

General comments on the interest of discussing Casciaro, Gino, and Kouchaki's paper (2014)

Many of your comments concern the first chapter of my dissertation, and more specifically the discussion of Casciaro, Gino, and Kouchaki's paper (2014). In particular, you have asked about the relevance of featuring this discussion, and question if I could not present my own investigation and conclusion without referring to CGK. I believe that this point warrants a longer discussion than other elements of feedback.

First, I had already received feedback from ED and IN in the Spring when they reviewed this first chapter. Following their comments, I made significant changes to this discussion. I clarified the concept of "moral purity" on which CGK was built, removed paragraphs that I considered to be irrelevant or confusing, and added others to clarify the contribution of this discussion.

It appears that, despite those changes, the contribution of this section might remain unclear to the reader. I of course understand that it is unusual for a dissertation (or a paper) to feature a direct criticism of a published paper. However, I believe that this discussion is important, and that the full contribution of my chapter can only be understood in the context of this discussion. I would like to clarify this position, and explain why I believe that this discussion presents a valuable contribution that should appear in my dissertation.

First, some background information on this chapter:

My interest in CGK started in my second year. At the time, I found the finding provocative and fascinating and wanted to extend the contribution. However, despite multiple attempts in different studies, I have never been able to extend CGK's findings. Those multiple failures encouraged me to conduct a systematic investigation of the theoretical premises on which CGK relies, to have a critical look at their methodology, and more generally to learn more about the research practices that prevailed at the time, and how they have changed since what Nelson, Simmons, and Simonsohn (2018) call Psychology's Renaissance. The discussion that I present in my first chapter reflects this intellectual journey.

Now, which purpose does this discussion serve?

First, the contribution of my paper can only be understood in light of this discussion. Indeed, if CGK's conclusion (that people feel "dirty" when networking) had been supported by sound theoretical and methodological foundations, the next scientific step would be to explore boundary conditions and extensions of their effect. This was my original goal when I started working on this chapter. However, my discussion shows that CGK is built upon a theoretical premise for which there is no empirical evidence (i.e., the Macbeth effect), and suffers from multiple conceptual and methodological flaws. As such, I was unable to extend their paper, and had to start from scratch. This explains why I propose another framework (understanding the cognitive frame and moral emotions associated with networking), and why I used this framework to examine people's discomfort when networking. To sum up, the discussion of CGK is what motivates my decision to "start from scratch," and a reader would not understand this decision if they were not aware of the problems in CGK.

Second, I believe that featuring this discussion sets a positive example. Through discussions with colleagues in other fields and at other universities, I was surprised to discover that the body of scientific knowledge in academia functions as a gated system: Some people have insider information about which papers are trustworthy and replicate, and other people are left in the dark, completely unaware that some published findings cannot be trusted. I for instance learned that graduate students at some

North American institutions (e.g., Wharton, Booth, UCLA) are advised not to work on certain topics, or are instructed to systematically replicate the main effect of a paper before trying to extend it.

I believe that this gated environment is in part a consequence of the fact that published papers are rarely openly criticized. Instead, the flaws of published papers are discussed behind closed doors, in journal clubs, or on anonymous forums, but rarely elsewhere. It does not have to be the case: If more journals published failures to replicate, and more people openly reported the flaws that they have discovered while surveying the literature, fewer people would waste time, money, and energy working on theoretical and empirical dead-ends. I also believe that discussing the flaws of paper in the open (rather than privately) would encourage researchers to ground their criticism in scientific (i.e., objective and verifiable) facts, and would contribute to a healthier climate.

For all those reasons, I believe that criticisms of published findings should be normalized: It is often said that science is “self-correcting,” but it is only true if past findings can be openly discussed and criticized. Multiple scholars have argued that science works best when researchers can account for the things that they discover, both positive and negative (Munafò et al., 2017). I believe that including this discussion in my dissertation is a small step toward this goal.

Third, my experience as a graduate student has given me a first-hand experience of the costs of unreliable findings. As I have written earlier, I have spent a year and a half of my PhD (and a significant amount of money from my advisors’ budget) trying to extend a paper that was built on shaky grounds. At the time, my limited experience did not allow me to consider that even the best scientists make mistakes and that even papers published in top management journal might report false positives. In fact, the issues in CGK themselves might be an illustration of the damage that false-positive findings exert on the literature. As I remind in my dissertation, the paper on the “Macbeth effect” (Zhong & Liljenquist, 2006), from which CGK borrows theoretical constructs and methods, has failed to replicate several times, and a p-curve analysis of the original paper shows no evidence in favor of H1 (Simonsohn, Nelson, & Simmons, 2014a, 2014b). For those reasons, I think that it is important to correct the scientific record by highlighting the flaws in CGK: If this discussion can prevent a future graduate student from making the same mistake as I made, then it will be a valuable contribution in my view.

The flaws that I point in CGK are serious. As such, I realize that my tone might appear harsh at times. To avoid being unfair to the authors, and to ensure that my discussion follows the rules of “civil criticism” in academia (“[52] Menschplaining,” 2016), I was particularly careful when writing this section. I have made a conscious effort to ground my criticism in objective, verifiable facts, and to avoid any speculation on the motives, intentions, or goals of the authors. I have also strived to establish a strict separation between my criticism of the paper and the opinion that I have of the authors and their other work. For instance, IN suggested that I replace all instances of “CGK” by “Casciaro et al.”: I believe that this change would be counterproductive, and that writing “CGK” establishes a stronger distinction between the elements of the paper and the authors. Finally, I have carefully pondered all the other comments that ED and IN made on earlier version of the chapter, the new comments that you have formulated, and have made significant edits to this section in response to them. I will send you an updated version of this section after the defense: If you believe that there are elements of my criticism that would lack diligence, that would be insufficiently supported by facts, or that would be unfair to the authors, please do not hesitate to point them out to me, and I will happily change the dissertation accordingly.

Ultimately, I think that the following thought experiment might be helpful to communicate the value of this discussion. If CGK were resubmitted to ASQ tomorrow, and if the issues that I discuss were

pointed out during the peer-review process, would it change your opinion of the paper? If my discussion would change someone's overall assessment of the paper, and would have an impact in a peer-review process, then I think that it is a valuable discussion. Through this discussion, each reader can determine whether it is worth the effort and resources to continue studying networking discomfort in the framework proposed in CGK or whether using another framework would be justified.

Finally, I wanted to further clarify my intentions if there are still doubts about them. I have no qualms against the authors, quite the opposite. I had the chance to meet Tiziana Casciaro several times and have the highest opinion of her and of her research. There is nothing personal in my criticism: My only goals are to raise awareness about methodological and theoretical issues, motivate the contribution of my first chapter, and normalize the criticism of published papers. I hope that this (rather lengthy) response will have helped you see this perspective, and will have made it clearer why it is so important for me that this discussion appears in my dissertation.

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