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Subject: Decision on submission to Journal of Experimental Social Psychology - Reject

Manuscript Number: JESP-D-20-00322

Self-interest is overestimated: Two successful pre-registered replications of Miller and Ratner (1998)

Dear Dr Feldman,

Thank you for submitting your manuscript to Journal of Experimental Social Psychology. I was fortunate to receive reviews from two experts in relevant fields. I of course read your manuscript independently, before reading their comments. Neither reviewer recommended the work be published in JESP, and my own reading concurs with theirs. As a result, the paper is rejected.

I support replication efforts, and believe there is a place for them in high-profile journals like JESP. However, their publication is subject to evaluation along criteria similar to those we would use for a paper reporting original studies. Of particular relevance to this decision, in both cases we must consider the level of the contribution made. In the case of your paper, I see two possible areas of contribution, one being the replication itself, and the other being the extension you propose.

In terms of the replication, I agree with both reviewers that you have conducted a pair of rigorous studies that go a long way toward demonstrating the direct replicability of the original Miller & Ratner paper. These data should be published somewhere, but the question I must answer is whether they should be published in JESP. As R2 points out, there appears to be little reason to question the Miller & Ratner findings; indeed subsequent papers report conceptually similar effects. You ask the question "Will the Miller & Ratner effects replicate?", which is always an empirical question, but in this specific case we have no particular theoretical or practical reason to doubt the replicability. In other words, within the range of degrees of confidence we might have about any given effect replicating, confidence in the Miller & Ratner paper is not one about which there would seem to be particular uncertainty. Your studies help reduce that uncertainty further, but more valuable replications are those that help eliminate an even greater degree of uncertainty.

Relatedly, I will note that I agree with R2's comment that your introduction lacks precision. You seem at times to be arguing that researchers and policy-makers have not yet understood the lesson of the Miller & Ratner paper, at other times to be arguing that the paper itself may have limited value since it examines hypothetical behavior, wherein people are likely to underestimate their own self-interest (Epley & Dunning), which suggests that maybe we are quite accurate in estimating its impact on ACTUAL behavior.

In terms of the extension you propose, I agree with both reviewers that it is of limited value, perhaps in part because it doesn't seem fully thought out. R1 challenges you to acknowledge that predicting more prosocial behavior does not have to equal more accuracy, if the more communal participants' estimates overshoot the sample's actual behavior. Picking up on that thread, I think you might want to consider it in light of your findings. You report, in Study 1, that more communal participants did in fact estimate greater prosociality in the sample. But then you report that more communal participants were not more accurate. But you do not really explain what you mean by accuracy—earlier in the results you presented accuracy separately for the two conditions (paid and unpaid), but in the extension section you report a single correlation. In the unpaid condition alone, it must mathematically be the case either that accuracy was higher for more communal participants, or that some non-trivial number of communal participants' estimates overshoot the sample's actual behavior.

In Study 4, it is not clear to me why you would have expected communal orientation to play a role. The influence of one's own status as a smoker may not reflect only self-interest, but rather a more collective interest of oneself and also all others who share one's status. As a non-smoker, if I object to smoking in restaurants, I do so not only for myself but for all my fellow non-smokers. So if a more versus less communal observer was going to make a guess, it's not clear why you'd expect the more communal person to think I'd be LESS opposed to smoking on planes.

The reviewers make many other very valuable points; I encourage you to consider them thoroughly as you continue to work on this manuscript.

We appreciate you submitting your manuscript to Journal of Experimental Social Psychology and thank you for giving us the opportunity to consider your work.

Kind regards,
Kristin Laurin
Associate Editor

Journal of Experimental Social Psychology

Editor and Reviewer comments:

Reviewer #1: The current manuscript sought to replicate Miller and Ratner's (1998) examination of self-interest overestimations through a pair of experimental studies. The authors sought to further extend the work by including an additional variable not considered in the original research (i.e. communalism), as well as a more detailed measure of self-interest in their second study. Overall, I found the manuscript to be a rigorous replication of the original work, though the attempts at extension seem more limited in their support and contribution. Please find some more detailed comments/questions below:

Introduction

1. On p.3, you note that "early conceptions of self-interest were too narrow for overlooking motivations such as being a good group member." Indeed, I wondered how the overall self-interest literature may be overly Westernized, lacking acknowledgment of collectivistic pro-group motivations as self-interest (e.g. an individual acting in favor of the group as it is in their benefit to cooperate given the norms of their society). Tying this into your investigation of communalism, I feel your front end could benefit from acknowledging the cultural implications of this investigation (though, I recognize your use of Western samples means you are limited in how much you can truly speak to this)- regardless, perhaps underscoring the need for diversified conceptualizations of self-interest and your initial attempt at unpacking that could increase your manuscript's overall contribution beyond the replication.

Study 1

2. Are you able to report racial demographic information for your samples?

3. Were no data quality checks used when collecting data from either Prolific or MTurk? See Chmielewski & Kucker (2020) or Kennedy, Clifford, Burleigh and colleagues (2020) for discussion about recent concerns in MTurk data quality specifically. I've heard fewer concerns about Prolific as compared to MTurk, but regardless would love more detail about any efforts to ensure sufficient participant attention levels, survey effort, and overall response quality.

4. I believe readers could benefit from greater explanation of reasoning regarding your final two extension hypotheses (H2E and H2F). While you suggest that communal individuals provide more accurate estimates of self-interest influences as they potentially overestimate self-interest's role to a lesser degree, is it also possible that communal individuals provide inaccurate estimates in the opposite direction (i.e. underestimate the role of self-interest in decision-making of others, believing that people are more motivated by altruism)? As this is the primary extension of this manuscript, I would encourage the authors to provide greater discussion of their rationale for readers to consider.

