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# Planning and Conducting Experimental Advertising Research and Questionnaire Design

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**We give an overview of the most important decisions to be taken when planning and conducting experimental advertising research. Based on previous research and state-of-the-art academic insights, we discuss good methodological practices with respect to fleshing out the contribution of a study, developing and testing experimental stimuli, selecting appropriate samples, collecting data, designing questionnaires, deciding which variables and measures to include, and determining scale measurement. This set of guidelines should enable experimental advertising researchers to optimize their study designs and to carry them out in a correct way.**

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Advertising research can be divided into two streams that use a different methodology. The first approach is experimental (Eisend 2006, 2010; Schmidt and Eisend 2015). Well-controlled experiments are set up to test the effect of carefully designed manipulations on ad responses and to explain why certain effects occur. The second approach views advertising effects from a modeling angle, for instance, to predict sales or to calculate an optimal media mix (Aleksandrov et al. 2015; Danaher and Dagger 2013; Frison et al. 2014; Naik and Raman 2003; Reynar, Phillips, and Heumann 2010). In line with the latter, the increasing availability of behavioral online data has made “big data” advertising research increasingly possible and important. The nature of big data analysis is radically different from experimental research. While experimental design focuses on causation and explanation of phenomena, big data research focuses on prediction, often

with no or little attention to explanation or causation. Although some have announced the end of experiments, hypotheses, and theories (see Anderson 2008), others warn of the pitfalls of big data, such as bias in collection and interpretation (Bottles, Begoli, and Worley 2014); validity, transparency, replicability, and interpretation of big data (Lazer et al. 2014; Boyd and Crawford 2012; Bollier and Firestone 2010); and lack of theoretical considerations (Bottles, Begoli, and Worley 2014). Therefore, while big data may imply a shift toward advertising research that combines big data analysis, modeling, and experimentation, experimental research and small data will remain indispensable.

The purpose of this overview is to discuss the most important decisions when planning and conducting experimental advertising research. Based on previous research and state-of-the-art academic insights, we suggest correct methodological practices with respect to different stages and components of the process of planning and conducting experimental research.

As with any other type of research, experimental advertising research should contribute to advertising theory and practice. This aspect is discussed in the first section. One of the most important decisions a researcher has to make is the development of advertising stimuli to be used in the experiments. In the second section we discuss what should be taken into account to end up with realistic and well-controlled stimuli. These stimuli are then tested in adequate samples of experiment participants. In the third section we discuss issues of sample selection, such as randomness, representativeness, relevance, size, and subsample equivalence.

A large part of this overview is devoted to data collection and measurement. We propose a suitable composition and measurement sequence for an experimental questionnaire, and we extensively discuss issues such as the briefing of participants, manipulation checks, mediating and moderating variables, dichotomizing, replication factors and robustness checks, and quality control measures. Variables in experimental advertising research are often measured by means of rating scales. In the final section, we deal with decisions regarding selecting

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or developing such scales. We discuss content and construct validity, single- versus multiple-item scales, reversed item practices, response style effects, number of scale points and scale polarity, numbering, and labeling. This set of guidelines should allow experimental advertising researchers to optimize study designs and to carry them out in a correct way.

To a large extent these guidelines are generic and apply to all experimental research. However, the advertising context and its constant evolution requires more attention to certain aspects of research design that are particularly relevant for advertising. For instance, stimulus and context realism become increasingly important. Advertising research should test its propositions in contexts that are ecologically valid and thus relevant for theory development and practitioners (Roynes-Stafford 2016). This guideline pertains not only to the advertising stimuli themselves but also to the context in which they are embedded. New advertising formats and contexts, such as in-game advertising, native advertising, content marketing, advertising integrated in social media, virtual/augmented reality ads, geo-based advertising, SoLoMo promotions, and so on, require more complicated and realistic stimuli that reflect the real world of advertising and its contexts. To make research practically meaningful, advertising researchers should think not only in terms of general theories but also in terms of contextualization; that is, how will different target groups respond to different types of messages across different media and in different situational circumstances (Laczniak 2015)? In this respect, it is important to bear in mind that the growth of Web 2.0 has drastically changed consumers' situational circumstances. Respondents are, for example, increasingly likely to be multitasking during ad exposure (see Angell et al. 2016; Duff and Sar 2015) and to have immediate access to online reviews of advertised products (see Foos, Keeling, and Keeling 2016).

Ecological validity also implies the selection of relevant and externally valid samples. Today, significant amounts of real-life (social media) behavioral data are available that can be used in experimental studies. However, these data are often generated in real-life situations (also implying measures about real-life ads), which hampers stimulus manipulation and sample control. Much experimental advertising research is carried out using online surveys with online panel participants (such as Amazon's Mechanical Turk or Prolific), which increases issues of controllability of the data collection process and "careless responding" that should be monitored.

There is, or should be, an increasing concern about the ethicality of some (novel) advertising practices. Integrated (online) advertising formats collect data that may pose a threat to the privacy of individuals. Integrated formats such as in-game advertising, native advertising, or content marketing may be considered unethical in their own right. This concern should be reflected in the measures that are part of advertising research that tries to explain the mechanisms behind consumers' responses to advertising stimuli, such as privacy issues,

persuasion knowledge, and so on. New advertising formats are increasingly aimed at behavior rather than evaluative responses, such as brand attitudes. This should also be reflected in dependent variables used. Activation measures, such as electronic word-of-mouth (eWOM) intentions, liking, sharing, and commenting on social media, become increasingly important. These particular advertising research issues are also revisited in later sections.

## CONTRIBUTION TO THEORY AND PRACTICE

Although fleshing out the contribution of a study is not part of its practical operationalization, it is an important first step in the planning process. Publishing an article starts with developing a relevant research question that provides a substantial contribution, which is then investigated by means of a solid and properly conducted research design. Many papers are rejected due to lack of meaningful contribution. Unlike many other fields, in advertising research a contribution needs to be meaningful not only for advertising theory but also for advertising practice. Advertising researchers thus have to think carefully before they set up a study. What will we learn that we did not know already? How does it build on previous research and add meaningful insights to the body of knowledge about how advertising works? Why is what is studied relevant, and for whom? Why is it useful for practitioners?

These contributions need to be substantial and not incremental. One way to find that out is to conduct a thorough literature review before setting up a study. It is very sad to discover that someone else already published a study similar to the one you are conducting. Even though the value of replication studies for the development of science is generally acknowledged, and although some journals have a "replication corner," pure replication studies do not tend to be well received by most marketing or advertising journals (Kerr, Schultz, and Lings 2016). Indeed, only 2.9% of the articles published in the *Journal of Advertising*, *Journal of Advertising Research*, *International Journal of Advertising*, and the *Journal of Current Issues and Research in Advertising* from 1980 to 2012, and only 1.2% of the articles published in the *Journal of Marketing*, *Journal of Marketing Research*, and *Journal of Consumer Research* between 1990 and 2004, were pure replication studies (Evanschitzky et al. 2007; Park et al. 2015).

Contributions such as "This is the first time this is tested in Belgium," "The authors live in Poland and Spain and therefore these two countries are studied," or "No one has tested in-game brand placement effects in fantasy games yet" are meaningful only if the researcher can come up with a compelling argument why there is added value in the study, beyond convenience or satisfying one's curiosity. For instance, regarding the fantasy game example, replicating response mechanisms that have already been studied in, say, sports games, may be

theoretically and practically interesting because of the very nature of a fantasy game. These games are set in a coherent fantasy world, with myths and legends for characters (e.g., elves, trolls), special settings (e.g., castles, dungeons), specific artifacts (e.g., rings, swords), and so on (Schultze and Rennecker 2007). Brands are not part of such a fantasy world. This incongruence could lead gamers to respond very differently to ads in fantasy games than to ads in sport games (Verberckmoes et al. 2016).

## STIMULI

Experimental research in advertising often uses manipulated advertising stimuli to test the effect of these manipulations on consumer responses. Therefore, developing correct and relevant test stimuli is of the utmost importance.

