

# Can There Ever Be Too Many Options? A Meta-Analytic Review of Choice Overload

BENJAMIN SCHEIBEHENNE  
RAINER GREIFENEDER  
PETER M. TODD

The choice overload hypothesis states that an increase in the number of options to choose from may lead to adverse consequences such as a decrease in the motivation to choose or the satisfaction with the finally chosen option. A number of studies found strong instances of choice overload in the lab and in the field, but others found no such effects or found that more choices may instead facilitate choice and increase satisfaction. In a meta-analysis of 63 conditions from 50 published and unpublished experiments ( $N = 5,036$ ), we found a mean effect size of virtually zero but considerable variance between studies. While further analyses indicated several potentially important preconditions for choice overload, no sufficient conditions could be identified. However, some idiosyncratic moderators proposed in single studies may still explain when and why choice overload reliably occurs; we review these studies and identify possible directions for future research.

In today's market democracies, people face an ever-increasing number of options to choose from across many domains, including careers, places to live, holiday destinations, and a seemingly infinite number of consumer products. While individuals may often be attracted by this variety, it has been suggested that an overabundance of options to choose from may sometimes lead to adverse consequences. These proposed effects of extensive assortments include a decrease in the motivation to choose, to commit to a choice, or to make any choice at all (Iyengar, Huberman, and Jiang 2004; Iyengar and Lepper 2000); a decrease in preference strength and satisfaction with the chosen option (Chernev 2003b; Iyengar and Lepper 2000); and an increase in negative emotions, including disappointment and regret

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Benjamin Scheibehenne is a research scientist at the Department for Economic Psychology, University of Basel, 4055 Basel, Switzerland (benjamin.scheibehenne@unibas.ch). Rainer Greifeneder is a research scientist at the University of Mannheim, 68131 Mannheim, Germany (greifeneder@uni-mannheim.de). Peter M. Todd is professor of cognitive science, informatics, and psychology at Indiana University, Bloomington, IN 47405 (pmtodd@indiana.edu). The present article is based on the first author's dissertation conducted at the Max Planck Institute for Human Development, Center for Adaptive Behavior and Cognition, Berlin, Germany. The authors are grateful for the constructive and valuable input they received from the editor, the associate editor, and the reviewers. In addition, the authors thank Wolfgang Viechtbauer and the Indiana Statistical Consulting Center for their support in conducting the meta-analysis.

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(Schwartz 2000). These phenomena have been selectively referred to as "choice overload" (Diehl and Poynor 2007; Iyengar and Lepper 2000; Mogilner, Rudnick, and Iyengar 2008), "overchoice effect" (Gourville and Soman 2005), "the problem of too much choice" (Fasolo, McClelland, and Todd 2007), "the tyranny of choice" (Schwartz 2000), or "too-much-choice effect" (Scheibehenne, Greifeneder, and Todd 2009); an increasing number of products to choose from is sometimes termed "consumer hyperchoice" (Mick, Broniarczyk, and Haidt 2004). Common to all these accounts is the notion of adverse consequences due to an increase in the number of options to choose from. Following the nomenclature in the literature, we refer to this common ground as the "choice overload hypothesis."

The choice overload hypothesis has important practical and theoretical implications. From a theoretical perspective, it challenges most choice models in psychology and economics according to which expanding a choice set cannot make decision makers worse off, and it violates the regularity axiom, a cornerstone of classical choice theory (Arrow 1963; Rieskamp, Busemeyer, and Mellers 2006; Savage 1954). From an applied perspective, a reliable decrease in satisfaction or motivation due to having too much choice would require marketers and public policy makers to rethink their practice of providing ever-increasing assortments to choose from because they could possibly boost their success by offering less. Wide proliferations of choice have also been discussed as a possible source for declines in personal well-being in market democracies (Lane 2000).

Given these implications, it is important to further un-

derstand the conditions under which adverse effects of choice overload are likely to occur. Therefore, in this article we aim to thoroughly reexamine the choice overload hypothesis on empirical and theoretical grounds. Toward this goal, we present a meta-analysis across all experiments we could find that investigated choice overload or provide data that can be used to assess it. This meta-analysis reveals to what extent choice overload is a reliable phenomenon and how much its occurrence depends on specific moderator variables. But first, we provide a brief summary of past research on choice overload and its underlying theoretical foundations, considering its proposed preconditions, what exactly constitutes “too much” choice, and arguments for and against the hypothesis that too much choice causes adverse consequences.

### PAST RESEARCH ON CHOICE OVERLOAD

The idea of choice overload can be traced back to the French philosopher Jean Buridan (1300–1358), who theorized that an organism faced with the choice of two equally tempting options, such as a donkey between two piles of hay, would delay the choice; this is sometimes referred to as the problem of “Buridan’s ass” (Zupko 2003). In the twentieth century, Miller (1944) reported early experimental evidence that relinquishing an attractive option to obtain another (a situation he referred to as “double approach-avoidance competition”) may lead to procrastination and conflict. The idea was further developed by Lewin (1951) and Festinger (1957), who proposed that choices among attractive but mutually exclusive alternatives lead to more conflict as the options become more similar. In his theory of attractive stimulus overload in affluent industrial societies, Lipowski (1970) extended this idea by proposing that choice conflict further increases with the number of options, which in turn leads to confusion, anxiety, and an inability to choose.

More recently, a series of experiments by Iyengar and Lepper (2000) marked the return of interest in possible negative consequences due to having too much choice. In their first study, Iyengar and Lepper set up a tasting table with exotic jams at the entrance of an upscale grocery store. The table displayed either a small assortment containing six jams or a large assortment of 24 jams. Every consumer who approached the table received a coupon to get \$1 off the purchase of any jam of that brand. In line with the idea that people are attracted by large assortments, the authors found that more consumers approached the tasting table when it displayed 24 jams. Yet, when it came to actual purchase, 30% of all consumers who saw the small assortment of six jams at the tasting display actually bought one of the jams (with the coupon), whereas in the large assortment case, only 3% of the people redeemed the coupon for a jam. The authors interpreted this finding as a consequence of choice overload such that too many options decreased the motivation to make a choice. The apparent contradiction between

the initial attractiveness of large assortments and its demotivating consequences is also referred to as the *paradox of choice* (Schwartz 2004).

In another study, Iyengar and Lepper (2000) offered participants a choice between an array of either six or 30 exotic chocolates. Participants who chose from the 30 options experienced the choice as more enjoyable but also as more difficult and frustrating. Most intriguingly, though, participants facing the large assortment reported less satisfaction with the chocolates they finally chose than those selecting from the small assortment (5.5 vs. 6.3 on a 7-point Likert scale). Moreover, at the end of the experiment, only 12% of the participants in the large assortment condition accepted a box of chocolates instead of money as compensation for their participation, compared to 48% in the small assortment condition. This suggests that facing too many attractive options to choose from ultimately decreases the motivation to choose any of them.

Other researchers found similar results in choices among other items, including pens (Shah and Wolford 2007), chocolates (Chernev 2003b), gift boxes (Reutskaja and Hogarth 2009), and coffee (Mogilner et al. 2008). Iyengar and Lepper (2000) also found empirical evidence for choice overload in a study in which the quality of written essays decreased if the number of topics to choose from increased. Along the same lines, Iyengar et al. (2004) found that the number of 401(k) pension plans that companies offered to their employees was negatively correlated with the degree of participation in any of the plans.

