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Evaluation of the dissertation “Psychological Specificity of Trust in Communal Sharing and Market Pricing Relationships” submitted by Maryam Kahn for the conferral of the PhD degree at SWPS University

The present dissertation is devoted to the question of how trust emerges in communal sharing and market pricing relationship contexts. This question is theoretically interesting as it bridges the literatures on relationship types, emotions and trust that span various disciplines, including psychology, sociology and economics. The dissertation presents three empirical chapters, two of which are already published in renowned peer-reviewed journals, as well as an introduction and a discussion / integration of the findings.

Chapter 1 is a not yet published meta-analysis of the effect of communal vs. market mindset on trust (measured with the trust game). The meta-analysis is motivated by the fact that past research has shown mixed findings on how communal and market mindsets influence trust. While communal sharing is thought to enhance trust, and market pricing to reduce it, experimental evidence has been inconsistent for both. The meta-analysis summarizes the effects from 58 studies (63 effect sizes, $N = 9,742$) and found that both communal and market cues reliably increase trust compared to control conditions. This chapter is very well-written, the selection of the research articles for the meta-analysis is well thought-through, the statistical analyses are meaningfully conducted, I especially appreciate the p -curve and z -curve analyses.

I have also identified the following areas of criticism or improvement. I wish the authors would give examples of most commonly used manipulations of communal vs. market cues in the trust game early on in the paper (e.g., in the introduction). That would have made it much easier to understand the context of the effects from the very beginning. Also, it would be helpful if one of the tables that list the included studies would also give the information about the type of the manipulation they used. Relatedly, I wish there was a more detailed presentation of the different ways the relational vs. market price cues were operationalized the meta-analyzed studies in the Method section.

Conceptually, I was wondering whether the manipulations used for communal and market contexts are too heterogeneous to be analyzed together. E.g., they include basic priming/framing effects (pictograms), as well as outgroup/ingroup effects. These constructs appear theoretically distinct and may involve different underlying mechanisms, so I wondered whether it is appropriate to group such varied studies together under the single framework of communal versus market relational cues. Another observation is that the ingroup/outgroup effects can also depend on the nature of these groups. To give an example, religious targets are often trusted more even by atheists (for whom they are the outgroup: Gervais, W. M., Shariff, A. F., & Norenzayan, A. (2011). Do you believe in atheists? Distrust is central to anti-atheist prejudice. *Journal of personality and social psychology*, 101(6), 1189).

Another aspect that I see somewhat critically is the focus on trust as measured by the trust game only. Trust game is certainly a well-known and widely used instrument to measure trust. At the same time, it

lacks ecological validity, represents a one-shot interaction and shows limited correlations with self-reports of trust. Trust can also represent an attitude/personality trait and I wonder whether communal vs. market pricing cues would show similar effects if trust was operationalized differently. After all, trust game does represent an economic transaction (it is even sometimes called investment game) and it's therefore not surprising that market pricing cues have a positive effect in this context. Would adding market pricing cues positively affect trust as an attitude or a worldview as well?

I also found it a little disappointing that while the paper set out to find an explanation (i.e. moderator) to the mixed findings in prior research, no significant study-level moderators were detected. I was wondering whether a more detailed coding of the study characteristics would help. E.g., for the market mindset cues, a dummy coding was used with the pictorial primes ($k=3$) vs. all other manipulations ($k=8$) pulled together. What were these other manipulations and are they conceptually similar to each other to justify this pulling? Is it possible that they are heterogeneous enough to explain why the effect is sometimes positive, sometimes negative and sometimes zero?

I'm not sure I'm convinced that the effect of communal cues can be explained by the fact that these cues activate the caring motivation and genuine benevolence, rather than strategic calculation (p. 29). If the most common manipulation of communal cues is whether the trustee was ingroup or outgroup, trust could very well be based on strategic calculation, as participants might expect their ingroup to be more likely to reciprocate, resulting in higher earning for them.

Finally, I'm wondering whether trust driven by communal vs. market cues might have different consequences. Is the former more stable and durable than the latter? These questions could be worth pursuing in the future research.

Chapter 2 presents a systematic review of the experimental studies on relational mindset and emotions on trust. Based on PRISMA guidelines, the authors identified 31 peer-reviewed studies published between 2004 and 2021. The paper fits well with the overall topic of the dissertation. It includes a review of a large body of literature. I do however see this chapter as the weakest in the overall dissertation, for the following reasons.