### Realism and Control

Advertising stimuli used in experiments have to strike a balance between realism and control. They have to be professionally made, of high quality, and realistic; in other words, they have to look like real advertisements. Stimulus and context realism become increasingly important due to the constant evolution of advertising and advertising contexts. Advertising research should test its propositions in advertising contexts that are relevant for theory development and practice. New advertising formats and contexts, such as in-game advertising, native advertising, content marketing, advertising integrated in social media, virtual/augmented reality ads, geo-based advertising, and SoLoMo promotions, require realistic stimuli that reflect the real world of advertising. For instance, testing the effects of in-game advertising realistically requires that the ad is integrated into a game (e.g., see Cauberghe and De Pelsmacker 2010). The researcher could develop static stimuli ("stills" of the game) in which in-game ads are integrated in different ways, but they would be far away from a real gaming experience, and it is then difficult to validly test the effect of, for instance, interactivity or intrusiveness. Vignettes—that is, short, carefully constructed descriptions of a person, object, or situation that is shown to respondents in order to elicit judgments about these scenarios (Atzmüller and Steiner 2010)—could be a valid alternative for real stimuli when creating real stimuli is too complex. They can provide useful insights about the behavior and decisions of consumers in real-life situations (Schoenberg and Ravdal 2000). However, they are still not realistic. In sum, developing realistic stimuli may sometimes be problematic, because this can be complex and/or expensive, but realism is an important consideration in advertising research.

On the other hand, stimuli should always be developed to clearly reflect a desired manipulation. The standard procedure is to change only those elements that are needed for the manipulation and keep the rest of the advertisement the same across manipulated conditions. Manipulation control becomes easier if the rest

of the ad is not too distracting or confusing. Complex, sophisticated, or creative ads may well look more professional, but they can confuse viewers and distract them from the components of the stimuli that are essential for the manipulation. A good stimulus for experimental advertising studies is thus well made and realistic but at the same time as simple as possible. Nowadays, lots of behavioral data are available that are often generated in real-life situations, with real ads (e.g., on social media). In those circumstances, control over stimulus manipulations becomes an increasingly challenging task.

In some cases, the nature of the study involves creating stimuli containing manipulated components that are, by themselves, potentially confounding. For instance, in a cross-cultural study, Rajabi et al. (2015) manipulated the endorser of a product to be either local (an actor from Belgium, Iran, or India), or global (Leonardo DiCaprio). Obviously, a pretest was used to select these endorsers, to check whether they were equally well-known in the country in which they were used in the study. But celebrities can be different in more ways than just being famous. Prior research shows that the effectiveness of endorsers also depends on how attractive, trustworthy, and credible they are (Erdogan 1999). If, say, the local celebrity is also perceived as more attractive or trustworthy than the global one, some of the effects measured may be due to these differences in attractiveness or trustworthiness, and not to the localness or globalness of the endorser. Such potential confounds have to be carefully pretested, and stimulus elements have to be selected that perfectly reflect the desired manipulation, without any other confounds. These potentially confounding elements should also be measured in the main experiment so that they can be used as covariates in the analysis to neutralize their potentially confounding effect.

### Real or Hypothetical Brands?

In developing realistic advertising stimuli, brand names have to be mentioned and/or brand identifiers (logos, slogans) have to be shown. In some cases, existing brand names have to be used for obvious reasons, for instance, in brand extension studies (e.g., Dens and De Pelsmacker 2010, 2016); in studies in which one of the independents is the distinction between existing and novel brands (e.g., De Pelsmacker and Janssens 2005; Campbell and Keller 2003); in brand typicality studies (e.g., Goedertier et al. 2015); or for purposes of comparing the effects of existing versus novel brands or of strong versus weak brands (e.g., Dahlén and Lange 2005). In these studies, existing brands are carefully selected on the basis of their suitability for the research objective at hand.

Yet in many cases a brand has to be used to create realistic advertising stimuli, but the brand in itself does not play a role in the study. In those cases, it is advisable to use hypothetical (new) brands to avoid potentially confounding effects of previous exposure or experience with existing brands (Schneider and Cornwell

2005). For instance, many participants may be aware of existing brands and may have associations with them, or positive or negative beliefs about them, or feelings and attitudes toward them. In measuring responses to advertising stimuli, confounds may easily invalidate the results and conclusions. For instance, memory effects (e.g., brand recall) may be seriously inflated by using existing brands, as a result of which it becomes impossible to distinguish the effect of the manipulations and preexisting brand awareness. The same goes for beliefs, feelings, and attitudes. Obviously, if the same brands are used in all stimuli, that should not affect differences in effects, but effect sizes of the stimuli themselves may be flawed.

If using existing brands is deemed desirable, the best solution to neutralize the confounding effects of existing brands (or ads, for that matter) is to base the study on many (existing) ads and/or brands (for instance, see Pham, Geuens, and De Pelsmacker 2013), but this may be impractical or expensive. Another solution is to use a control group that is not exposed to any of the experimental stimuli. For instance, Dens et al. (2012) studied the effect of prominence and plot connection of eight brands placed in two movies on, among others, brand attitude. The existing brand attitudes for these eight brands were measured in a comparable control group that had not been exposed to the movies. In the analysis the mean attitude score of each brand in this control group was subtracted from the postscore of the same brand in the test group to form the dependent variable. As a third alternative, sometimes premeasures of brand attitudes in the same sample of participants are used. However, this is not an ideal practice because participants may take their preattitude scores into account when giving their postscores (i.e., the interactive testing effect; Verbeke, Farris, and Thurik 1998). This could be partly avoided by also collecting prescores for several filler brands and/or gathering the prescores well before the main study is carried out, but even then the interactive testing effect may affect postmeasurement. Even history effects can be induced in case an external event (repositioning, negative publicity, new campaign) takes place in between the prescore measurement and the actual study.

Also, new brands to be used in advertising studies have to be carefully pretested and selected. The variables on which hypothetical brands should be pretested partly depend on the context. In any case, elements such as (false) recognition and undesirable or biasing connotations should be avoided. Indeed, even new brand names or logos can evoke undesired responses or associations. For instance, a brand name can evoke a certain meaning, or people may think they know the brand. This is all the more true in cross-cultural research.

## SAMPLES

### Randomness, Representativeness, and Relevance

Drawing conclusions based on experimental studies is based on statistical testing. Strictly speaking, no statistical inferences can be made without the selection of a random

sample from a well-defined population to ensure that sample characteristics differ only by chance from the population characteristics (Menon 1993). This implies that samples and subsamples should be obtained by random sampling. In advertising research, this is usually not the case, be it because the population is not well described or there is no sampling frame of the population or it is impossible or cumbersome to draw a random sample. Moreover, even if it would be possible to select a random sample for a study, it would not guarantee representativeness of a certain population. As Shaver (1992) points out, randomness and representativeness are related only to the extent that there is repeated random sampling to ensure representativeness in the long run, not that a single random sample is wholly representative of the population.

Needless to say, the samples in most advertising studies are neither randomly drawn out of a well-specified population nor representative of such a population. Nevertheless, in practice, they are generally accepted for theory testing, as long as they are relevant in the context of the study. Relevance of a sample means that there is a fit between the sample, the product, and the consumption situation related to that product. For instance, using student or teenager samples to test the effect of public awareness campaigns against binge drinking is appropriate. For testing the effect of advertising for extending a brand of lawn mowers, a sample of people who have lived only in apartments is inappropriate. And recruiting men for testing feminine hygiene products ads is, of course, irrelevant.

For convenience and cost reasons, many advertising studies are conducted in samples of university students. Therefore, advertising research is sometimes labeled “the science of the undergraduate marketing student” (James and Sonner 2001). Many authors have argued against using student subjects by presenting comparisons of the differences between “college students” and “real people.” These studies have almost universally found some differences in the results obtained from the two samples. Students are, in relevant ways, different from “ordinary” consumers. They usually have less money, their lifestyle is atypical for the consumption situation of most consumers, and they usually have less experience (or no experience) buying most consumer products. These differences may bias their perceptions, motivations, and preferences. Some authors have argued that these differences are small enough to justify using students as research subjects, but most conclude that students are not good surrogates for adult consumers (for a discussion and overview, see James and Sonner 2001). Calder, Phillips, and Tybout (1981) make the distinction between what they call “effects application” and “theory application.” The former aims at statistical generalization of a theory. In that case, a close correspondence between the research sample and the population, and statistical sampling, is required. Student samples would often be inappropriate in this case. The latter aims at theory falsification, for which any (preferably homogeneous) sample in the theory domain is suitable. In this case, student samples could be used.