### NECESSARY PRECONDITIONS

Researchers observing choice overload have commonly argued that negative effects do not always occur but rather depend on certain necessary preconditions. One important such precondition is lack of familiarity with, or prior preferences for, the items in the choice assortment so that choosers will not be able to rely merely on selecting something that matches their own preferences (Iyengar and Lepper 2000). Chernev (2003a, 2003b) showed that people with clear prior preferences prefer to choose from larger assortments and that, for those people, choice probability and satisfaction increased with the number of options to choose from, the opposite of choice overload. Comparable results were obtained by Mogilner et al. (2008), who found a negative relationship between assortment size and satisfaction only for those people who were relatively less familiar with the choice domain. For this reason, experiments on choice overload have typically used options that decision makers are not very familiar with to prevent strong prior preferences for a specific option and consequently a highly selective search process that would allow participants to ignore most of the assortment.

It can also be assumed that choice overload can occur only if there is no obviously dominant option in the choice set and if the proportion of nondominated options is large, because otherwise the decision will be easy regardless of the number of options (Dhar 1997; Dhar and Nowlis 1999;

Hsee and Leclerc 1998; Redelmeier and Shafir 1995). But while the existence of prior preferences or a dominant option might explain why one would not suffer from having too much choice, it is not directly obvious why the lack of a dominant option or of prior preferences should lead to the occurrence of choice overload. Thus these appear to be necessary but not sufficient preconditions for choice overload.

While the size of the assortment is at the core of the choice overload hypothesis, there is no exact definition of what constitutes too much choice. Iyengar and Lepper (2000, 1996) described it as a “reasonably large, but not ecologically unusual, number of options.” In contrast, Hutchinson (2005) argued that at least for nonhuman animals, choice overload effects are seldom found because organisms are adapted to assortment sizes that naturally occur in their environment. If this holds true for humans as well, choice overload may be most likely to loom in novel situations with an excessive number of options such that the assortment exceeds ecologically usual sizes.

### **ARGUMENTS IN FAVOR OF THE CHOICE OVERLOAD HYPOTHESIS**

Several reasons have been proposed for why facing too many options may lead to less, or less satisfying, choice among them. Having more options to choose from within a category is likely to render the choice more difficult as the differences between attractive options get smaller and the amount of available information about them increases (Fasolo et al. 2009; Timmermans 1993). Large assortments also make an exhaustive comparison of all options seem undesirable from a time-and-effort perspective, which could in turn induce fears of not being able to choose optimally (Iyengar, Wells, and Schwartz 2006; Schwartz 2004). The attractiveness of the second-best, nonchosen alternative is also likely to be greater in larger assortments, which might lead to more counterfactual thinking and regret concerning what was not chosen. Large assortments may also increase expectations, and if the available options are all very similar, these expectations may not be met (Diehl and Poyner 2007; Schwartz 2000). Together, these processes may decrease the decision maker’s satisfaction with the finally chosen option. To the degree that the most attractive options get more similar as choice set size grows, it can also become more difficult to justify the choice of any particular option (Sela, Berger, and Liu 2009). If such consequences are anticipated, they could lower the motivation to make any choice in the first place (Bell 1982; Zeelenberg et al. 2000). Finally, a decision maker who has more options to choose from while having only loosely defined preferences might sometimes also face more unattractive alternatives or options that cater to the specific needs of others and thus are of no personal interest. Weeding out those alternatives while retaining the interesting ones requires additional time and cognitive resources (Kahn and Lehmann 1991), and again anticipating this effort might deter some people from engaging in the choice process in the first place.

### **ARGUMENTS AGAINST THE CHOICE OVERLOAD HYPOTHESIS**

However, there are also arguments that question the choice overload hypothesis. First, large assortments can have advantages, as a large variety of choices increases the likelihood of satisfying diverse consumers and thus caters to individuality and pluralism (Anderson 2006). Accordingly, retailers in the marketplace who offer more choice seem to have a competitive advantage over those who offer less (Arnold, Oum, and Tigert 1983; Bown, Read, and Summers 2003; Craig, Ghosh, and McLafferty 1984; Koelemeijer and Oppewal 1999; Oppewal and Koelemeijer 2005). Second, if negative effects of too much choice are robust and generalizable, one might think that retailers could increase sales by offering less variety. Yet, while researchers analyzing actual field data have reported some instances in which sales actually went up with fewer options, in many cases, reducing the number of different items apparently led to reduced sales or to no change (Boatwright and Nunes 2001; Borle et al. 2005; Drèze, Hoch, and Purk 1994; Sloat, Fok, and Verhoef 2006). In line with this, in a series of experiments, Berger, Draganska, and Simonson (2007) showed that introducing finer distinctions within a product line increased perceptions of quality and that a brand offering high variety within a category has a competitive advantage.

There are other advantages of having many options to choose from: a large assortment that is made available all in one place reduces the cost of searching for more options, allows for more direct comparisons between options, and makes it easier to get a sense of the overall quality distribution. These factors can lead to better-informed, more confident choices (Eaton and Lipsey 1979; Hutchinson 2005). Choosing from a variety of options also meets a desire for change and novelty and provides insurance against uncertainty or miscalculation of one’s own future preferences (Ariely and Levav 2000; Kahn 1995; Simonson 1990). With regard to food, humans and other omnivorous species consume higher quantities when the number of options to choose from increases (Rolls et al. 1981), possibly indicating the benefits of diversifying one’s dietary intake.

On a theoretical level, researchers have argued that an increase in the number of attractive alternatives increases an individual’s freedom of choice, particularly if the alternatives are equally high valued (Reibstein, Youngblood, and Fromkin 1975). There is also early evidence reported by Anderson, Taylor, and Holloway (1966) showing that an increase in the number of options leads to more satisfaction with the finally chosen option, especially when all options were initially rated as about equally attractive. This finding was explained as a postdecisional spreading apart of the alternatives’ subjective values to reduce cognitive dissonance (Brehm 1956; Festinger 1957).

### **NEED FOR FURTHER INVESTIGATION**

Given the unsettled state of the field indicated by these opposing reasons for and against the choice overload hy-

pothesis, it is important to try to further understand when extensive choice sets may trigger negative consequences. Direct replications of previous studies have been suggested to be a good starting point for such an endeavor (Evan-schitzky et al. 2007; Hubbard and Armstrong 1994). In an attempt at this, Scheibehenne (2008) aimed to replicate the results of the Iyengar and Lepper (2000) jam study in an upscale supermarket in Germany but did not find any negative effects of choice overload. Likewise, Greifeneder (2008) found no difference between small and large assortment sizes for choices among exotic chocolates in the lab. A related attempt to replicate the earlier findings by using jelly beans instead of chocolates also failed (Scheibehenne 2008). The authors of these studies pointed out a number of small differences between their experiments and the originals, including cultural differences and minor procedural variations. Yet, if these small differences eliminated the negative consequences of choice overload, it appears important to further understand how robust these negative effects are and what moderates their occurrence: theorists and practitioners need to know under what conditions a particular finding can be expected to be valid (Mick 2001). Therefore, in the following section we investigate the extent and robustness of negative consequences arising from having too much choice by conducting a meta-analysis of all experimental studies we could find that assessed these effects. As we will outline in more detail below, this analysis also allowed us to test the impact of several potential moderator variables and preconditions of choice overload that have been proposed in the literature.

## META-ANALYSIS

Following common practice, we begin our meta-analysis of choice overload studies by first analyzing the distribution of effect sizes across studies. Building on this initial result, we next calculate the mean effect size of choice overload across studies. We also explore the extent to which the diverging results between those studies that found the effect and those that did not can be explained by potential moderator variables. Finally, we ask how much of the variance can be attributed to mere random variation around the mean effect size. Because the meta-analysis integrates data from many sources, these questions can be tested with more statistical power than is achievable by any individual study. Therefore, the meta-analysis yields an integrative overview of research on choice overload and so provides a basis for further constructive research in this area.