The literature review lacks coherence and clarity, combining both relevant and irrelevant information in a way that makes it difficult to follow. More importantly, it does not sufficiently justify the integration of two distinct strands of research, namely, the effects of emotions on trust and the influence of relational mindsets on trust. At a theoretical level, it remains unclear why these domains are brought together within a single framework. In particular, the review would benefit from a more explicit integrative research question or conceptual model that explains the rationale for examining relationships and emotions in conjunction.

The literature review also seems to miss some points. E.g., on p.2 it says that trust is a state and not a trait. This is a very narrow view that contradicts a large body of research in personality psychology that sees trust as a trait. On p. 3, the manuscript cites studies suggesting that expressions of anger in bargaining signal weakness. However, it overlooks a body of research indicating the opposite, namely that anger can convey power and signal a willingness to walk away from the negotiation (Van Kleef, G. A., De Dreu, C. K., Pietroni, D., & Manstead, A. S. (2006). Power and emotion in negotiation: Power moderates the interpersonal effects of anger and happiness on concession making. *European Journal of Social Psychology*, 36(4), 557-581).

Figure 1 is intended to clarify the relationships among the key constructs, yet it ultimately adds confusion rather than insight. For instance, the arrow from emotions to relationship types is ambiguous: does it represent a causal effect, a correlation, or a moderating influence? Similarly, the meaning of the three arrows at the bottom is not explained, leaving their interpretation unclear. Greater precision in labeling and conceptualization would substantially improve the figure's explanatory value. Regarding exclusion criterion 4, it appears that only studies examining changes in trust were included. It is not entirely clear, however, whether this implies a restriction to pre-post designs. If so, the rationale for excluding studies that assess trust as a dependent variable without measuring change should be more explicitly justified.

I think that some of the text in the results belongs to the methods, specifically the description of how manipulations of emotions were coded (p. 7). Further, some coding decisions were unclear: e.g., why does "facial attractiveness / emotional expression" belong to the communal context? Facial attractiveness can be manipulated in work context also (and was shown to affect hiring decisions). Given the lack of clarity regarding the added value of integrating relational context and emotions within a single review of trust, it

remains uncertain what this paper contributes beyond the meta-analysis presented in Chapter 1, which not only synthesizes but also quantitatively assesses the effects of communal and market mindsets. Finally, I couldn't help but notice a substantial number of typos and instances of awkward phrasing (e.g., there is a typo even in the abstract with Fiske name lacking an "e" at the end).

Chapter 3 is, in my view, the strongest in the dissertation. Across six experiments in multiple countries ($N = 4086$), the authors examined how subtle monitoring affects trust in communal sharing (CS) versus market pricing (MP) relationships, among both trustors and trustees. Monitoring was perceived as more acceptable in market contexts and adding monitoring in the communal context shifted the perception of the relationship to be more market oriented. From the trustee's perspective, monitoring reduced trust in both contexts, while from the trustor's perspective it only reduced trust in communal settings – which is a very interesting finding. The paper is very well-written, it presents a nice theoretical contribution by challenging the idea that control always undermines trust, showing that its effects vary depending on relationship type. The studies are nicely designed and executed and encompass samples from various countries. I especially liked the way the Cheat Talk game was thoughtfully adapted to suit the aims of this investigation. Further strengths of the paper include the open science approach and consistent power analysis, a comprehensive analysis of the limitations and a thoughtful discussion.

As a potential point of criticism, I would note the absence of a discussion addressing whether the findings might be explained by differing social norms across relationship types and these norms' origins. Additionally, the fact that approximately one third of participants in Study 1 failed the attention check is concerning, as it raises the possibility of a highly selective sample. I also found the label "trust from the trustee perspective" somewhat misleading: upon first encountering it, I interpreted it as referring to trustworthiness, whereas it in fact denoted the extent to which trustees believe they are trusted by the trustor. I would recommend more precise labels, such as "self-reported trust" (trustor perspective) and "perceived trust" (trustee perspective), or "trusting" versus "feeling trusted". Finally, the justification for manipulating initial levels of trust, as well as for the expected interaction between initial trust and relationship type, struck me as somewhat underdeveloped.

Among the unresolved questions I was wondering whether removing monitoring might also render the perception of work relationships more communal, whether monitoring can restore trust in market pricing relationships after betrayal and how to close the gap between trustor and trustee perception of the acceptability of monitoring in MP contexts (questions for future research).

In sum, the submitted work represents a meaningful contribution to the literature on trust and relationship types. Based on the CRediT statements, I evaluate Maryam Khan's individual contribution as sufficient and conclude that Maryam Khan has demonstrated the requisite competencies to conduct independent scientific research. Therefore, I support her progression to the next stages of the doctoral degree conferral process.

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