Besides student samples, researchers increasingly are making use of samples drawn from crowdsourcing websites, of which Amazon's MTurk is the largest and the one most often used (Shank 2016). MTurk offers clear advantages, such as a low cost, easy handling, a fast turnaround time, a large and heterogeneous respondent pool, respondents from different nationalities (allowing cross-cultural comparisons), respondents from hard-to-reach target groups, a good response rate for follow-up studies, truly random allocation to experimental conditions, and importantly, reliable, consistent, and high-quality data (Buhrmester, Kwang, and Gosling 2011; Berinsky, Huber, and Lenz 2012; Mason and Suri 2012; Rand 2012; Shank 2016). However, although the MTurk population is diverse, it cannot be considered representative of the entire population. In general, MTurk respondents are younger, more educated, less employed, less religious, more liberal, have a lower income, and consist of relatively more females than the general U.S. population (Berinsky, Huber, and Lenz 2012; Mason and Suri 2012; Paolacci and Chandler 2014; Paolacci, Chandler, and Ipeirotis 2010; Shank 2016). While results obtained with an MTurk sample appear to correspond well with other online and offline samples (Shank 2016), "super-Turkers" (i.e., quasi-professional respondents with excessive experience) and "spammers" (i.e., respondents interested only in maximizing their pay rate) represent a major concern (Deetlefs, Chylinski, and Ortmann 2015). Deetlefs, Chylinski, and Ortmann (2015) recommend beginning with a self-report question on experience and several control questions to enable researchers to screen for and discard both types of respondents. We discuss this further in the Quality Control section. If these issues are resolved, MTurk usually provides good samples for experimental research.

In any case, researchers should keep in mind that ensuring some form of representativeness and randomness is desirable, and relevance is imperative.

### Sample Size

How large should subsamples per experimental condition be? A study must be of adequate size. It must be "big enough" to find effects of scientific relevance to be also statistically significant. However, it should also be not "too big," such that an effect that is not scientifically relevant becomes statistically significant (Lenth 2001). Many experiments in consumer behavior and advertising are conducted in small groups, often even as small as 20 participants or less per experimental condition. As argued in the next section, small subsamples can be problematic in terms of equivalence, but they also lead to less power in statistical testing. Formally, the power or sensitivity of a test is the probability that the test correctly rejects the null hypothesis ( $H_0$ ) when the alternative hypothesis ( $H_1$ ) is true. In other words, it is the probability of accepting the alternative hypothesis ( $H_1$ ) when it is true, that is, the ability of a test to detect an effect, if the effect actually exists. Weak power leads to type II errors: existing scientifically

meaningful effects cannot be confirmed. Consequently, differences between scores in different experimental conditions will have to be quite substantial before they are statistically significant. On the other hand, large subsamples lead to higher statistical power, and even relatively small and scientifically insignificant differences between scores may be statistically significant. However, to be meaningful, differences should be not only statistically significant but also substantial enough to warrant conclusions about the acceptance or rejection of hypotheses. In other words, too much power can lead to type I errors: a nonexistent effect is found to be statistically significant.

Typically, in experimental advertising research, one wants to test the significance of the difference between scale scores of different experimental conditions. Often, this difference is relatively small, so sufficiently large subsamples are needed to achieve sufficient statistical power. For instance, for a test comparing two independent samples (as in a simple effects test in a between-subjects design) in which the scores on a construct are 3.0 and 3.6 ( $SD = .80$ ) on a 7-point scale, the significance level required is .05, and the required statistical power is 80%, the required size for each subsample is 28. The higher the desired power, the smaller the difference in scores, and the larger the standard deviations, the larger the samples need to be. For instance, in the given example, if the  $SD = 1.00$ , a sample size of 41 is needed (see [www.stat.ubc.ca/~rollin/stats/ssize/n2.html](http://www.stat.ubc.ca/~rollin/stats/ssize/n2.html)). Obviously, the researcher does not know what the scores and their standard deviations will be. However, to ensure sufficient statistical power, a size of 30 to 40 participants per experimental condition seems to be indicated.

If in the study a number of variables are measured that will be used as moderators in the analysis, larger subsamples per experimental condition are needed. For instance, assume that the researcher sets up a two-condition experiment. In one condition, participants are exposed to an advertisement for a new product in which a celebrity is used as an endorser, and in the second condition the participants see an advertisement for the same product with no visual but with a lot of product information. A total sample of 60 would be sufficient to test the differences in responses between the two conditions (30 per condition). However, if during the experiment participants are asked for their gender, and in the analysis the researcher wants to test whether gender moderates the effect of type of stimulus on the attitude toward the product (a gender [2]  $\times$  stimulus type [2] interaction), then 60 participants will not be enough. In terms of analyses there are now four conditions instead of two, and simple effects testing within the interaction would be based on subsamples of, on average, 15 participants.

### Subsample Equivalence

Comparability or equivalence of samples across experimental conditions is extremely important for a valid experimental design. The traditional solution for this is to randomly assign participants to experimental conditions, but that may not be enough. Especially in small (sub)samples, randomness does not necessarily lead to

sample equivalence, especially in populations that are heterogeneous in terms of important and relevant characteristics in the context of the study. For instance, if the study is done in a sample of business undergraduates (assuming that group is a relevant sample for the topic under study), the participants will at least be relatively homogeneous in terms of age, level of education, knowledge about business administration, and certain lifestyle elements. In this case, random allocation of participants to experimental conditions may lead to sufficient subsample equivalence. However, if the population of interest is very heterogeneous in terms of age, level of education, profession, and so on, and all these are relevant characteristics in the context of the study, then randomly assigning individuals to small experimental subsamples will not necessarily lead to subsample equivalence in terms of several of these characteristics.

There are three solutions for this problem. The first is to increase sample sizes. The second is to select a stratified sample, that is, define categories of participants on the basis of variables that are relevant in the context of the study, decide which part of the samples should be in which category, and then allocate participants accordingly. In face-to-face or computer-aided lab experiments, this can be easily organized, but this can also be easily programmed in online survey tools. The third solution is to make use of matched samples. That is, if a higher-educated, middle-aged woman is assigned to one condition, a similar higher-educated, middle-aged woman should be assigned to the other condition(s).

Nowadays, lots of data that are generated in real-life situations are available that can be used in experimental studies. In

such circumstances, control over the composition and equivalence of samples across experimental conditions becomes increasingly challenging.

## DATA COLLECTION

After carefully crafting stimuli and/or primes and selecting test samples, participants are exposed to the stimuli and their responses and characteristics measured. Research results can be seriously confounded by the use of inappropriate measurement procedures. Therefore, the content and sequence of the measurements is very important. Figure 1 outlines a recommended sequence of measures in a questionnaire.

## Briefing

Upon arrival in the lab or when starting to fill out a questionnaire, respondents should receive a proper introduction or briefing. This briefing usually entails some form of deception to conceal the real goal of the research (i.e., a cover story). Misleading respondents is often necessary to avoid demand effects: If respondents are aware of the researchers' objectives, some may decide to counteract and cross these objectives, while others may take on the role of a "good subject" and behave in line with the researchers' hypotheses (e.g., Orne 1962; Rosnow and Rosenthal 1997). Either way, no true response to the manipulated stimuli will be obtained. To enhance the credibility of the cover story, researchers often use filler items and/or rely on the independent studies paradigm. The latter refers to making respondents believe they are participating in several unrelated studies. Tessitore and Geuens (2013), for example, disguised that they were investigating the effectiveness of product placement by telling respondents they would be participating in two separate experiments. The so-called first experiment was allegedly from Researcher X, who was interested in reactions of viewers to specific movie fragments, while the so-called second experiment was from Researcher Y, who was interested in the strength of different brands.

Notwithstanding using a good, believable cover story, at the end of the study a suspicion probe should check whether respondents believed the ostensible purpose of the study. The use of such a probe enables researchers to identify (and possibly eliminate) those participants who figured out the real purpose (Taylor and Shepperd 1996). After the suspicion probe, some form of debriefing is necessary, especially when participants have been given false information as part of the briefing, or when participants' psychological states have been manipulated. In a debriefing, the researchers explain the true purpose of the study to the participants and explain when and how they were manipulated.