### Method

*Data Collection.* Data were collected by means of an extensive literature search that involved scanning journals and conference proceedings and personal communication with scholars in the field. We also put out a broad call for relevant studies (published or unpublished) that went out to several Internet newsgroups covering the areas of consumer behavior, marketing, decision making, and social psychol-

ogy, including Electronic Marketing of the American Marketing Association, the Society for Judgment and Decision Making, the European Association for Decision Making, and the Society for Personality and Social Psychology.

*Inclusion Criteria.* The meta-analytical integration of different studies requires that their designs and research questions be comparable. Therefore, we focused on data from randomized experiments in which participants were given a real or hypothetical choice from an assortment of options, with the number of options being subject to experimental manipulation in a between- or within-subject design. Studies employing correlational or qualitative designs were not included.

*Dependent Variables.* The dependent variables measured in the experiments were either a continuous measure of self-reported satisfaction with the finally chosen option (which usually requires a forced-choice paradigm) or a dichotomous measure indicating whether an active choice was made (which can then be averaged across participants to yield an overall probability of making a choice). In the latter case, when participants did not make an active choice, they received either a default option or nothing (the no-choice option). A few studies assessed the total amount of consumption (as in the studies by Kahn and Wansink [2004]) or preference strength (operationalized as the willingness to exchange a chosen option at a later point; e.g., Chernev 2003b; Lin and Wu 2006).

*Overview of Analyzed Studies.* The data set stems from 50 experiments, with a total of 5,036 participants, reported in 13 published or forthcoming journal articles and 16 unpublished manuscripts made available between the years 2000 and 2009. The unpublished manuscripts include 11 working papers or conference contributions as well as three PhD and two master's theses. In cases in which experiments comprised different conditions or manipulations in a between-subject design, we tried to code each condition separately to retain possible interaction effects. Thus, the data set consists of a total of 63 data points that provided the basis for the meta-analysis.

Experiments were conducted in the United States, Europe, Asia, and Australia. The types of options to choose from covered a wide range including food items (jelly beans, chocolates, jam, coffee, wine), restaurants, diverse consumer goods (mobile phones, pens, magazine subscriptions), dating partners, charity organizations, lotteries, vacation destinations, wallpapers, and music compact discs. The mean sample size per data point was 80, with an interquartile range (IQR) of 45–80 participants. Across all experiments, assortment sizes for the small choice conditions had an average size of seven (IQR five to six) versus 34 for the large assortments (IQR 24–30). Table 1 provides an overview of all data included in the meta-analysis, sorted by the last name of the first author.

*Effect Size Measure.* To enable meta-analytical integration across the data set, we transformed the difference

in the dependent variable between the small and the large assortment of each experiment into a Cohen's  $d$  effect size measure that expresses the difference between the two assortments, scaled by its pooled standard deviation (Cohen 1977). A positive  $d$ -value indicates choice overload and a negative sign indicates a more-is-better effect. Effect sizes were calculated either from raw data or from the statistics presented in the manuscripts. Most experiments adopted a comparison between two groups (small assortment vs. large assortment) and thus could be integrated without further assumptions.

To ensure the comparability of the results from studies with comparisons between more than two assortment sizes, we calculated sensible one-degree-of-freedom contrasts as suggested by Rosenthal and DiMatteo (2001). To make this choice of contrasts reasonable, we selected those conditions that would amplify possible effects of choice overload without discarding too much of the original data. In the case of studies by Reutskaja (2008) and Reutskaja and Hogarth (2009), where the assortment size varied between five and 30 with increments of five, we selected the contrast between 10 and 30 options. In the study by Shah and Wolford (2007), where assortment sizes varied between two and 20 with increments of two, we contrasted the mean of small assortments ranging from six to 12 with the mean of large assortments ranging from 14 to 20 options. For Mogilner et al. (2008), we calculated contrasts between the small and the large assortment separately for participants who were relatively familiar with the domain of choice (so-called preference matchers) and those who were relatively unfamiliar with it (so-called preference constructors) to retain the interaction effect and to include all data. In the experiment by Scheibehenne et al. (2009) on choices among charities, we combined the two large assortments with 40 and 80 options into one large set. For the experiments by Greifeneder (2008), Greifeneder, Scheibehenne, and Kleber (2010), Lenton and Stewart (2007), and Söllner and Newell (2009), we contrasted the small condition with the large condition and discarded the medium condition. For seven experiments in which authors reported only approximate statistical indices to indicate negligible effects (e.g.,  $p > .1$  or  $p = \text{NS}$ ) and the detailed data could not be retrieved, we followed the suggestion of Rosenthal and DiMatteo (2001) and assigned an effect size of zero (the respective data points are marked in table 1).

*Data Analysis.* We integrated the results of all experiments by calculating a random effects model in which the effect size  $d$  for each study  $i$  is assumed to be randomly distributed around  $D$ , the mean effect size across all studies:

$$d_i = D + u_i + e_i, \quad (1)$$

where  $u_i$  is the deflection of the true effect size of study  $i$  from  $D$  and  $e_i$  is the sampling error of study  $i$ . Both  $u$  and  $e$  are assumed to be independent and normally distributed with a mean of zero. The within-study variance of  $e_i$  is denoted  $s_i^2$ . The between-study variance of  $u$  is denoted  $\tau^2$

and indicates the true variance of the effect sizes across studies, which quantifies the heterogeneity of the effect sizes.

Estimates of within-study variance were calculated as a function of sample size and effect size for each study (Hedges and Olkin 1985; Lipsey and Wilson 2001; Shadish and Haddock 1994). Estimates for  $\tau^2$  were obtained on the basis of an algorithm by Viechtbauer (2006) that uses a restricted maximum likelihood estimation for  $\tau^2$  and is implemented in R 2.9.1 (see also Raudenbush 1994).

## Results

*Mean Effect.* Figure 1 provides an overview of the data by means of a forest plot. The mean effect size of choice overload across all 63 data points according to equation 1 is  $D = 0.02$  (95% confidence interval  $[CI_{95}] -0.09$  to  $0.12$ ). The between-study variance is  $\tau^2 = 0.12$  ( $CI_{95} -0.08$  to  $0.25$ ), indicating that the difference between effect sizes may not be totally accounted for by sampling error. The null hypothesis of  $\tau^2 = 0$  can be tested by means of the  $Q$ -statistic (Cochran 1954). Under the null hypothesis of homogeneity among the effect sizes, the  $Q$ -statistic follows a chi-square distribution. For the data set on hand,  $Q(62) = 192$  ( $p < .001$ ), which suggests that the variance between studies may partly stem from systematic differences and not just from random variation. However, the  $Q$ -statistic has been criticized because of its excessive power to detect unimportant heterogeneity when there are many studies (Higgins and Thompson 2002). The presumably more informative  $I^2$ -statistic that quantifies the proportion of between-study variance due to heterogeneity independent of the number of studies yields  $I^2 = 68\%$ , also implying medium to high heterogeneity (Huedo-Medina et al. 2006). While this is not definite proof of the existence of moderators driving the heterogeneity, it strongly invites further investigations of the variability across studies (Egger and Smith 1997; Higgins et al. 2003). Given this possibility, we next turn to a more detailed exploration of the differences between studies in the meta-analysis.

