

Reply to Chernev, Böckenholt, & Goodman (2010), Choice overload: Is There Anything to It?

In the October 2010 issue of the *Journal of Consumer Research* (JCR), we published a review and meta-analysis of the effect of large assortment sizes entitled "Can There Ever Be Too Many Options? A Meta-Analytic Review of Choice Overload." Chernev, Böckenholt, and Goodman (hereafter, CBG) published a comment on our paper in the same issue, entitled "Choice overload: Is There Anything to It?" We welcome the scientific discussion spurred by our paper, which was our intention in writing it. In an effort to advance science with constructive contributions and new insights, in what follows we will reply to CBG's comment by summarizing their major points and revisiting our original article in light of these points. (We were not given the opportunity to submit a reply to CBG's comment in JCR.)

CBG criticize our meta-analysis of choice overload by arguing that: (1) the analysis of an overall mean effect size is not useful, and compares apples with oranges; (2) the search for sufficient conditions to generate choice overload is inadequate; (3) the absence of a negative effect of increasing assortment size does not contradict the choice overload hypothesis. Note that these are the main points highlighted by CBG, and not the major findings of our original paper.

Inclusion criteria for the meta-analysis

In their comment, CBG maintain that investigating an overall mean effect is not informative when there are moderators at work. Implicit in this statement and the example they provide on articulated ideal points is the assumption that different levels of the moderator cancel each other out, thus comparing "apples with oranges." There are a few experiments included in our meta-analysis where researchers tested particular moderators like prior preferences, time pressure, or option complexity – in our original publication we are explicit about these conditions and itemize them in a detailed review. However, most contributions we reviewed actually did test a main effect of assortment size and tried to establish its presence across various contexts, thus, contradicting CBG's claim that studies "typically aim to identify conditions under which effects of these constructs are likely to occur, rather than to simply establish its presence" (p. 426). We will come back to this aspect in detail below.

We originally ran our meta-analysis without those conditions where people had prior preferences or made choices under time pressure. But the reviewers (among them one of the authors of the CBG comment) explicitly requested to include these conditions, specifically those in the experiments by Chernev (2003) that CBG find problematic for our analysis. The reviewers' argument was that an extended inclusion criteria provides a better overview of the effect as a whole and that it helps in identifying possible moderators by means of meta-regression and a detailed review of single studies—we agreed, and so extended the data we analyzed. (We also include the original analysis below.)

The researchers' intentions as a selection criterion. Given the wide-spread interest in the negative effect of choice overload, it is important to ask whether or not there is a tendency for this effect to appear overall, across all the studies available. Therefore, a criterion that includes more rather than less data in the meta-analysis seems justified. In contrast to this, CBG seem to suggest that the selection of studies should be constrained by considering only those where the aim or intention of the researcher who conducted a study was to demonstrate the choice overload effect. This idea seems problematic though because one typically does not know the intentions an experimenter had prior to conducting a study (Kruschke, 2010). Thus, with the criterion “experimenters' intentions” comes the risk of cherry-picking only those studies that found an effect and dismissing null or contrary findings from the meta-analysis. Ultimately, this weakens the effectiveness of meta-analysis. Furthermore, from an epistemological perspective, the results of a study should not be affected by the aim or the intention of the researcher, and researchers typically go to great lengths to prevent this from happening (e.g. by using randomization or double-blind designs). Finally, a number of researchers explicitly *aimed* to find an effect of choice overload but nevertheless failed to do so (e.g., Fasolo, Carmeci, & Misuraca, 2009; Gingras, 2003; Scheibehenne, Greifeneder, & Todd, 2008).

Re-analysis of the data. We have run numerous additional analyses that address CBG's call for different inclusion criteria and found that the results did not appear to depend on the inclusion or exclusion of small sets of specific studies. When using a more strict inclusion criterion matching CBG's concerns above by only analyzing studies where people had no prior

preferences or expertise and made choices without time pressure, the mean effect size is $D = 0.04$ ($N = 56$) – indicating that still no effect of choice overload is present in those studies.

To further quantify how much the mean effect size depends on the inclusion or exclusion of particular conditions, we excluded five more data points that some critical readers may feel do not fit with the rest of the studies: the "buy-select" condition by Gao and Simonson (2008), the two experiments with a large number of attributes by Greifeneder, Scheibehenne, and Kleber (2010), and the three "organized" conditions in Kahn & Wansink (2004). The mean effect size of the remaining 51 conditions is $D = 0.07$ – still no evidence for choice overload. In yet other analyses, we excluded all studies that we conducted ourselves (yielding $D = 0.03$, $N = 47$) and our own studies plus all studies with subject preferences, expertise and time pressure ($D = 0.14$, $N = 37$, the 95% confidence interval still includes zero), which did not yield an effect of choice overload either.

The robustness of this mean effect size is likely driven by the fact that each single study carries relatively little weight itself (see Figure 1 in the original publication for a list of all study weights). It therefore appears that the results of these analyses do not appear to depend on the inclusion or exclusion of small sets of specific studies.

Note that all the necessary information we used from the original studies in our meta-analysis are included in our original paper. Researchers who feel that a particular condition should or should not be included in the meta-analysis may thus create any particular theoretically-driven subset of the studies and check it for an effect of choice overload.