While demand effects may never be completely ruled out, they tend to pose a bigger problem for within-subjects designs than for between-subjects design (Pany and Reckers 1987). In a between-

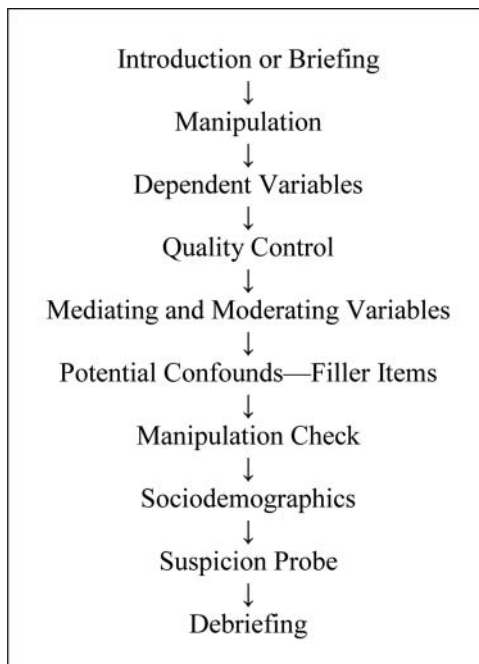


FIG. 1. Sequence of measures in an experimental advertising study.

subjects design, participants are exposed to only one manipulated stimulus. In a within-subjects design, participants are exposed to several stimuli. Researchers sometimes decide to use a within-subjects design because it requires fewer respondents and has more power, as individual differences are controlled for (Kirk 1982). However, exposing respondents to multiple treatment levels of an experimental factor (e.g., in an effort to investigate the effectiveness of a celebrity endorser, show respondents an ad for Brand X with an expert endorser, as well show them an almost identical ad for the same brand with a noncelebrity endorser) makes it very easy for respondents to figure out what the real purpose of the study is.

### Manipulation (Check)

The briefing introduction is usually followed by exposing the participants to a manipulation. This manipulation is often embedded in the advertising stimulus itself, but it can also consist of a prime or priming instruction. For example, to investigate the role of construal level on ad persuasiveness, researchers could manipulate the ad in such a way that it evokes an abstract versus concrete construal (e.g., by referring to the respondents' home country versus to a distant country), or they can prime spatial distance externally by having respondents describe their opinion about getting a degree in their home country versus a distant country before they are exposed to the focal advertising stimulus (e.g., see Kulkarni and Yuan 2015).

After exposure to the manipulated stimulus, the dependent variables are measured, such as feelings, thoughts, attitudes toward the stimulus and the brand, and behavioral intentions. It is advised to order these dependent variables in reverse-causal order, especially if the researchers are interested in the relationship between these variables, for example, by first measuring behavioral intentions, then brand attitudes, and then stimulus-related responses, such as the attitude toward the ad, feelings, and thoughts.

Irrespective of what type of manipulation is used, to avoid any bias a manipulation check can cause it is better to insert the manipulation check after measuring the dependent, mediating, moderating, and control variables. Some researchers choose to have a manipulation check immediately after the manipulation, but this is usually not a good idea. For example, if a researcher is interested in the effectiveness of humor in advertising, probing to what extent respondents thought the ad was humorous before measuring the dependent variables (often attitudes and intentions) could easily induce demand effects but could also make respondents take into account the humorous aspect to a larger extent than they would if their attention was not drawn to it. Prior research shows that context factors, such as manipulation checks, alter the relation among variables (Richter 2010). For example, when investigating ease-of-retrieval effects (that is, the effect experienced ease by which information can be brought to mind has on attitudes, judgments, and preferences; see Schwarz, Bless, et al. 1991),

studies that found evidence for ease-of-retrieval effects under low-elaboration conditions always included the manipulation check before the dependent variables. The studies that measured the manipulation check after the dependent variables did not find evidence of ease-of-retrieval effects in this same elaboration condition—the reason being that, if the manipulation check preceded the dependent variables, this procedure increased the likelihood that respondents relied on ease of retrieval when making their judgment (Kühnen 2010). For the same reason, it is also advised to measure mediating, moderating, and controlling variables after the dependent variables (Kühnen 2010).

In case the time lapse between exposure to the manipulation and the manipulation check is quite large, the stimulus or prime can be shown again to reduce the concern that respondents do not clearly remember it anymore. Another possibility is to not use a manipulation check in the main study but extensively pretest the manipulated stimuli or primes in a separate study, for instance, when manipulation checks in the main study can be confounded by previous measures or questions. However, in the latter case, the researcher never knows for sure that the manipulation also worked properly in the main study. Also, using a manipulation check in the main study provides the researchers the possibility to discard those respondents from the analyses for whom the manipulation was clearly ineffective.

Which elements of a manipulation should be tested? To be on the safe side, a conservative rule would be to use manipulation checks (or pretest manipulations) whenever there is doubt about how “obvious” manipulations are. Manipulation checks are not needed for obvious and blatant facts, such as colors, man/woman, numbers (e.g., number of arguments or number of people in an ad).

### Mediation: Measurement of Mediation and Experimental Causal Chains

The basic premise of experimental advertising research is that it tests the relationship between one or more predictor (independent) variables (e.g., different advertising stimuli) on one or more criterion (dependent) variables (e.g., brand attitude, purchase intention). However, behind this straightforward logic there may be more than meets the eye.

Behind the relation between a predictor and an outcome (criterion), there may be a mechanism that explains the effect. For instance, personalized advertisements (predictor) lead to a better attitude toward the ad, because personalized advertisements are perceived as more relevant (De Keyser, Dens, and De Pelsmacker 2015). In other words, relevance “explains” the relation between personalization and brand attitude. In this example, relevance is a mediator. A mediator accounts for the relation between the predictor and the criterion. Mediators explain how a predictor influences a criterion. They clarify what would otherwise remain a black box in terms of why a



manipulated stimulus predicts an outcome. Consequently, experimental advertising research should include potential mediators. They are important for theory building.

If mediators are measured variables, Hayes's (2013) PROCESS macros have become the standard approach to test the (serial) mediation process. However, Spencer, Zanna, and Fong (2005) warn against the use of measurement-of-mediation designs. They should be used only when the proposed underlying process (i.e., the mediator) is easy to measure and difficult to manipulate. A more powerful methodological approach to assess mediation is the use of experimental causal chain designs, that is, a series of experiments that demonstrate the proposed causal chain. For example, to provide evidence that downward versus upward head movements increase preference-decision consistency because looking down (versus looking up) induces a concrete construal level (versus abstract construal level), Van Kerckhove, Geuens, and Vermeir (2015) showed that (1) looking down versus looking up leads consumers to adopt a lower (higher) level of construal and (2) priming a lower (higher) level of construal increases (decreases) preference-decision consistency. Spencer, Zanna, and Fong (2005) argue that experimental causal chain designs are recommended whenever the mediator is easy to manipulate. The reason is that manipulating both the independent variable and the mediating variable allows strong inferences about the causal chain of events to be made.

### Moderation and Median Splits

The relationship between a predictor and a criterion is also subject to boundary conditions. What may be a strong and positive relationship in some circumstances may be a weak or negative relationship in other circumstances. For instance, highly anxious people are strongly influenced by fear appeals, whereas less anxious people are less affected (De Meulenaer, Dens, and De Pelsmacker 2015). In this example, anxiety is a moderator. A moderator affects the direction and/or strength of the relation between a predictor variable and a criterion variable. In experimental research, moderator effects can be represented as an interaction between an independent variable and a factor that specifies the conditions for its operation. Moderators specify when and to what extent certain relations between an independent (predictor) and a dependent (criterion) will hold. Consequently, moderators are also crucial for theory development and for practice. They bring nuance to a relationship and shed light on the boundary conditions of an effect.

A moderator can be a qualitative (e.g., sex, race, class) or quantitative (e.g., level of anxiety) variable. Qualitative moderators can be easily added as an extra factor in analyses of variance (ANOVAs). However, for quantitative moderators it also used to be standard procedure to median-split the scores on these moderators and use the dichotomized variable as an extra factor in ANOVAs. Although the question of median-splitting a

continuously measured variable is an analytical decision and, as such, outside the scope of the present overview, it has to be taken into account in setting up an experiment.