*Robustness Test.* As a first step toward further investigation, a trimming procedure to control for possible outliers as recommended by Wilcox (1998) revealed that the mean effect size is rather robust and not biased by a few studies that report extreme effect sizes. If the data set was trimmed by 20% by excluding the six studies with the highest effect sizes and the six studies with the lowest,  $D_{\text{trimmed}} = 0.001$  ( $CI_{95} -0.08$  to  $0.07$ ). The unexplained variance in the trimmed data set reduced to  $I^2 = 22\%$ , indicating little heterogeneity, which hints that most of the heterogeneity (but not the small mean effect size) in the original data set might be due to a few studies reporting large effect sizes in both directions.

*Moderator Variables.* To further explore the variance within the untrimmed data set, we coded a number of variables that could potentially moderate choice overload and

**TABLE 1**  
OVERVIEW OF ALL DATA INCLUDED IN THE META-ANALYSIS, SORTED BY NAME OF FIRST AUTHOR

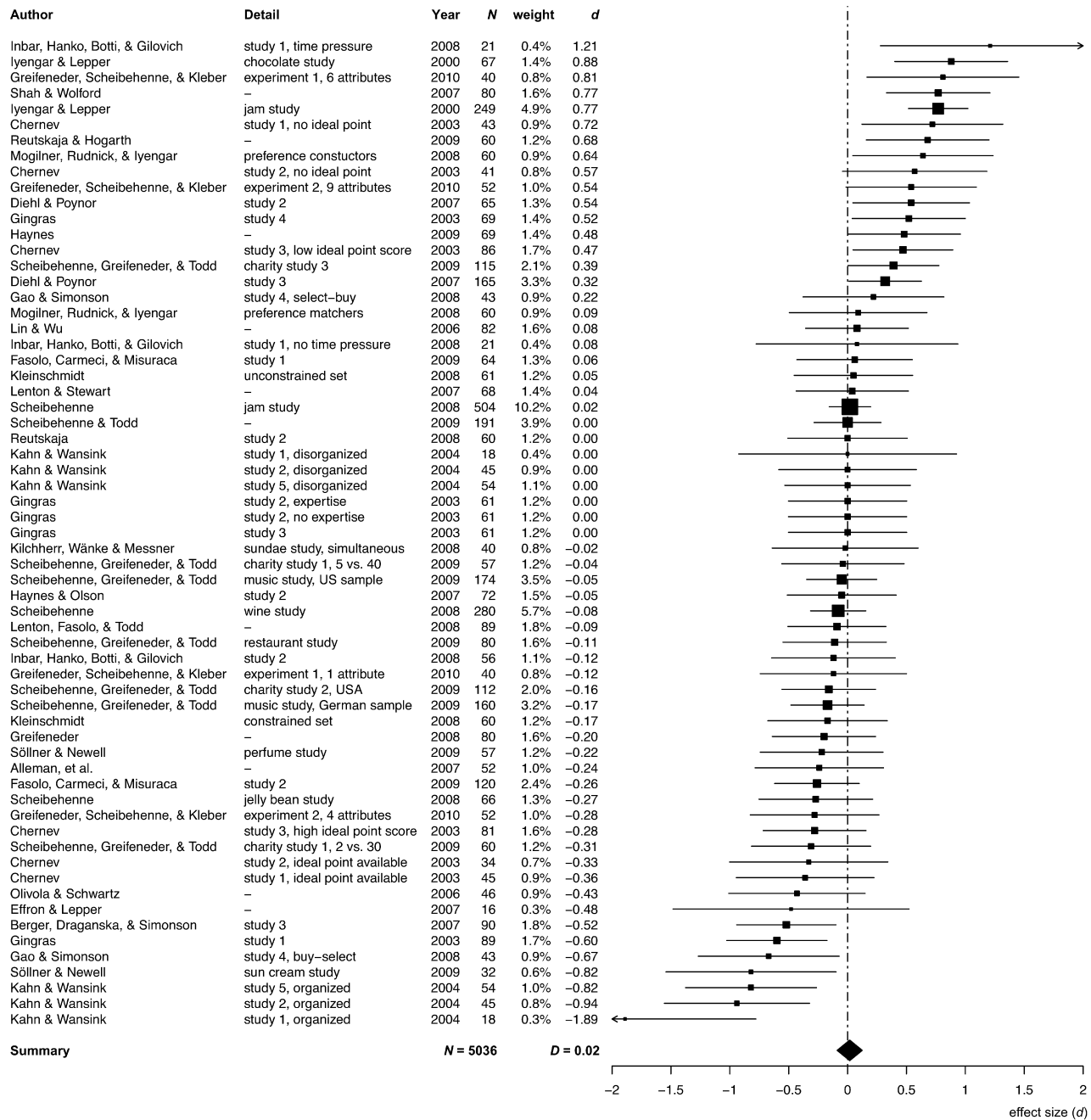
Author(s)	Year	Effect size		Set size		Total sample size	Moderators <sup>a</sup>	Choice entity	Further detail
		<i>d</i>	<i>s</i> <sup>2</sup>	Small	Large				
Alleman et al.	2007	-.24	.08	6	24	52	3/1/0/3/0	Monetary gambles	Study 3
Berger, Draganska, and Simonson	2007	-.52	.05	13	30	90	1/0/0/1/0	Chocolates	Study 1, ideal point
Chernev	2003	-.36	.09	4	16	45	1/0/0/3/1	Chocolates	Study 1, no ideal point
Chernev	2003	.72	.09	4	16	43	1/0/0/3/0	Chocolates	Study 2, ideal point
Chernev	2003	-.33	.12	4	16	34	1/0/0/3/1	Chocolates	Study 2, no ideal point
Chernev	2003	.57	.10	4	16	41	1/0/0/3/0	Chocolates	Study 3, high ideal score
Chernev	2003	-.28	.05	6	24	81	1/0/1/3/1	Various products	Study 3, low ideal score
Chernev	2003	.47	.05	6	24	86	1/0/1/3/0	Various products	Study 2
Diehl and Poynor	2007	.54	.06	60	300	65	3/0/1/1/0	Wallpapers	Study 3
Diehl and Poynor	2007	.32	.02	8	32	165	3/0/1/1/0	Camcorders	Study 3
Efron and Lepper	2007	-.48	.26	10	15	16	3/0/0/1/0	Jelly beans	Study 1
Fasolo, Carmeci, and Misuraca	2009	.06	.06	6	24	64	1/1/1/1/0	Mobile phones	Study 1
Fasolo, Carmeci, and Misuraca	2009	-.26	.03	6	24	120	1/1/1/1/0	Mobile phones	Study 2
Gao and Simonson	2008	-.67	.09	10	50	43	3/0/0/3/0	Jelly beans	Study 4, buy-select
Gao and Simonson	2008	-.22	.09	10	50	43	3/0/1/3/0	Jelly beans	Study 4, select-buy
Gingras	2003	-.60	.05	10	30	89	2/0/1/1/0	Food dishes	Study 1
Gingras	2003	0 <sup>b</sup>	.07	6	24	30	2/0/1/3/1	Vacation packages	Study 2, expertise
Gingras	2003	0 <sup>b</sup>	.07	6	24	31	2/0/1/3/0	Vacation packages	Study 2, no expertise
Gingras	2003	0 <sup>b</sup>	.07	5	21	61	2/0/0/1/0	Candy bars	Study 3
Gingras	2003	.52	.06	6	30	69	2/0/0/1/0	Chocolates	Study 4
Greifeneder	2008	-.20	.05	6	30	80	3/1/0/3/0	Chocolates	Experiment 1, 1 attribute
Greifeneder, Scheibehenne, and Kleber	2010	-.12	.10	6	30	80	1/1/1/1/0	Pencils	Experiment 1, 6 attributes
Greifeneder, Scheibehenne, and Kleber	2010	.81	.11	6	30	80	1/1/1/1/0	Pencils	Experiment 2, 4 attributes
Greifeneder, Scheibehenne, and Kleber	2010	-.28	.08	6	30	52	1/1/1/1/0	mp3 player	Experiment 2, 9 attributes
Greifeneder, Scheibehenne, and Kleber	2010	.54	.08	6	30	52	1/1/1/1/0	mp3 player	Experiment 2, 9 attributes
Haynes	2009	.48	.06	3	10	69	1/0/1/1/0	Various products	Study 2
Haynes and Olson	2007	-.05	.06	5	20	72	3/0/1/1/0	Various products	Study 1, no time pressure
Inbar et al.	2008	.08	.19	6	30	21	3/0/0/1/0	Chocolates	Study 1, time pressure
Inbar et al.	2008	1.21	.23	6	30	21	3/0/0/1/0	Chocolates	Study 1, time pressure