Expertise as a moderator. To account for potential moderator variables in our original analysis, we explicitly coded them in the meta-regression. In particular, we coded studies testing for expertise or prior preferences to quantify their impact on the overall effect size. Besides the experiments by Chernev (2003), this includes an experiment by Gingras (2003) and an experiment by Mogilner, Rudnick and Iyengar (2008). Our meta-regression indicates that people with prior preferences benefit from larger assortments. Can one deduce the reverse though? Did past research show that "choice overload reliably occurred for respondents without established preferences" (CBG, p. 427, italics added), as CBG conclude? Logically, denying the antecedent does not allow for valid deductions of the reverse condition—so it is essentially an empirical question whether the lack of prior preferences reliably leads to choice overload. And a definitive

answer to this question requires meta-analytic techniques to consider pooled results, rather than relying on individual studies. Checking for appropriate studies to analyze, we found that in 58 of the 63 data points listed in our original paper participants had no strong prior preferences or expertise. As we state in our original paper, that is because "experiments on choice overload have typically used options that decision makers are not very familiar with." (p. 410, see also Iyengar & Lepper, 2000 for a similar argument). So, looking across all these studies, does choice overload reliably occur for respondents without established preferences? The meta-regression indicates that the answer is no. Having prior preferences leads to a more-is-better effect, but the opposite is not true: Having no prior preferences does not reliably lead to choice overload.

Necessary vs. sufficient conditions

CBG further criticize our original publication by stating that the search for sufficient conditions to generate choice overload is inadequate. However, their statement that "the search for elusive sufficient factors should not overlook important moderators that produce significant outcomes under an identifiable set of conditions" (CBG, p. 427) agrees with our stance as indicated when we state that it "is certainly possible, however, that choice overload does reliably occur depending on particular moderator variables, and researchers may profitably continue to search for such moderators" (p. 421). Towards this goal, in our original paper, we point out a number of possible moderators and pre-conditions that may turn out to reliably elicit choice overload and ways of exploring them in future research. Our review of the existing literature allows readers to form their own informed opinion about how well an effect of choice overload actually qualifies as a "given phenomenon" that is "robust" (CBG, p. 427) .

But CBG felt that the selection of moderators explored in our paper were largely uninteresting "inconsequential antecedents" (CBG, p. 427). As we pointed out, "there are other potential moderators of choice overload that would be worthwhile to compare across studies, [but] meta-analytic methods require that such variables be measured (or can at least be coded) in more than one study" (p. 417). Two issues are noteworthy here: First, the particular value of meta-analysis comes from integrating many studies—moderators that are only available for a few studies cannot be convincingly treated. To still be able to consider those less-studied moderators, we "assessed those specific moderators that only appeared in single studies by means of a

qualitative review following the meta-analysis” (p. 417). Second, moderators such as “geographic location” are not inconsequential as suggested by CBG, given that they correlate with culture, which has been shown to have strong effects on choice, judgment, and decisions, and has been emphasized by other researchers in the realm of choice overload (Markus & Schwartz, 2010).

The choice overload hypothesis

In their comment, CBG claim that even if no negative effect of assortment size can be established meta-analytically, such an “absence of a monotonic (linear or curvilinear) effect is not contradictory to the choice overload hypothesis” (p. 427). While CBG do not spell out what exact hypothesis their claim refers to, in our paper we provide an explicit definition of choice overload as “adverse consequences due to an increase in the number to choose from” (p. 409) – a definition that is based on a thorough review of the choice overload literature. Defined this way, our results do indeed contradict the choice overload hypothesis.

In light of our results and possible future findings in this area, qualifying the original definition of choice overload by adding specific boundary conditions may be justified. It is important that such ancillary conditions are explicit though because otherwise there is a risk that the choice overload hypothesis becomes a moving goalpost that will be immune against empirical testing. But there is also the risk that constraining the scope of the choice overload hypothesis by adding “a number of intervening factors” (CBG, p. 427) will eventually dilute the significance of the effect as a general and widespread phenomenon.

Conclusion

Based on a comprehensive meta-analysis and a further qualitative review we conclude in our original paper that the empirical evidence to date does not reveal that an increase in the number of options reliably leads to a decrease in choice satisfaction, preference, or motivation. Instead, choice overload is probably restricted to rather particular circumstances and pre-conditions that are not yet fully understood. Our meta-analysis further indicates that researchers have trouble finding the effect even when attempting to replicate previous studies. In support of this conclusion, a number of other studies that we have recently learned of have also had trouble

finding the effect even though they apparently aimed (or at least expected) to find it (Arunachalam et al. 2009; Bundorf & Szrek, 2010; Hafenbraedl & Hoffrage, 2009).

Explicitly going beyond just “interpreting the nonsignificant overall mean effect size” (CBG, p. 426), our original publication identified a number of potentially important preconditions for choice overload as well as several promising directions worth exploring in future research. Against this background, overlooking the balanced picture presented in our original publication and reducing it to an argument over a single mean effect size may come at the risk of creating a straw man that hinders fruitful discussion and scientific progress. As we conclude in our original publication, “[i]t is certainly possible, however, that choice overload does reliably occur depending on particular moderator variables and researchers may profitably continue to search for such moderators” (p. 421). To move the field along toward this goal, it is paramount to continue with constructive discussions and investigations and to focus on empirical evidence.

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