

Our Conclusions Were Tentative, But Appropriate: A Reply to Schnall et al. (2015)

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In our original meta-analysis (Landy & Goodwin, 2015, this issue), we demonstrated that the amplification effect of incidental disgust on moral condemnation is very small at best, $d = .11$, and may not exist at all—the effect disappears entirely when publication bias is accounted for, and existing studies may overestimate the true effect size owing to prevalent confounds. In their reply, Schnall, Haidt, Clore, and Jordan (2015, this issue) expressed surprise at the nature of our conclusions and argued that our meta-analysis supported most of what they demonstrated in 2008. They have taken issue with three aspects of our meta-analysis: that we did not include personality variables as moderators, that we minimized the importance of the relatively large effect of incidental gustatory/olfactory disgust on moral judgment, and that we included studies in which participants may have been able to correctly attribute their disgust to its true source. We directly addressed the first concern in our original article, but we reiterate and expand upon that response here and reply to the second and third concerns.

First, Schnall, Haidt, Clore, and Jordan (2015) were particularly concerned with our failure to include personality variables, such as private body consciousness (PBC), as moderators. As we pointed out in the original article, we did not include such variables as moderators because so few studies have included them that it would have been uninformative to meta-analyze them. In the case of PBC, only a handful of studies have included this individual difference measure (Baron, Rozman, & Goodwin, 2013; Cerban & Helweg-Larsen, 2010; Johnson, Cheung, & Donnellan, 2014; O'Connor & Helweg-Larsen, 2011; Schnall, Haidt, Clore, & Jordan, 2008). If more of the studies that we reviewed had included PBC, or anything else, then we would have included these variables in our meta-analysis. Schnall et al. extended this criticism by querying “why a meta-analysis was conducted on data for which essential variables had not even been assessed” (p. 537). In raising this query, Schnall et al. seem to be presenting social intuitionist theory as fundamentally including the assertion that the amplification effect is moderated by PBC, such that it only occurs for those

high in PBC. Yet, this is not how the theory has typically been presented in the literature, including by Schnall et al. themselves. In their 2008 paper, Schnall et al. found that PBC moderated the amplification effect in three of their four studies, yet they framed their paper by claiming that “for most people, most of the time, most of the action [in determining moral judgments] is in the quick, automatic, affective evaluations they make of people and events” (p. 1097). And they concluded by asserting that the amplification effect occurs “most strongly” for people high in PBC (p. 1105), not that it does not occur at all for those low in PBC. Now, however, they seem to be saying something closer to “for some people, at least some of the action is in the quick, automatic, affective evaluations they make of people and events, but this is not the case for other people.” This strikes us as a quite different—and much weaker—claim than previous statements of the social intuitionist theory. This may prove to be an appropriate revision of the theory. But, it is not the theory that has so strongly shaped the existing literature. Our meta-analysis clarifies the state of the evidence regarding social intuitionism’s most fundamental theoretical claim.

Schnall et al. noted that we did find a robust, relatively large amplification effect for studies using gustatory or olfactory disgust inductions, $d = .37$, and claimed that we minimized this finding. We did minimize this finding, but there is a good reason for doing so. This result cannot provide clear evidence for social intuitionism because confounding variables beyond disgust are particularly problematic for these types of inductions. Participants in these studies are typically exposed to noxious odors, or—in two studies—made to drink a disgusting, bitter liquid. In each case, the experimenter’s behavior is capable of provoking considerable offense, and even anger—in the first case, the experimenter has failed in his duty to keep the lab area clean and habitable, and in the latter,

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he is forcing a very aversive experience on participants. Such actions may seem disrespectful and offensive. The offense they provoke may well prime a condemnatory mindset, or simply anger the participants, which could potentially explain any observed amplification effect separate from bodily disgust. Thus, there is some evidence that gustatory/olfactory disgust inductions produce more extreme moral condemnation, but these inductions are especially hard to interpret because it is unclear whether induced disgust itself is the proximal cause of this effect.

Finally, Schnall et al. argued that the amplification effect should only be observed when disgust does not become an object of focus prior to making a moral judgment. Indeed, Schnall et al. have previously argued that disgust inductions can be “too potent” and too obvious and thus cause participants to correctly attribute their disgust to the induction (see Footnote 2 of our meta-analysis for discussion of this argument). In their commentary, they made a slightly different point, namely that certain experimental procedures might lead participants to focus on their own emotions or bodily states before the judgment task, thus eliminating the misattribution process that is critical to the amplification effect. This is an interesting suggestion, but it strikes us as being at odds with their first point, in which they argued that a dispositional tendency to attend to bodily states is required to produce the amplification effect. In their theory, attending to one’s bodily states seemingly makes it both more and less likely that one will show the amplification effect. How, exactly, these two aspects of their theory can be reconciled is not clear to us.

Nonetheless, we reanalyzed our data in light of this point. Specifically, we ran an additional moderator analysis comparing studies that had any form of emotion measure that could draw participants’ attention to their emotional states after the disgust induction but prior to the moral judgment task ($k = 10$) with studies that did not have this feature ($k = 40$). And, consistent with Schnall et al.’s third argument, studies with such emotion measures showed no amplification effect whatsoever, $d = -.06$, $p = .39$, whereas studies that did not draw participants’ attention to their emotions showed a significantly larger effect, $d = .18$, $p < .001$, $Q(1, 48) = 8.08$, $p = .004$. This effect is still quite small overall (equivalent to a correlation of $r = .09$, and explaining only 0.8% of the variance in participants’ moral judgments), and the difference in the effect across these studies may not solely be attributable to this single methodological difference. Moreover, this analysis cannot account for publication bias, so the estimated effect sizes may be larger than the true effect sizes. In the future, it might be profitable to manipulate whether participants focus on their emotional states or not to see if this does in fact moderate the amplification effect (see Cameron, Payne, & Doris, 2013 and Van Dillen, van der Wal, & van den Bos, 2012, for somewhat similar procedures).

We appreciate Schnall et al.’s commentary, and we think their last suggestion in particular warrants further attention. But we stand by the original conclusions of our meta-analysis: The (unmoderated) amplification effect is, at most, very small—so small, in fact, that no existing study should have been able to detect it. It disappears entirely when accounting for publication bias. Gustatory/olfactory disgust inductions produce a robust and relatively large amplification effect, but this could very well be due to confounding factors. These conclusions, like any, are tentative, and could change in light of new data or clearer theorizing, but for now, we see no reason to revise them.

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Declaration of Conflicting Interests

The authors declared that they had no conflicts of interest with respect to their authorship or the publication of this article.

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