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The Sleeper Effect in Persuasion: A Meta-Analytic Review

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A meta-analysis of the available judgment and memory data on the sleeper effect in persuasion is presented. According to this effect, when people receive a communication associated with a discounting cue, such as a noncredible source, they are less persuaded immediately after exposure than they are later in time. Findings from this meta-analysis indicate that recipients of discounting cues were more persuaded over time when the message arguments and the cue had a strong initial impact. In addition, the increase in persuasion was stronger when recipients of discounting cues had higher ability or motivation to think about the message and received the discounting cue after the message. These results are discussed in light of classic and contemporary models of attitudes and persuasion.

Persuasive messages are often accompanied by information that induces suspicions of invalidity. For instance, recipients of communications about a political candidate may discount a message coming from a representative of the opponent party because they do not perceive the source of the message as credible (e.g., Lariscy & Tinkham, 1999). Because the source of the political message serves as a *discounting cue* and temporarily decreases the impact of the message, recipients may not be persuaded by the advocacy immediately after they receive the communication. Over time, however, recipients of an otherwise influential message may recall the message but not the noncredible source and thus become more persuaded by the message at that time than they were immediately following the communication. The term *sleeper effect* has been used to denote such a delayed increase in persuasion observed when the discounting cue (e.g., noncredible source) becomes unavailable or "dissociated" from the communication in the memory of the message recipients (Hovland, Lumsdaine, & Sheffield, 1949). Because the sleeper effect concerns initial message impact, as well as recall of the information presented in the communication, the phenomenon has implications for broad models of persuasion, including early learning approaches (e.g., Hovland, Janis, & Kelley, 1953) as well as more recent conceptualizations, such as the heuristic-systematic model (Chen & Chaiken, 1999) and the elaboration-likelihood model (Petty & Cacioppo, 1986; Petty & Wegener, 1999).

The sleeper effect is counterintuitive because, generally, the impact of a persuasive communication is greater when one measures the effect closer to the presentation rather than farther away from the time of reception (Cook & Flay, 1978; Eagly & Chaiken, 1993). As such, this phenomenon has stimulated a large amount of research about the possibility of increased persuasion over time, as well as the potential decrease and lack of longitudinal change in persuasion (for reviews, see Cook & Flay, 1978; Cook, Gruder, Hennigan, & Flay, 1979; Eagly & Chaiken, 1993). Moreover, if one can understand the conditions that elicit increases, decreases, and stability in persuasion, one should be able to explain the mechanisms that mediate such attitude change. Similarly, explaining the mechanisms that underlie attitude change and stability implies increasing researchers' understanding of the processes that influence behavioral change. Currently, many disciplines are concerned with cognitive and behavioral change, including psychology, medicine, nursing, marketing, organizational behavior, political science, sociology, and environmental sciences, among others. Therefore, an analysis of the stability and change of persuasion can be informative for a large number of researchers and practitioners.

Given that the sleeper effect constitutes an important testing ground to understand the cognitive mechanisms that produce attitude change over time, one might assume that various systematic literature reviews would have investigated the effect. Such a presumption would be correct, at least in part. For instance, Capon and Hulbert (1973) qualitatively reviewed the literature on the sleeper effect available at the time and concluded that the effect was so unreliable that researchers would be better off accepting the null hypothesis than continuing to further examine the phenomenon (for similar conclusions, see Gillig & Greenwald, 1974). Other reviewers, however, have cautioned against accepting the null hypothesis and have suggested that a careful review of past data, as well as rigorous theorizing testing would resolve the controversy (Cook & Flay, 1978; Cook et al., 1979; Eagly & Chaiken, 1993; Gruder et al., 1978). What no review has done to date is meta-analyze all available research and carefully explore the conditions under which the sleeper effect is most likely to occur. Prior reviews either have not meta-analyzed the moderators of the effect (e.g., Allen & Stiff, 1989; Capon & Hulbert, 1973; Cook & Flay,

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1978; Cook et al., 1979; Pratkanis, 1981) or have done so within the context of a single series of studies (Pratkanis, Greenwald, Leippe, & Baumgardner, 1988). However, none of them has presented a synthesis of past research after taking into account variability across available studies, as is the case when one conducts a meta-analysis.

The present article reports a meta-analysis of the sleeper effect. We used different types of methods to review the literature relevant to investigating the effect. These procedures allowed us (a) to identify the conditions under which the sleeper effect is most likely to occur and (b) to derive overall estimates of the effect. Furthermore, the review permitted the estimation of decay and stability in attitude change, which, to our knowledge, has never been done across different studies of persuasion (for meta-analyses on other attitudinal phenomena, see Eagly, Chen, Chaiken, & Shaw-Barnes, 1999; B. T. Johnson & Eagly, 1989; Wood & Quinn, 2003). Finally, this meta-analysis is among the first to consider predictions coming from a variety of conceptualizations, including models of persuasion that specify memory processes, as well as more recent analyses of the factors that increase the influence of message arguments and cues—namely the heuristic-systematic model (for reviews, see Chaiken, Liberman, & Eagly, 1989; Chen & Chaiken, 1999) and the elaboration-likelihood model (Petty & Cacioppo, 1986; see also Petty & Wegener, 1999).