Inbar et al.	2008	-.12	.07	1.1	6	30	56	3/0/0/1/0	Chocolates	Study 2
Iyengar and Lepper	2000	.88	.06	1.4	6	30	67	1/0/0/3/0	Chocolates	Chocolate study
Iyengar and Lepper	2000	.77	.02	4.9	6	24	249	1/0/0/3/0	Jam	Jam study
Kahn and Wansink	2004	0 <sup>b</sup>	.22	.4	6	24	18	1/0/0/2/0	Jelly beans	Study 1, disorganized
Kahn and Wansink	2004	-1.89	.32	.3	6	24	18	1/0/0/2/0	Jelly beans	Study 1, organized
Kahn and Wansink	2004	0 <sup>b</sup>	.09	.9	6	24	46	1/0/0/2/0	Jelly beans	Study 2, disorganized
Kahn and Wansink	2004	-.94	.10	.8	6	24	46	1/0/0/2/0	Jelly beans	Study 2, organized
Kahn and Wansink	2004	0 <sup>b</sup>	.07	1.1	6	24	54	1/0/0/2/0	Jelly beans	Study 5, disorganized
Kahn and Wansink	2004	-.82	.08	1.0	6	24	54	1/0/0/2/0	Jelly beans	Study 5, organized
Kilchherr, Wänke, and Messner	2008	-.02	.10	.8	12	72	40	2/1/0/1/0	Sundaes	Simultaneous choice
Kleinschmidt	2008	-.17	.07	1.2	3	12	60	2/1/0/1/0	Chocolates	Constrained set
Kleinschmidt	2008	.05	.07	1.2	6	24	61	2/1/0/1/0	Chocolates	Unconstrained set
Lenton, Fasolo, and Todd	2008	-.09	.04	1.8	4	20	89	1/1/1/1/0	Dating partner	. . . .
Lenton and Stewart	2007	-.04	.06	1.4	4	64	68	3/1/1/1/0	Dating partner	. . . .
Lin and Wu	2006	.08	.05	1.6	6	16	82	1/1/1/3/0	Chocolates	. . . .
Mogilner, Rudnick, and Iyengar	2008	.64	.09	.9	5	50	60	1/0/0/1/0	Coffee	Preference constructors
Mogilner, Rudnick, and Iyengar	2008	.09	.09	.9	5	50	60	1/0/0/1/0	Coffee	Preference matchers
Olivola and Schwartz	2006	-.43	.09	.9	4	20	48	3/0/1/3/0	Magazines	. . . .
Reutskaja	2008	0 <sup>b</sup>	.07	1.2	10	30	60	2/1/1/1/0	Gift boxes	Study 2
Reutskaja and Hogarth	2009	-.68	.07	1.2	10	30	60	1/1/1/1/0	Gift boxes	. . . .
Scheibehenne	2008	.02	.01	10.2	6	24	504	2/1/0/3/0	Jam	Jam study
Scheibehenne	2008	-.27	.06	1.3	6	30	66	2/1/0/3/0	Jelly beans	Jelly bean study
Scheibehenne	2008	-.08	.01	5.7	3	12	280	2/1/0/3/0	Red wine	Wine study
Scheibehenne, Greifeneder, and Todd	2009	-.31	.07	1.2	2	30	60	1/1/0/3/0	Charities	Charity study 1, 2 vs. 30
Scheibehenne, Greifeneder, and Todd	2009	-.04	.07	1.2	5	40	57	1/1/0/3/0	Charities	Charity study 1, 5 vs. 40
Scheibehenne, Greifeneder, and Todd	2009	-.16	.04	2.0	5	40 & 80	112	1/0/0/3/0	Charities	Charity study 2, U.S.
Scheibehenne, Greifeneder, and Todd	2009	.39	.04	2.1	5	40 & 80	119	1/1/0/3/0	Charities	Charity study 3
Scheibehenne, Greifeneder, and Todd	2009	-.17	.03	3.2	6	30	80	1/1/0/1/0	Classical music	Music study, Germany
Scheibehenne, Greifeneder, and Todd	2009	-.05	.02	3.5	6	30	87	1/0/0/1/0	Classical music	Music study, U.S.
Scheibehenne, Greifeneder, and Todd	2009	-.11	.05	1.6	5	30	80	1/1/0/3/0	Restaurants	Restaurant study
Scheibehenne, Greifeneder, and Todd	2009	.00	.02	3.9	6	63	191	3/0/0/3/0	Mutual funds	. . . .
Shah and Wolford	2007	.77	.05	1.6	6-12	14-20	80	1/0/0/3/0	Pens	. . . .
Söllner and Newell	2009	-.22	.07	1.2	3	18	57	3/1/1/3/0	Perfume	Perfume study
Söllner and Newell	2009	-.82	.14	.6	3	30	32	3/1/1/3/0	Sun cream	Sun cream study

<sup>a</sup>Sequence of moderators: source of publication (1 = journal, 2 = thesis, 3 = unpublished)/country (0 = United States, 1 = outside the United States)/type of decision (0 = real, 1 = hypothetical)/dependent variable (1 = satisfaction, 2 = consumption quantity, 3 = choice probability or preference strength)/expertise or prior preferences (1 = yes, 0 = no).

<sup>b</sup>Authors report "nonsignificant" results or a mean difference of zero.

**FIGURE 1**  
OVERVIEW OF THE DATA AS A FOREST PLOT



NOTE.—The positions of the squares on the x-axis indicate effect sizes of each data point. The bars indicate the 95% confidence intervals of the effect sizes. The sizes of the squares are inversely proportional to the respective standard errors (i.e., larger squares indicate smaller standard errors).

that could be assessed for all experiments in the data set. These were the year in which the data were made publicly available, the country in which the experiment was conducted, the size of the large assortment, whether the study employed a real or a hypothetical choice task, the type of

dependent variable (satisfaction, consumption quantity, or a measure of choice), whether the data stem from a journal article or an unpublished source, and whether participants had clear prior preferences or expertise in the respective choice domain.



While there are other potential moderators of choice overload that would be worthwhile to compare across studies, meta-analytic methods require that such variables be measured (or can at least be coded) in more than one study. We instead assessed those specific moderators that appeared only in single studies by means of a qualitative review following the meta-analysis.

