, I am happy to accept your submission for publication, pending the completion of copyediting and formatting processes.

As there are no further reviewer revisions to make, you do not have to complete any tasks at this point. The accepted submission will now undergo final copyediting. You will be contacted once this is complete to answer any queries that may have arisen during copyediting and to allow a final chance to edit the files prior to typesetting. If you wish to view your submission during this time, you can log in via the journal website.

Thank you for considering Collabra: Psychology as an outlet for your work. Please do keep us in mind for the future!

Sincerely,

Brent Donnellan
Michigan State University
donnel59@msu.edu