Whether it is good practice to median-split or dichotomize a continuous variable for further analysis has been a matter of debate for a long time. It is common knowledge that dichotomizing a continuous variable leads to loss of information (individual scores are dichotomized) and loss of statistical power. Consequently, median splits lead to greater type II errors: scientifically meaningful effects will not be statistically significant. Some scholars argue this is not a big deal; they argue that it is not problematic for an effect not to be found, that it just makes studies more conservative. Moreover, median splitting does not produce spurious results (i.e., type I errors). That is, effects that are not there will not be statistically significant (Maxwell and Delaney 1993). Median splits, they further argue, are warranted if the researcher is mainly interested in group effects rather than individual effects. Finally, analyses performed on categorized data, typically ANOVAs, would be easier to interpret and understand than moderated regression analysis typically used on the combination of manipulated factors and continuous moderators (Iacobucci et al. 2015).

However, the bulk of methodologists have argued against median splits for quite some time (e.g., Fitzsimons 2008; Irwin and McClelland 2003). First, they argue that dichotomization discards the potentially rich variation in individual scores. For example, subjects scoring just above the median, moderately above the median, and substantially above the median are all treated as identical. Importantly, they argue that, in many cases, median splitting does increase the risk of type I errors (Rucker, McShane, and Preacher 2015). Others also argue that loss of statistical power due to median-splitting a continuous variable is not a decision to be "conservative," but rather a decision to be less persuasive: more statistical power leads to bigger effect sizes and more persuasive results (McClelland et al. 2015). All in all, there is a growing consensus that moderators should not be dichotomized, and using continuous measures to test moderation is the best research practice.

Moderated regression analyses, using a combination of categorical manipulated factors and measured predictors and moderators, has become the normative standard of analysis. Hayes's PROCESS macros have become the standard approach to moderation analysis. To interpret the results, spotlight analyses (Aiken, West, and Reno 1991) or floodlight analyses (Spiller et al. 2013) can be used.

Moderating and mediating variables often constitute the real contribution of a study.

### Replication and Robustness Check

From conventional statistical knowledge we can infer that, with a conventional significance level of 5%, one out of 20 times a true null hypothesis will be rejected (i.e., type I error). Taking into account that for most studies multiple

statistical tests are carried out, that authors usually do not report all the analyses run, and that papers with significant findings are more likely to be submitted to and accepted by journals, the likelihood on type I errors in published papers is much higher than the 5% statistical tests would suggest (Eisend, Franke, and Leigh 2016). Therefore, to be able to trust the findings of a study, replication of the results in follow-up experiments is crucial and should be an integral part of each research design (Uncles and Kwok 2013).

Next to replicating the main findings in a different sample and/or using different stimuli, follow-up experiments can be designed in such a way that they rule out confounds or rival theoretical explanations and allow investigation of mediating and/or moderating processes. For example, to provide robust proof that a symbolic versus physical product presentation leads consumers to purchase less unhealthy products in an online store than in a brick-and-mortar store, Huyghe et al. (forthcoming) set up a series of experiments in which each experiment replicated the main effect (i.e., consumers buy less vices online than offline) but ruled out other explanations and confounding variables such as (1) being exposed to the unhealthy products or not, (2) store atmospherics, (3) payment method, (4) order lead time, and (5) shopping motive.

As mentioned, next to mediating and moderating variables, potential confounding effects should be controlled for. As such, variables like age, gender, mood, brand/product familiarity, and brand/product usage are often measured and included in the analyses as covariates. Control variables should be included in the analyses only if there are solid theoretical or logical reasons or if previous research provides indications to do so.

### Quality Control

Data collection in experimental studies are done in face-to-face settings (e.g., paper-and-pencil forms or in a computer lab) or, increasingly, online. Online studies usually have the advantage of being fast and cheap, and involving a more representative sample. Furthermore, as a self-administration technique, online studies offer respondents the possibility to work at their own pace, and they avoid the presence of an interviewer (and a corresponding interviewer bias) (e.g., Catania et al. 1996; Davis et al. 2010). Importantly, the presence of an interviewer may lead respondents to answer questions in a different way, for example, by expressing a clearer or more extreme opinion (Jordan, Marcus, and Reeder 1980; Weijters, Schillewaert, and Geuens 2008). Interviewer presence may also generate less honest answers, as it increases respondents' inclination to impression management (i.e., social desirability) (Krumpal 2013; Paulhus 2003). To control for socially desirable responding, it may be wise to include a scale such as the Balanced Inventory for Desirable Responding (for details, see Steenkamp, De Jong, and Baumgartner 2010).

On the other hand, researchers should realize that they have less control over the conditions in which respondents fill out an online questionnaire, due to the physical distance, lack of personalization, and distraction. In a face-to-face setting, participants can be briefed and monitored in real time. Lab experiments may therefore be better for preserving internal validity. For example, to investigate the impact of consumers' mood on their attitude-behavior consistency, Elen et al. (2013) manipulated mood by exposing respondents to either a negative or a positive movie clip before they were asked to make choice decisions. If such a study is run online, respondents that take a break in between the mood induction and completing the dependent variables become useless, as the mood manipulation will not last long enough to affect the dependent variables. Also, multitasking (e.g., listening to music or watching TV while completing the questionnaire), being in the presence of friends or relatives or distracting or annoying background noise, and so on, may all render manipulation efforts ineffective. As an example, in a recent survey, MTurk respondents admitted to frequently multitasking, with 18% of them watching television, 14% listening to music, and 6% instant messaging while filling out a questionnaire (Chandler, Mueller, and Paolacci 2014).

Irrespective of whether the study is run online or in a lab, a number of controls should be built in to identify and eventually remove careless or inattentive respondents. This safeguard is important, because data from such responses can invalidate the results of a study. Various control mechanisms have been proposed (for an extensive overview, see Meade and Craig 2012). For instance, most questionnaire software allows researchers to set minimum and maximum duration times for exposure to stimuli to ensure that participants do not click away from the stimulus too quickly. Most questionnaire software also allows measurement of the time that participants spend on each page and how long it takes them to complete the full questionnaire. The researcher can then decide later on to discard results from those participants who completed the survey too quickly or took a break after the experimental manipulation. For example, Deetlefs, Chylinski, and Ortmann (2015) excluded respondents who filled out questions faster than readable, as well as respondents who needed time more than three standard deviations away from the mean.

Another strategy is to try to prevent inattentive responses via instruction sets. However, researchers should realize that not all participants are diligent in reading and following instructions. Participants who do not read the questions and/or fail to follow instructions induce noise and threaten the validity of the data. To be able to identify such participants, Oppenheimer, Meyvis, and Davidenko (2009) suggest inserting an instruction manipulation check. That is, a question embedded within the other questions, which is similar in length and response format (e.g., also a 7-point Likert scale). However, in contrast to the other questions for which respondents have to report their own opinion, they are asked to follow an

instruction, that helps identify respondent care in response or to flag those who are not carefully reading the item. Examples include social desirability and nonsensical or “bogus” instructed response items (e.g., “If you are completing a questionnaire now, respond with a six for this item”; “To monitor quality, please respond with a two for this item”). Another option is to include self-report measures of response quality placed at the end of a survey (“In your honest opinion, should we use your data?”) (Meade and Craig 2012). In addition, control questions can be included that probe what the stimulus, scenario, or instruction was about to get an idea of which respondents failed to pay sufficient attention. Respondents who fail these checks should be excluded from further analyses.

Several indices can also be computed post hoc for identifying careless response (Meade and Craig 2012). Consistency indices match items with control items that are highly similar, be it in meaning or based on known empirical correlations among these items. A lack of consistent responding is then indicated by deviation among responses to similar items. Response pattern indices identify persons responding too consistently to items measuring theoretically distinct constructs. These indices are typically computed by examining the number of consecutive items for which a respondent has indicated the same response option. Excessive utilization of a single response option can flag careless responding. Again, these respondents should be removed from the analysis. Finally, if a respondent pool is used in which some respondents may have excessive experience filling out questionnaires (e.g., super-Turkers), it is advisable to include a question that probes how often respondents participate in academic research. These overexperienced respondents should be discarded from the analyses (Deetlefs, Chylinski, and Ortmann 2015).

## MEASURES

When designing the concrete measures for their variables, researchers have to make several decisions both concerning the items that will be asked and the scale format that will be used to measure these items. We discuss the most important issues related to each in the following sections.