*Meta-Regression Model.* To estimate the amount of variance in the untrimmed data set that could be explained by these potential moderators, we extended the random effects model in equation 1 by a meta-regression in which the mean effect size  $D$  is predicted by a linear combination of the coded variables:

$$d_i = \beta_0 + \beta_1 x_{1i} + \beta_2 x_{2i} + \dots + \beta_j x_{ji} + u_i + e_i, \quad (2)$$

where  $x_{ji}$  denotes the value of the  $j$ th moderator variable for study  $i$ , and  $\beta_j$  denotes the regression coefficient of the  $j$ th moderator variable. Nominal moderator variables (such as the publication source or the country of origin) entered the model dummy coded. This meta-regression model was fitted by the same R script used for the random effects model (Viechtbauer 2006). Table 2 shows a summary of the estimated  $\beta$  coefficients for each moderator, along with  $z$ -scores, standard errors, and  $p$ -values. The results can be interpreted analogous to a conventional multiple linear regression. The  $\beta$  estimates in the table indicate deflections relative to an arbitrarily chosen imaginary baseline study ( $\beta_0$ ) conducted in the United States that adopted a real rather than a hypothetical choice from a large set with 30 options and that was publicized as a working paper in the year 2004. Given this coding, a positive  $\beta$  estimate means that the respective moderator increases choice overload and a negative  $\beta$  estimate indicates a decrease relative to the baseline.

*Influence of Moderators.* Given the data coding used, the meta-regression showed that “more choice is better” for those experiments that use consumption quantity as a dependent measure, an effect that was driven by the data from Kahn and Wansink (2004). The analysis also confirmed previous findings showing that decision makers with strong prior preferences or expertise benefit from having more options to choose from (Chernev 2003b; Mogilner et al. 2008). This supports the intuitions of those experimenters who took measures to control for prior preferences, for example, by removing familiar options from the choice set or by using exotic products as in the studies by Iyengar and Lepper (2000).

The meta-regression results further show that published articles as compared to unpublished manuscripts are somewhat more likely to report positive effect sizes, indicating a slight publication bias in favor of choice overload results. Finally, there is an effect of the year of publication such that more recent experiments are less likely to find negative consequences of extensive choice sets. This may be indicative of a so-called Prometheus effect, according to which tantalizing counterintuitive findings have an initial advan-

TABLE 2

MODERATOR ESTIMATES IN THE META-REGRESSION

Moderator	$\beta$ estimate	SE( $\beta$ )	$z$	$p$
$\beta_0$ (intercept)	.11	.11	1.02	.309
Consumption as dependent variable	-.87	.21	-4.13	<.001
Expertise or prior preferences	-.50	.20	-2.49	.013
Journal publication (vs. unpublished data)	.27	.10	2.72	.007
Publication year	-.05	.02	-2.11	.035
Hypothetical choice (vs. real choice)	-.01	.10	-.13	.898
Satisfaction as dependent variable	.06	.11	.56	.576
Size of the large choice set	.002	.001	1.48	.140
Study conducted outside the United States	-.08	.13	-.61	.540

tage for getting published compared to follow-up experiments that often find less strong results (Trikalinos and Ioannidis 2005).

Beyond these results, no other moderators could be established. Effect sizes did not depend on whether the choice task in the experiment was hypothetical or real or whether satisfaction or choice was the dependent variable. Likewise, there were no differences between experiments conducted within or outside the United States, which questions cultural differences as an explanation for when choice overload occurs, at least on this broad level. This is also in line with the results of Scheibehenne et al. (2009), who directly tested and did not find cultural differences in choice overload between Germany and the United States by conducting closely matched experiments in both countries. Further experiments in other countries and with more fine-grained measures of cultural differences could still reveal other patterns in the future.

Furthermore, within the tested range, there is no linear relationship between the effect size and the number of options offered in the large choice set. However, this does not rule out a curvilinear relationship, which we will explore in more detail below.

With all moderators included, the meta-regression accounts for 56% of the effect size variance ( $\tau^2 = 0.07$ ), indicating that there is still a moderate degree of variance between studies left unexplained. We proceed to use other meta-analyses to try to explore some of the sources of that remaining variance.

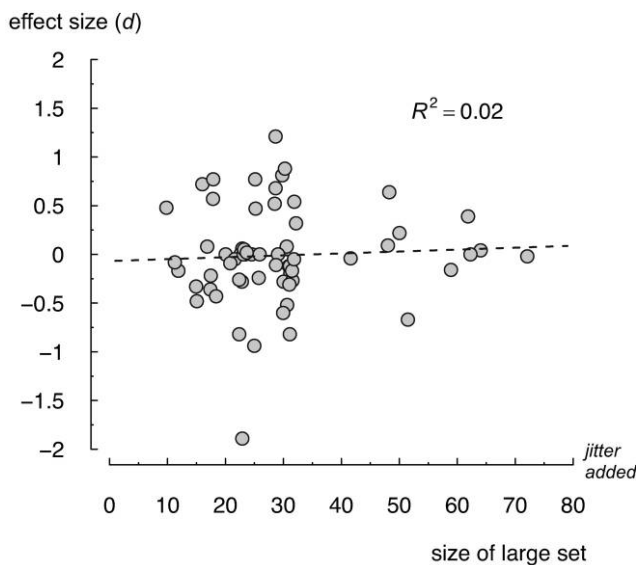
*Curvilinear Relationship with Assortment Size.* Past research suggested that possible consequences of having many options to choose among might follow a curvilinear relationship, such that an initial increase in the assortment size leads to a more-is-better effect but a further increase eventually leads to choice overload (Reutskaja and Hogarth 2009; Shah and Wolford 2007). To test this proposed relationship meta-analytically, we fitted a quadratic function

to the relationship between the effect size and the size of the large assortment across all 63 data points (fig. 2). Yet the fit of the function was poor ( $R^2 = 0.02$ ), suggesting that a curvilinear relationship cannot be substantiated on the basis of the data on hand.

**Funnel Plot Analysis.** As an alternative approach to exploring the heterogeneity (and hence possible systematic differences) across studies, Rosenthal and DiMatteo (2001) recommended looking for naturally occurring groupings in the plotted data that could point to potential moderators. Toward this goal, figure 3 shows the effect size of each data point plotted against its inverse sampling error variance ( $s^{-2}$ ) in a so-called funnel plot. Under the assumption of homogeneous variance, one would expect the points to scatter such that studies with a smaller error—thus higher on the y-axis—are closer to the grand mean effect size on the x-axis, so that the plot would look like an upside-down funnel or a volcano. Figure 3 indeed shows this pattern, as studies with a smaller sampling error scatter closer around the grand mean of zero, further supporting the notion of a true population effect size around zero. Yet there are also a few studies showing an effect size  $d \geq 0.2$ , indicating choice overload, that appear to cluster slightly apart from the majority of studies with zero or negative effect size. In line with the results of the moderator analysis, most of the studies in this cluster were published as journal articles. It might be that this cluster indicates an artifact due to publication bias that would disappear with more data being collected in

FIGURE 2

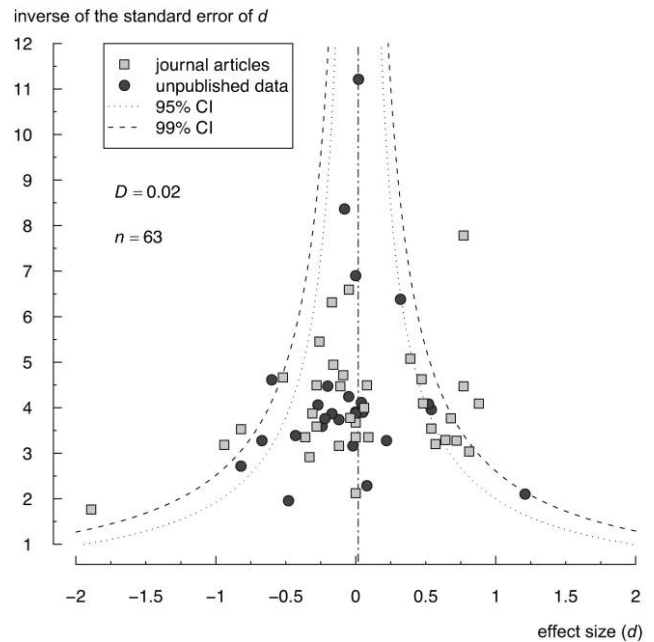
RELATIONSHIP BETWEEN THE EFFECT SIZE AND THE SIZE OF THE LARGE SET



NOTE.—The dotted line indicates the best-fitting quadratic regression curve. To improve presentation, jitter was added to the x-coordinate values and the x-axis was truncated at 80, omitting one data point representing a set size of 300 options.