5. Were the MTurk and Prolific samples comparatively examined or evaluated in any manner prior to combination to ensure sufficient similarity to warrant grouping? I ask as the difference in paid vs. unpaid blood donations rates for self as listed in Table 4 appear to differ in magnitude between MTurk and Prolific samples- I appreciate you providing some presentation of results broken down across these two samples, but am not sure I

understand your reasoning for evaluating some hypotheses across samples and some using the combined sample. In fact, I'm not sure I understand the use of the combined sample at all, as both of your studies individually seem to have robust sample sizes. Greater description of these relevant decisions would benefit readers.

6. I find this notion of price sensitivity, or sensitivity to payment as predictive of over-estimations of self-interest effects interesting. In tying this to your exploratory age findings (i.e. that younger participants displaying greater self-interest), how does the current study inform self-interest as conceptualized in circumstances of need? Might you anticipate differences in self-interest overestimation across one's lifetime (within-person), or perhaps across SES levels (between-person)?

Study 4

7. Was there a reason the ordinal measure of smoking frequency only used in the MTurk sample, and not the Prolific sample?

8. How might you suggest future researchers operationalize self-interest given the results of your attempt at a more detailed conceptualization (perhaps to add in your discussion of these findings on p. 25-26)? Given the limited frequency of participants in certain categories of the five examined, might you suggest researchers seeking to continue building on this work revert back to the original measure used in Miller and Ratner (1998), or perhaps compromise with three or four groups to operationalize self-interest? More broadly, I'm trying to understand what was learned via this exercise to contribute to future research, and ensure that contribution is underscored within the manuscript.

Overall Discussion

9. I find the p. 25 explanation of non-effects on four of the eight policies of Study 4 self ratings to not be particularly compelling given your fairly large sample sizes- might there be other reasons for the lack of found effects?

10. Acknowledging your lack of support found for a relationship between communality and overestimation of self-interest, what other individual difference characteristics might you suspect relate to one's tendency to over- or underestimate? Further, I'm curious what you might expect had you utilized a more multi-cultural sample with greater representation of Eastern, collectivistic cultures- might your results have emerged as expected if tested in a sample with greater variance in communality?

11. What are the practical implications of a human tendency to overestimate self-interest? Though an interesting replication, I'm still left a bit with the "why does this matter" question. You do acknowledge that these beliefs "inform not just personal interactions but also public policies and institutions such as the criminal justice system (p. 2)," but I feel your current discussion of practical implications is fairly vague and could benefit from greater grounding, perhaps with concrete examples.

Overall, I found the study to be methodologically rigorous, though I wonder of the work's theoretical contribution beyond the replication. Though replication is a worthy endeavor in and of itself, I feel the authors have opportunity to either reflect on or contextualize their attempts at extension for greater contribution. I hope these recommendations are helpful, and I wish the authors best of luck as they continue their research!

Reviewer #2: The authors provide a replication of people's tendency to overestimate the power of self-interest in determining attitudes. The replications are almost identical to the original paper (Miller & Ratner, 1998) and replicate the original results.

I applaud the authors' thorough work in the replication effort and appreciate the importance of replications in psychological science in general.

Having said this, it is unclear why this replication will be of interest to JESP readers. As the authors note, results

consistent with Miller & Ratner's effect have been found in other papers since the original publication and there is no viable reason to doubt its replicability. Yes, the sample sizes were smaller in the original paper (probably because the cost per participant was higher back then and not because of "lower standards") than the current one and the original paper did not report relevant stats like SDs and variance. However, those facts alone do not justify publication of replication paper. Although replication is important in general, this concern must be balanced with sufficient scientific interest.

The addition of communal orientation/motivation does not represent sufficient scientific progress in my view over and above the original Miller & Ratner paper.

Moreover, in contrast to the meticulous methodology, I found the introduction and GD to be imprecise in various ways. These parts of the current paper are simply all over the place. The authors seem to oversell the importance of Miller & Ratner's original paper - yes, it's an interesting paper and I personally was inspired by it early in my career - but it's only one of many papers that challenged the influence of self-interest in determining attitudes and behaviors.

Moreover, the authors at times bring in citations that are irrelevant to the topic at hand. Among other examples, Epley & Dunning (2000) focus on identifying where people err when overestimating their own moral behaviors and Van Boven et al. (2018) simply note that people can misunderstand political outgroups' attitudes. Neither citation is related to the overestimation of the impact of self-interest.

There is one issue with the authors' methodology that should be explained further (which may have also existed in the original Miller & Ratner paper) - the option "no opinion" in measuring support for policies/donations should not be coded at 0.5 (where 0 = no; 1 = yes). "no opinion" can mean something in-between "yes" and "no" but it also can mean "I genuinely have no opinion" or "I have no information to make this decision." It's not clear that coding these responses as 0.5 is appropriate. I still think the authors will replicate the effects even after excluding the "no opinion" participants, but the analysis should be done both ways.

Finally, in noting the shortcomings of the original Miller & Ratner methodology, the authors should be more precise - they correctly note smaller sample sizes and lack of reporting of some statistics, but they also note "assuredly confirmatory analyses" (p. 6) - why do the authors think Miller & Ratner provided confirmatory analyses? The authors provide basically similar/identical analyses to those used in the original Miller & Ratner. I would delete this point or explain it further.

In sum, I believe that the introduction and parts of the GD must be more precise and clear, and more realistically place the Miller & Ratner paper in the context of its contribution to research on the influence of self-interest on attitudes.

The key difficulty I have in providing a positive recommendation is that while I respect replications I do not believe the current has sufficient scientific value for JESP. I am happy to be contradicted by the other reviewers and AE on this point - this is a matter of opinion on my part, after all.

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