### Decisions Concerning the Items

*Content or construct validity.* Researchers should pay careful attention to use only those measures that adequately represent the construct and thus possess content or construct validity (Nunnally 1978; Rossiter 2002). Put differently, it is important to use measures that really measure what they are intended to measure. Loose definitions and operationalizations of constructs, which embrace several other characteristics besides the intended ones or which do not embrace all aspects of the construct, induce a construct validity problem and leave researchers and readers uncertain of what was actually

measured. For example, recognizing that consumers may have different motivations to purchase innovative products but that most scales take into account two different types of motivations at most, Vandecasteele and Geuens (2010) developed a new consumer innovativeness scale that fully grasps the four types of motivation (functional, hedonic, social, and cognitive) that underlie consumer innovativeness.

According to Rossiter (2002), to achieve content or construct validity, a construct should be defined in terms of an object, attribute, and rater entity. Take as an example how Zaichkowsky (1994) defined “involvement” for her Personal Involvement Inventory, that is, “[a] person’s perceived relevance of the advertisement” (p. 61). The object here is “the advertisement,” the attribute is “personal relevance,” and the rater refers to “a person” or, more specifically, a consumer. Personal relevance, though, should be further defined in terms of its underlying dimensions as Zaichkowsky conceptualized it as consisting of a cognitive and affective dimension (Rossiter 2002).

Fortunately, over recent decades many marketing and psychological scales have been extensively validated and large-scale inventories have been built from which researchers can easily select scales (e.g., Bruner 2015; Bruner, James, and Hensel 2001; Bearden and Netemeyer 1999). In case no validated scales are available, researchers can turn to the well-established methodological guidelines to construct their own scales (Churchill 1979; Peter 1981; Rossiter 2002). But, as mentioned, even when they turn to existing scales, researchers should ascertain that the measure is construct valid.

*Single versus multiple items.* In his influential article, Churchill (1979) states that marketers should develop and use multi-item scales to measure constructs. Since then, standard practice in (experimental) advertising research is to measure often-used constructs, such as the attitude toward the ad (Aad) or the brand (Ab), by means of multi-item scales. To be more precise, researchers have used multiple items to measure the attribute of the construct (e.g., attitude, quality, liking) for the object of the construct (e.g., a company, a brand, an ad). Multiple-item measures are assumed to be inherently more reliable than single-item measures because they enable computation of correlations among items. If these correlations are positive and produce a high average correlation (i.e., a high coefficient alpha), this indicates the “internal consistency” of all the items in representing the presumed underlying attribute (Bergkvist and Rossiter 2007). Moreover, a multiple-item measure captures more information than can be provided by a single-item measure, and a multiple-item measure “is more likely to tap all facets of the construct of interest” (Baumgartner and Homburg 1996, p. 143). However, practitioners favor single-item measures on the practical grounds of minimizing respondent refusal and cost.

On the basis of comparative predictive validity tests, Bergkvist and Rossiter (2007) argue that Aad and Ab could be measured by means of single-item measures and that this could

also be the case for all measures that are “double concrete.” Double concrete means that, first of all, the object of the construct (e.g., an advertisement, a brand, a company) is “concrete singular,” meaning that it consists of one object that is easily and uniformly imagined. Second, the attribute of the construct is “concrete,” again meaning that it is easily and uniformly imagined, such as Aad and Ab. Another argument for single-item measures is the desire to avoid common methods bias. Common methods bias occurs when the correlation between two or more constructs is inflated because they were measured in the same way (e.g., see Podsakoff et al. 2003; Williams, Cote, and Buckley 1989).

However, to measure abstract constructs validly, multiple items are needed. A construct is “abstract” if (1) the object of the construct consists of two or more components (e.g., the materialism value, which has three components—namely, use of possessions to judge success, centrality of possessions in a person’s life, and the belief that possessions lead to happiness), or consists of a set of constituent subobjects (e.g., for job satisfaction, aspects of a person’s job, such as supervisor, coworkers, job duties, workplace technology, and policies) or (2) the attribute of the construct is formed from two or more components (e.g., service quality, with its components of reliability, responsiveness, empathy, and so forth) or elicits and is reflected in a series of mental or physical activities (e.g., most personality states or traits).

Despite Bergkvist and Rossiter’s (2007) argumentation, many researchers have their doubts, and it remains a challenge to convince reviewers and editors of the use of single-item measures for responses to advertising stimuli.

**Reversed items.** Should researchers include reversed items (i.e., items that need to be recoded to show a relation in the same direction with the underlying construct) in their scales or not? This question is not easy to answer, as the practice has clear advantages but also some disadvantages. The use of reversed items can enhance respondents’ attentiveness (Barnette 2000); it can increase construct validity as the construct may be more fully grasped (Carroll et al. 1999); it can counter respondents’ tendency to agree (i.e., acquiescence bias) (Paulhus 1991); and it can lower respondents’ confirmation bias (in the sense that the inclusion of reversed items makes it harder for the respondents to guess what the researchers are exactly after; Weijters and Baumgartner 2012). However, the use of reversed items can also confuse respondents and often goes together with methodological problems such as low factor loadings and poor internal consistency because of their weaker correlation with the nonreversed scale items (e.g., Motl and DiStefano 2002; Quilty, Oakman, and Risko 2006).

In as far as reversed items are obtained by using negations (e.g., by inserting the particle *not*, as in “Most advertising is believable”/“Most advertising is not believable”), research shows that such negations are especially confusing (Swain, Weathers, and Niedrich 2008) and thus should better be avoided. In contrast, reversed items can also be obtained by

using an antonymic expression (e.g., “Advertising usually is believable”/“Advertising often is misleading”). According to Weijters and Baumgartner (2012), a careful use of such reversals can be beneficial, because not including reversed items still leaves the researcher with the problem of careless responding, acquiescence, and confirmation bias. When no reversed items are used, the method effects resulting from these biases cannot be disentangled from content and as such become more difficult to detect.

So how should researchers proceed with reversed items? First, extremely worded reversed items (such as “I love this ad”/“I hate this ad”) should be avoided because they lead to inconsistent responses. Second, it is advised to use balanced scales (that is, an equal number of normal and reversed items) to avoid response inconsistencies that may arise when respondents with a positive answer on an item do not have an opposite response option to choose from for the reversed item. Third, to keep respondents attentive and to avoid incorrect expectations, it may be best to alternate the keying of the items. Finally, because research shows that respondents consider a wider variety of relevant beliefs the further apart related items, it is best to distribute related items throughout the questionnaire, separated by unrelated buffer items (Weijters and Baumgartner 2012; Weijters, Geuens, and Baumgartner 2013; Weijters, Schillewaert, and Geuens 2008).

### Decisions Concerning the Scale Format

Constructs such as attitude toward the ad, attitude toward the brand, purchase intention, involvement, and several behavioral responses are usually measured by means of rating scales. These rating scales come in different formats and differ, for example, in terms of the number of scale points, the valence of the numbering, scale polarity, and the way scale points are labeled. The choice of a scale format influences the way respondents answer the questions (Tourangeau, Rips, and Rasinski 2000). The scale format can bias means, making ad responses look more or less positive than they really are (Weijters, Cabooter, and Schillewaert 2010), it can affect item variances affecting how homogeneous or heterogeneous participants’ ad responses seem (Greenleaf 1992), and it can alter correlations between items and constructs (Baumgartner and Steenkamp 2001). However, even though the choice of the scale format alters research results and conclusions, researchers often do not contemplate on why a certain format is chosen. They even regularly use a scale format that deviates from the originally validated scale format (Cabooter et al. 2016).

In the next sections, we summarize some of the most important research results and repeat some of the guidelines offered in the literature. However, we first pay attention to response styles, as they are the main source of contamination of research results. Response styles can emerge no matter which scale format is used (as they are also respondent and context

dependent), but some scale formats are more vulnerable than others.

*Response style effects.* Respondents' answers on questionnaire items are not always a reflection of their true opinions due to the presence of measurement errors such as response styles (Paulhus 1991; Baumgartner and Steenkamp 2001). Response styles can be defined as a tendency to select a certain response category of a rating scale more often regardless of the item content (Paulhus 1991). Frequently studied response styles are acquiescence (a preference for the positive response options), disacquiescence (a preference for the negative response options), extreme responding (a preference for the extreme response options), and midpoint responding (a preference for the middle response option) (Baumgartner and Steenkamp 2001).