FIGURE 3

FUNNEL PLOT OF ALL DATA



NOTE.—The dotted and dashed lines indicate confidence intervals under the assumption that the data set is homogeneous and normally distributed around the mean effect size.

the future that would fill the gap. An alternative explanation is that the true distribution of effect sizes might be bimodal. When the data are split at  $d = 0.2$ , the variance within both subsets appears to be homogeneous according to the  $Q$ -statistic, which gives some preliminary support for the idea of two distinct clusters of studies. This post hoc split should be interpreted with caution, but it could nevertheless be of heuristic use for exploring how the studies in the two sets differ. Most of these differences involve study-specific moderators that cannot be tested meta-analytically but rather call for a descriptive examination, which we turn to next.

DISCUSSION

The overall mean effect size across 63 conditions from 50 experiments in our meta-analysis was virtually zero. On the basis of the data, no sufficient conditions could be identified that would lead to a reliable occurrence of choice overload. While this suggests that adverse consequences due to having too much choice are not a robust phenomenon, there are also still a number of studies that report effect sizes indicating choice overload. Also, the variance between the effect sizes in the whole data set is higher than what would be expected by a mere random distribution around an effect size of zero. Although most of this extra variance presumably arose from a small number of studies that found very high positive and negative effect sizes, it nevertheless appears instructive to further seek conditions that may propel

or hinder choice overload. As a first step in this direction, the meta-regression indicated that some of the variance was due to the fact that using consumption as the dependent variable or having decision makers with well-defined preferences led to a more-is-better effect. Also, there was a slight publication bias such that unpublished and more recent studies were less likely to find an effect, contributing further to the variance. However, theoretically more meaningful moderators such as the magnitude of the assortment size difference did not account for the variance.

These results do not rule out the possibility that the reliable occurrence of choice overload may depend on particular conditions not included in our meta-analysis. A few experiments have explored other such conditions in the past. While those idiosyncratic conditions cannot be tested meta-analytically as indicated earlier, it is still valuable to review them qualitatively to identify and evaluate possible pathways for future research. Because human decision behavior can fruitfully be understood as an interaction between the mind and the environment (Simon 1990; Todd and Gigerenzer 2007), we organize this review by looking at three types of moderators, relating to the structure of the assortment or choice environment, to the goals and strategies of the individual decision makers, and to the interactions between them.

### Assortment Structure

*Categorization and Option Arrangement.* One aspect of the assortment structure that potentially moderates choice overload is the ease with which options can be categorized. For example, Mogilner et al. (2008) found that an increase in the number of options decreased satisfaction only if the options were not prearranged into categories. In line with research on the effect of ordered versus unordered assortments (Diehl 2005; Diehl, Kornish, and Lynch 2003; Huffman and Kahn 1998; Russo 1977), the authors argued that categories make it easier to navigate the choice set and decrease the cognitive burden of making a choice, especially in unfamiliar situations. Thus, the lack of categorization may be another contributing factor to choice overload. Yet it does not appear to be a sufficient condition in itself because most studies included in our meta-analysis that did not find the effect also did not categorize the options.

*Difficult Trade-offs.* Besides the way the options are presented, there are additional aspects of the assortment structure that may explain some of the differences between the study results. For example, the similarity between available options and the degree to which a choice among them involves difficult trade-offs have often not been precisely controlled in studies on choice overload, even though they potentially affect choice satisfaction, regret, and motivation (Hoch, Bradlow, and Wansink 1999; Kahn and Lehmann 1991; Simonson 1990; Van Herpen and Pieters 2002; Zhang and Fitzsimons 1999). Especially if options possess complementary or unique features that are not directly comparable, the number of difficult trade-offs is likely to in-

crease with assortment size (Chernev 2005; Gourville and Soman 2005). On the basis of this analysis, one may suspect that ease of comparison (or lack thereof) constitutes an important potential moderator of choice overload.

*Information Overload.* Reutskaja and Hogarth (2009) elicited reduced choice satisfaction by increasing the complexity of the offered options. In line with Mogilner et al. (2008), they hypothesized that choice overload is due to the increased cognitive effort needed to make a choice. This argument bears similarity to the information overload hypothesis that predicts a negative impact on decision making if the total amount of information concerning the choice assortment grows too large (Jacoby, Speller, and Kohn 1974; Jacoby, Speller, and Kohn Berning 1974). In its original formulation, the amount of information was calculated as the number of options within an assortment multiplied by the number of attributes on which the options are described. From this perspective, choice overload (where only the number of options is large) is a special case of information overload.

While the original conception of information overload was criticized on methodological and empirical grounds (Malhotra 1984; Malhotra, Jain, and Lagakos 1982; Meyer and Johnson 1989), more refined measures of information quantity, for instance, based on the entropy of a choice set, subsequently led to more reliable results (Van Herpen and Pieters 2002): decision makers who are confronted with more information, measured in terms of entropy, have been found to make less informed choices, presumably because cognitive limits prevent them from thoroughly processing the relevant information (Lee and Lee 2004; Lurie 2004). To the degree that people are aware of these limitations, they might feel less comfortable and hence avoid making a choice in such situations, manifesting choice overload.

The entropy-based information measure takes into account the number of options, but it is influenced more strongly by the number of attributes and the distribution and number of levels within each attribute. Past research on choice overload was often not concerned with the exact amount of information presented to decision makers, which might explain some of the diverging results captured in our meta-analysis. In line with this, Greifeneder et al. (2010) found a decrease in satisfaction with an increase in assortment size only when the options to choose from were described on many attributes.

*Time Pressure.* Inbar et al. (2008) found that more options decreased satisfaction with the choice outcome and increased regret only when decision makers felt rushed because of experimentally induced time pressure. To the degree that time pressure kept participants from processing all the information they needed to make a satisfactory choice, they might have suffered from too much information relative to the amount of time they had to consider it. Similar results were reported by Haynes (2009), who found evidence for choice overload only if he constrained the decision makers' time to make a decision. Time pressure must be considered

relative to the amount of information being presented to decision makers, so that this moderator and the previous one are intertwined.

## Decision Strategies

Participants' choice strategies and motivations have often not been thoroughly assessed or controlled in experiments on choice overload. Here we highlight some aspects of decision strategies that could explain some of the variance we found between experiments in the meta-analysis.