Response styles affect the validity of research conclusions because they affect item means and variances, as well as correlations among items. For example, the presence of acquiescence (disacquiescence) inflates (deflates) item means and inflates (deflates) the estimated relation among items (Baumgartner and Steenkamp 2001). Response styles can be measured and corrected for, but this requires that researchers include a set of heterogeneous items that share no common content, either with one another or with the substantive items (Weijters, Baumgartner, and Geuens 2016). Such a set of unrelated, heterogeneous items is needed to be able to disentangle response styles from content. For example, if researchers include only substantive items, such as scales measuring respondents' attitudes toward a specific ad, attitudes toward the promoted brand, and purchase intentions, responses on these scales likely are highly correlated. Therefore, if respondents rate each item very positively, it is impossible to know to what extent these responses reflect the respondents' true opinions and to what extent they are a result of response style. Therefore, questions unrelated to the ad and the specific brand should be included as well, such as, for example, questions on sports, politics, gender equality, charity, and so on. For a set of  $k$  unrelated, heterogeneous control items measured on a 7-point response scale (1 = *Disagree*, 7 = *Agree*), acquiescence, disacquiescence, extreme responding, and midpoint responding can be measured as follows (Baumgartner and Steenkamp 2001):

$$\text{Acquiescence} = [f(7) * 3 + f(6) * 2 + f(4)]/k$$

$$\text{Disacquiescence} = [f(1) * 3 + f(2) * 2 + f(3)]/k$$

$$\text{Extreme responding} = [f(1) + f(7)]/k$$

$$\text{Midpoint responding} = f(4)/k$$

where  $f(x)$  refers to the frequency a respondent selects response option  $x$ .

Several options are available to correct for response styles (Weijters, Baumgartner, and Geuens 2016). For example, the original data can be corrected for response styles by regressing

the substantive items on the computed response style measures and using the residuals from this regression in subsequent analyses (Baumgartner and Steenkamp 2001). An alternative to computing and working with residuals is to use the response style measures as covariates in the main analyses. In this respect, the representative indicators response style means and covariance structure (RIRSMACS) approach, in which each substantive item is specified as a function of multiple measures of each response style, proves to be a reliable but also more complicated method (Weijters, Schillewaert, and Geuens 2008).

The foregoing methods correct for response styles at the individual level. However, sometimes it may be necessary or useful to correct on a group level. This could be the case in cross-cultural research when different languages or different nationalities lead to differences in how scales are used (e.g., Weijters, Geuens, and Baumgartner 2013; see also Puntoni, Weijters, and Baumgartner, this issue), but also in experimental research if it can be assumed that the experimental conditions trigger differences in scale usage. For example, as extreme responding has been shown to be more prevalent the more respondents are involved with the topic (Gibbons, Zellner, and Rudek 1999), an involvement manipulation may induce response styles at the group level (i.e., on the level of the different conditions). The same goes for a cognitive load manipulation, as cognitive load induces more acquiescence (Knowles and Condon 1999). An easy and convenient way to correct for group differences in response styles is the recently developed calibrated sigma method (Weijters, Baumgartner, and Geuens 2016). Whereas for a 5-point Likert scale, researchers usually assign 1 to the *Completely disagree* category, 2 to the *Disagree* category, 3 to the *Neutral* category, 4 to the *Agree* category, and 5 to the *Completely agree* category, the calibrated sigma method assigns different numbers to these categories for different groups (conditions). More specifically, the chosen numbers depend on how often the different response options are chosen for a set of heterogeneous, control items by the participants in a specific group. Thus, instead of automatically assigning the scores 1 to 5, the scale scores depend on the response behavior of the different groups. As such, *Agree* is not coded as 4 for the whole sample, but might be coded as 4.2 for one group and 3.8 for another group (Weijters, Baumgartner, and Geuens 2016).

It has to be mentioned, though, that, despite the general recognition that response styles threaten the validity of research findings, they are seldom corrected for. This is probably due to the fact that a set of extra items needs to be included in the questionnaire or that some correction methods are rather sophisticated. The more difficult it is to control for response styles, the more important it becomes to keep the occurrence of response styles as small as possible.

*Number of scale points.* In deciding on the number of scale points, researchers have to reconcile two considerations: the more scale points (1) the finer the information that can be

gathered but also (2) the more complex and time-consuming the choice task becomes, as respondents must discriminate between more and finer response categories (Weathers, Sharma, and Niedrich 2005).

A general recommendation in the literature is to use between five and nine scale points (Cox 1980; Schwarz, Knäuper, et al. 1991). Weijters, Cabooter, and Schillewaert (2010) observed that seven-point scale formats are most commonly used, followed by five-point scales. Investigating four-, five-, six-, and seven-point scales, these authors also conclude this is best practice. More specifically, they suggest to use seven-point scales for populations high in cognitive ability, verbal skills, and/or experience with questionnaires (such as students) but to limit the number of scale points to five for the general population. For respondents that can handle many scale points, scales with more response categories are better than scales with a smaller number of response categories, not only because they lead to more information but also because these scales reduce extreme responding. For respondents who have more difficulties with a large number of scale points, a scale with many (versus fewer) response options induces confusion and misresponse (i.e., responding in the same direction to opposite items) (Weijters, Cabooter, and Schillewaert 2010).

*Odd or even number of scale points.* Should response scales contain a midpoint or not? Some researchers believe that offering a midpoint decreases data quality because it may seduce respondents to simply select the midpoint without forming or reflecting on their true opinion (e.g., Klopfer and Madden 1980; Gilljam and Granberg 1993). Others advocate that respondents who hold a truly neutral or ambivalent opinion should not be forced to take a stance (Schuman and Presser 1981).

Recent evidence suggests that providing a midpoint may actually increase data quality. Forcing respondents to choose sides may result in task-related distress, and these negative emotions may lead respondents to disproportionately select negative response options. In line with this explanation, several researchers found that offering a midpoint leads to a significant shift from negative response options to the midpoint (Velez and Ashworth 2007; O'Muircheartaigh, Krosnick, and Helic 2000; Weijters, Cabooter, and Schillewaert 2010). In addition, Weijters, Cabooter, and Schillewaert (2010) report less extreme responding and less misresponse for scales with (versus without) a midpoint.

*Unipolar or bipolar scale format.* A scale format can be unipolar or bipolar. In the unipolar scale format each item is represented by a single statement (e.g., "I think this is a good ad"), whereas the bipolar scale format uses two opposing statements for each item (e.g., "I think this is a bad ad"/"I think this is a good ad"). According to some researchers, the polarity choice largely depends on the construct under study. Attitudes (e.g., attitude toward the ad, attitude toward the brand, ad-evoked emotions, satisfaction) are considered to be bipolar and thus should be measured with a bipolar scale format,

whereas behavior is considered to be a unipolar construct and thus should be measured with a unipolar scale format (Rossiter 2011). Other researchers argue that both unipolar and bipolar scale formats can be used to measure, for example, emotions (Bagozzi, Gopinath, and Nyer 1999). In practice, several researchers appear to use both type of scale formats interchangeably for attitudes and behaviors (Cabooter et al. 2016).

Important to know, though, is that respondents activate a different knowledge base depending on whether unipolar or bipolar scale formats are used. For bipolar scale formats, two explicit statement poles are provided and respondents activate knowledge related to both the positive and the negative pole. Subsequently, they consider response options on the left-hand side as gradations of the negative pole (e.g., bad), and they consider response options on the right-hand side as gradations of the positive pole (e.g., good). Doing so, respondents treat the scale format as symmetrical. In contrast, for unipolar scale formats, only one explicit statement pole is provided (e.g., good). This makes respondents uncertain of the other pole (could be not good or bad), the consequence of which is that respondents give meaning to the majority of the response options on the basis of the explicit pole. Thus, for an item such as "I think this is a good ad," respondents do not treat half, but more than half of the scale options as gradations of "good" (Gannon and Ostrom 1996; Cabooter et al. 2016).

Due to this difference in the perception of symmetry, respondents show more agreement (i.e., more acquiescence) for bipolar as compared to unipolar scale formats. For example, Cabooter et al. (2016) found 42% agreement for the unipolar statement "We are experiencing an improvement in our quality of life," but 66% agreement for its bipolar equivalent "We are experiencing a deterioration in our quality of life"/"We are experiencing an improvement in our quality of life."

To conclude, bipolar scale formats have the advantage that they are easier to interpret for respondents and that the researchers also know for sure how the two poles are interpreted (bad/good), while unipolar scale formats leave the researchers unsure of how respondents interpreted the implicit pole (not good or bad). The disadvantage of bipolar scales is that they can inflate means and thus provide too optimistic a picture (Cabooter et al. 2016).