*Relative versus Absolute Evaluations.* The effect of a given assortment structure on a choice crucially depends on the goal and strategy of the individual decision maker. Accordingly, Gao and Simonson (2008) found decision makers to be overloaded by choice when they first selected a specific option from an assortment and then decided whether they wanted to purchase it, but not when they first decided if they wanted to purchase from a given assortment and only then chose a specific option. The authors argued that in the latter case (first purchase, then choose), people are more likely to focus first on the overall attractiveness of an assortment, which tends to increase with size. In contrast, in the former case (first choose, then purchase), individuals might be overloaded with choice because they initially focus on the relative attractiveness of a specific option, which tends to decrease with increasing assortment size because the options become more similar to each other (Fasolo et al. 2009). Thus, to the degree that decision makers are looking for the relative best option within a given set, choice overload might occur.

*Maximizing.* The tendency to search for the relative best available rather than a merely satisfactory option is at the core of the maximizing versus satisficing personality construct, reflecting the degree to which decision makers aim to maximize their outcomes (Schwartz et al. 2002). This construct seems to be a plausible moderator for choice overload, as maximizers tend to desire large choice sets while at the same time finding it more difficult to commit to a choice. Furthermore, maximizers are often less satisfied with their selection compared to satisficers (Dar-Nimrod et al. 2009). However, single studies in our meta-analysis that formally tested maximizing in a context of choice overload could not establish it as a moderator (Gingras 2003; Kleinschmidt 2008; Scheibehenne 2008; Scheibehenne et al. 2009). A more reliable, domain-specific measure of maximizing might change these results in the future.

*Choice Justification.* From their third study, Iyengar and Lepper (2000) advanced the conclusion that choice overload might be driven by an increased feeling of personal responsibility when choosing from extensive choice sets. Putting this reasoning to the test, Scheibehenne et al. (2009) found an effect of too many options when people knew that they would have to justify their choice later on. Presumably justification becomes more difficult when choosing from a large set where the best options are more similar, which

apparently led people to avoid making a choice in the first place. Along the same lines, Sela et al. (2009) hypothesized that large assortments make it more difficult to come up with a good reason for any particular choice, which might make it harder for some people to commit to a decision. Thus, even though most studies on choice overload did not explicitly ask for a reason for choices made, it could still be that some decision makers felt the need to justify their decisions anyhow, which could contribute to the differences in results between studies.

*Simple Decision Heuristics.* Irrespective of individual goals and motivations, decision makers commonly cope with excessive choice and information by means of simple choice heuristics (Anderson et al. 1966; Gigerenzer, Todd, and the ABC Research Group 1999; Hendrick, Mills, and Kiesler 1968; Payne, Bettman, and Johnson 1993). Classic examples of such heuristics are the satisficing heuristic that guides individuals to choose the first option that exceeds their aspiration level (Simon 1955), the elimination-by-aspects strategy that quickly screens out unattractive options (Davey, Olson, and Wallenius 1994; Huber and Klein 1991; Tversky 1972), the choice of a default option (Johnson 2008; Johnson and Goldstein 2003), and the consideration-set model that balances search costs and expected outcomes (Hauser and Wernerfelt 1990). There is ample evidence showing that decision makers adaptively apply such heuristic strategies across a wide range of situations (Gigerenzer et al. 1999). In line with this, Jacoby (1984), the original proponent of information overload, concluded that in most real situations decision makers will stop far short of overloading themselves. Consequently, a potential moderator of choice overload is the degree to which decision makers make use of simplifying decision heuristics. This calls for assessing more than just final choice outcomes in studies on choice overload by adding measures of decision processes (see Scheibehenne and Todd [2009b] for a first step in this direction).

## Perception of the Distribution

Empirical evidence suggests that consumers are less likely to prefer large assortments over smaller ones if they assume that the options in both assortments are mostly attractive and of high quality (Chernev 2008). If instead the options are variable but on average of low quality, a large assortment increases the chance that at least one somewhat attractive option will be found. While the options used in the choice experiments in the meta-analysis were certainly not similar, it could still be that participants differed in the degree to which they perceived them as similar or as having high or low mean quality, which could moderate the presence of choice overload. This could be tested in future experiments by manipulating the variance (real or perceived) independent of the assortment size. Similarly, the perception of the assortment size can be very different from the actual number of options (Broniarczyk, Hoyer, and McAlister 1998; Hoch et al. 1999). As it is perception that ultimately guides be-

havior, this is another aspect that could explain some of the variance.

### Choice and Satisfaction as Dependent Variables

The meta-analysis indicated that whether the dependent variable was measured as choice or satisfaction did not moderate choice overload. One reason for this could be that neither dependent variable actually measures a well-defined concept. For example, research on choice overload commonly refers to the satisfaction with the single chosen option rather than the choice experience as a whole. This difference in what is asked could trigger different answers because people may sometimes happily select and try a single less satisfying option in order to learn about the range of possibilities, because they enjoy variance, or because they are aiming for a specific sequence of experiences (Ratner, Kahn, and Kahneman 1999). These goals might be particularly prominent when making choices among exotic and hedonic options that have few long-term consequences, as is often the case for experiments on choice overload. Furthermore, studies on choice overload consistently find that satisfaction with the choice process and the perceived difficulty of making a choice change with assortment size. Thus, these three satisfaction measures—satisfaction with the choice experience as a whole, with the decision process, and with the finally chosen (single) option—can all relate to different aspects of the choice, for which different answers can be expected. In the literature on choice overload, researchers are mostly interested in the third measure, but participants in a typical experiment might very well confound all three when asked about their satisfaction with a choice, which could explain the differences between the studies in the meta-analysis. Greater care in specifying which measures are being sought from participants, as well as collecting all three measures in experiments, would provide valuable data to help pull apart the possible consequences of choice overload.

In a related vein, Anderson (2003) pointed out that making no choice is not a homogeneous concept either but rather embraces different phenomena, including procrastination, an explicit preference for the status quo, or a trade-off between the effort to make a choice and its possible benefits. Because the presence of an extensive choice assortment could also indicate the availability of good or even better options in the future, what looks like choice omission might sometimes be an adaptive deferral strategy for the time being (Hutchinson 2005), which current short-term experimental designs do not capture. Little is known about the specific reasons why participants in choice overload experiments sometimes refrain from making a choice, but further exploring these reasons, including via longer-term studies that allow deferral and future choice, is another pathway to a better understanding of when and why choice overload can reliably be expected to occur.

## FINAL CONCLUSIONS

Although strong instances of choice overload have been reported in the past, direct replications and the results of our meta-analysis indicated that adverse effects due to an increase in the number of choice options are not very robust: The overall effect size in the meta-analysis was virtually zero. While the distribution of effect sizes could not be explained solely by chance, presumably much of the variance between studies was due to a few experiments reporting large positive and large negative effect sizes. The meta-analysis further confirmed that “more choice is better” with regard to consumption quantity and if decision makers had well-defined preferences prior to choice. There was also a slight publication bias such that unpublished and more recent experiments were somewhat less likely to support the choice overload hypothesis. Effect sizes did not depend on whether the choice was hypothetical or real or whether satisfaction or choice was the dependent variable. Likewise, there was no evidence for cultural differences. At least within the analyzed set of experiments, there was also no linear or curvilinear relationship between the effect size and the number of options in the large set.

In summary, we could identify a number of potentially important preconditions for choice overload to occur, but on the basis of the data on hand, we could not reliably identify sufficient conditions that explain when and why an increase in assortment size will decrease satisfaction, preference strength, or the motivation to choose. This might account for why some researchers have repeatedly failed to replicate the results of earlier studies that reported such effects.

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. Our review of this literature identified a number of promising directions worth exploring in future research. To understand the effect that assortment size can have on choice, it will be essential to consider the interaction between the broader context of the structure of assortments—beyond the mere number of options available—and the decision processes that people adopt.

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