*Scale numbering.* Researchers have the option to indicate response options with positive numbers only (e.g., from 1 to 7 for a 7-point scale) or with negative and positive numbers (e.g., -3 to +3 for a 7-point scale). In general, unipolar scale formats use positive numbers, whereas bipolar scale formats use negative and positive numbers (Schwarz, Knäuper, et al. 1991; Krosnick and Fabrigar 1997). However, other combinations of polarity and numbering are also used in practice (Cabooter et al. 2016).

In what sense do respondents react differently to differently numbered scale formats? Scale formats with positive and negative numbers, as compared to scale formats with positive numbers only, come across as more intense, and as such widen

the range of the scale (Judd and Harackiewicz 1980; Schwarz, Knäuper, et al. 1991). As people are more likely to avoid intense labels because they represent more extreme, exceptional positions (e.g., de Langhe et al. 2011), one would expect to find less extreme responding for scale formats with positive and negative numbers as compared to scale formats with positive numbers only.

Cabooter et al. (2016) find that scale numbering indeed lowers extreme responding for a bipolar scale, but not for a unipolar scale. The reason for the latter is that the use of positive and negative numbers for a unipolar scale not only widened the scale in respondents' minds but also made the unipolar scale symmetric (which has an opposite effect on extreme responding).

As a more general conclusion, Cabooter et al. (2016) found that scale numbering has a bigger impact for unipolar scale formats than for bipolar scale formats. Bipolar scales are clear, and therefore respondents do not really need the numbers to give meaning to the response options, but unipolar scales are less clear and therefore respondents draw on the numbers. As a consequence, including positive and negative numbers (as compared to positive numbers only) makes a unipolar scale resemble a bipolar scale.

*Scale labels.* Researchers need to choose (1) how many labels to use (e.g., textual labels for all response options or end-point labels only) as well as (2) which specific labels to use. Concerning the former, a scale format with fully labeled response options is easier to interpret both for respondents and researchers (Wildt and Mazis 1978) but increases respondents' tendency to agree, and thus results in a positive bias (Tourangeau, Rips, and Rasinski 2000; Weijters, Cabooter, and Schillewaert 2010). If only a few options are labeled, these options are more salient and become more accessible to respondents, and consequently they are chosen more often (e.g., Krosnick and Fabrigar 1997; Posavac, Herzstein, and Sanbonmatsu 2003). As a consequence, a scale with textual labels for the end points increases extreme responding as compared with a fully labeled scale. At the same time, it increases misresponse, because the unlabeled categories are less clear (Weijters, Cabooter, and Schillewaert 2010). Weijters, Cabooter, and Schillewaert (2010) recommend using fully labeled scales if researchers are interested in opinion measurement (i.e., means and percentages) but scale formats with end-point labels only if researchers are interested in estimating relations between variables.

As far as the specific labels are concerned, research has shown that wider, more intense end-point labels (e.g., *Strongly agree* and *Strongly disagree*) lead respondents to avoid these labels and thus to select the intermediate response options more often than when narrower, less intense end labels are used (e.g., *Agree* and *Disagree*) (Wyatt and Meyers 1987). Manipulating the amplifier of the end labels (e.g., *Strongly [dis]agree* versus *Completely [dis]agree*), Weijters, Geuens, and Baumgartner (2013) found that familiarity mattered even more than intensity.

Their results showed that the more respondents were familiar with an end-point label (i.e., the more often they used it in daily language), the more they endorsed the response option associated with this label. For example, U.S. respondents are more familiar with the end-point labels *Completely (dis)agree* than with the end-point labels *Strongly (dis)agree*. As a consequence, they will select the end points less when the scale format uses *Strongly (dis)agree* as end labels as compared to when *Completely (dis)agree* is used. Especially when opinion measurement is important or when researchers want to make comparisons among, for example, different cultural groups, it is important to think carefully about which exact labels to use (see also Puntoni et al. in this issue).

## ETHICAL ISSUES

In planning and conducting advertising research, a number of ethical considerations must be taken into account. First of all, permissions may have to be obtained from participants, companies, and institutions. Often, it is advisable to seek some form of informed consent from participants, that is, explain to them what their participation entails and ask for their permission to be included in the study. This is particularly important when including participants from vulnerable target groups, such as children, in which case parental permission is needed. Informed consent is somewhat contradictory to the principle of disguising the true nature of the study during the briefing. The best thing to do may be to seek partly informed consent, explain to participants that they will be debriefed after the study, and disclose the true nature of the study and the manipulations used during a debriefing at the end of the study. In case real brands are used, it may be advisable to gain permission from the brand-holding company for including their brands in the study. Finally, most research institutions or grant providers now have an ethical policy that requires permission for studies that include measurements with human subjects as, of course, all experimental advertising does. At all times, researchers should respect the privacy of participants and not collect, use, or publish privacy-infringing data that are not absolutely necessary to answer the research questions.

An important ethical concern is to avoid questionable research practices (John, Loewenstein, and Prelec 2012). Fiedler and Schwarz (2016) tested the prevalence of 10 of these practices in psychological research. Most of them are also relevant (and probably prevalent) in advertising research. In order of importance, they are as follows (Fiedler and Schwarz 2016):

- Claiming to have predicted an unexpected outcome
- Selectively reporting studies regarding a specific finding that "worked"
- Deciding whether to exclude data after looking at the impact of doing so regarding a specific finding
- Failing to report all dependent measures that are relevant for a finding

- Collecting more data after seeing whether results were significant in order to render nonsignificant results significant
- Failing to report all conditions that are relevant for a finding
- Rounding off  $p$  values (e.g., reporting a  $p$  value of .054 as .05)
- Stopping data collection after achieving the desired result concerning a specific finding
- Claiming that results are unaffected by demographic variables (e.g., gender) when actually unsure whether they are affected (or knowing that they are affected)
- Falsifying data

The last three practices are very rare, at least as reported by researchers. However, researchers report engaging in the first five practices between 30% and 50% of the time. Obviously, these practices should be avoided, although it is fair to say that the most prevalent ones are also the most ambiguous ones, in that they do not always imply questionable practices. For instance, not always reporting all dependent measures can reflect justifiable behaviors (e.g., editorial requests). Focusing on studies that worked may reflect the reality that null results are not easily accepted for publication and thus discarded. Collecting more data after initial analyses may be an acceptable research practice when effect sizes appear to be smaller than expected. Excluding data after looking at the impact of doing so may simply reflect the correct behavior of finding out what, for instance, outliers do to one's conclusions. Discarding respondents on the basis of controls for inattentive or careless responding is good research practice and is always necessary (Meade and Craig 2012). Nevertheless, although some of the most prevalent questionable research practices may be justifiable, researchers are certainly called to provide full openness about their data collection and data analyses (e.g., sharing raw data sets online, disclosing how many respondents they discarded and for what reasons).

## CONCLUSION

Experimental advertising research requires well-developed, well-controlled, and well-executed designs in which stimuli, samples, questionnaires, and measures are methodologically correctly combined to provide a genuine and meaningful contribution to theory and practice. A wrong or inappropriate decision on any of the issues discussed can completely invalidate the findings and thus render the study meaningless, or at least its results questionable. There is no such thing as a perfect experimental study, and to several of the issues discussed there are no straightforward right or wrong answers. But it is of the utmost importance that researchers carefully consider every methodological step and make the most appropriate choices. We conclude with a general list of questions that need to be answered when setting up a sound experimental advertising study:

- Is there a meaningful theoretical and practical contribution?
- Are the (advertising) stimuli simple, realistic, well controlled, and embedded in a realistic advertising context?

- Is the sample of participants relevant for the study at hand and of appropriate size, and are subsamples equivalent?
- Does the order of the questions safeguard against any biases?
- Are manipulation check and quality control questions taken into consideration?
- Are construct valid, balanced scales used?
- Are 7-point scales used for student, MTurk, or other highly educated or experienced samples, and 5-point scales for the general population?
- Where possible, are bipolar, fully labeled scales used? (If the researcher is interested in estimating relations rather than opinion measuring, end-point labels may be better.)
- Can an unrelated, heterogeneous set of items be included to correct for response styles?
- Are the necessary permissions obtained?

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