For example, the heuristic-systematic model maintains that people simultaneously process the arguments contained in a persuasive message and the characteristics of persuasion cues, such as source credibility (Chaiken et al., 1989; Chen & Chaiken, 1999). Thus, the model is consistent with the possibility that message recipients may be influenced by sound arguments but momentarily disappointed by the limited credibility of the source. These conditions are, presumably, the ones that stimulate the sleeper effect (for detailed discussions about the need for initial message impact, see Cook et al., 1979; Gruder et al., 1978).

The initial impact of these factors was an important moderator in our analysis. Furthermore, the elaboration-likelihood model (Petty & Cacioppo, 1986) can accommodate the sleeper effect by identifying specific conditions under which the source of a persuasive message and the arguments discussed in the message can both have an impact. For instance, the message arguments and the source may both have an impact when recipients, as required to be persuaded by the communication arguments, think carefully about them (i.e., have high instead of low ability and/or high instead of low motivation) even if they later learn about the limited credibility of the source (e.g., when the source is presented last; Petty & Cacioppo, 1986). In light of these predictions, we examined the influence of ability and motivation to process the communication, as well as the more general influence of the initial impact of the message. In addition, we considered several other methodological factors that can influence the longitudinal course of change in attitudes. First, we discuss the characteristics of the effect and the way in which one can distinguish sleeper effects from other courses of attitude change. After this analysis, we turn to a consideration of the models that have explicit or implicit implications for the sleeper effect.

Definition of the Sleeper Effect

Figure 1 presents several theoretical patterns of change in attitudes over time including the (a) nonpersisting boomerang effect, (b) absolute sleeper effect, and (c) relative sleeper effect. All these patterns show that persuasion is likely to decay when individuals receive a communication containing an acceptance cue, such as a credible source. However, there are important differences in patterns of change when the participants receive a discounting cue.

Panel A of Figure 1 depicts a *nonpersisting boomerang effect*, which is conceptually distinct from the sleeper effect. In this situation, a message initially produces attitude change opposite to the direction of the advocacy. Because recipients of discounting cues initially agree with the advocacy to a lesser extent than do control or baseline participants (dotted line in Figure 1), an increase in persuasion over time among recipients of discounting cues implies dissolution of the boomerang effect and return to baseline rather than a sleeper effect.[1]

The other two lettered panels of Figure 1 present sleeper effects. To diagnose a sleeper effect, agreement with the message among recipients of discounting cues should not fall below the level of agreement among control participants (baseline), as would be the case in a boomerang effect. Therefore, to identify the sleeper effect, it is first necessary to confirm that immediately following the persuasive communication, recipients of discounting cues are at least as persuaded of the advocacy as baseline participants (see Panels B and C). Of course, recipients of discounting cues could be more persuaded of the advocacy than baseline participants. However, if the discounting cue is sufficiently strong to suppress the impact of the persuasive arguments, the initial combined effect of the persuasive arguments and the discounting cue should be zero, represented with attitudes at the same level of baseline attitudes.

It is important to note that past researchers have identified two types of sleeper effects. In the *absolute sleeper effect,* there is a statistically significant increase in persuasion among recipients of discounting cues not observed in the baseline conditions (see Panel B1). If there is an increase in baseline attitudes, this increase should be smaller than the increase in persuasion observed among recipients of discounting cues (see Panel B2). In the *relative sleeper effect*, persuasion among recipients of discounting cues (a) decays less than among those in acceptance-cue conditions (see Panel C1); (b) persists over time, whereas there is decay among those in acceptance-cue conditions (see Panel C2); or (c) increases over time, but the same increase is apparent among baseline participants (see Panel C3).

Theoretical Mechanisms Involved in Delayed Attitude Change

Past research on persuasion has considered several mechanisms to explain the absolute sleeper effect. The original explanations conceptualized the sleeper effect as a function of (a) forgetting of the discounting cue, (b) dissociation of the message and the discounting cue, and (c) differential decay in the impact of the message and the discounting cue. Further, more general theories of

[1] Although a boomerang effect represents an alternative interpretation for an effect that assumes that the initial communication had an impact, the mechanisms involved in the dissolution of an earlier boomerang effect might be similar to those in the sleeper effect. For instance, recipients of a communication may become less favorable toward the advocacy following the message than prior to it because the source of the communication lacks credibility. In those situations, recipients may return to their more initial favorable attitude as they forget who the source was.

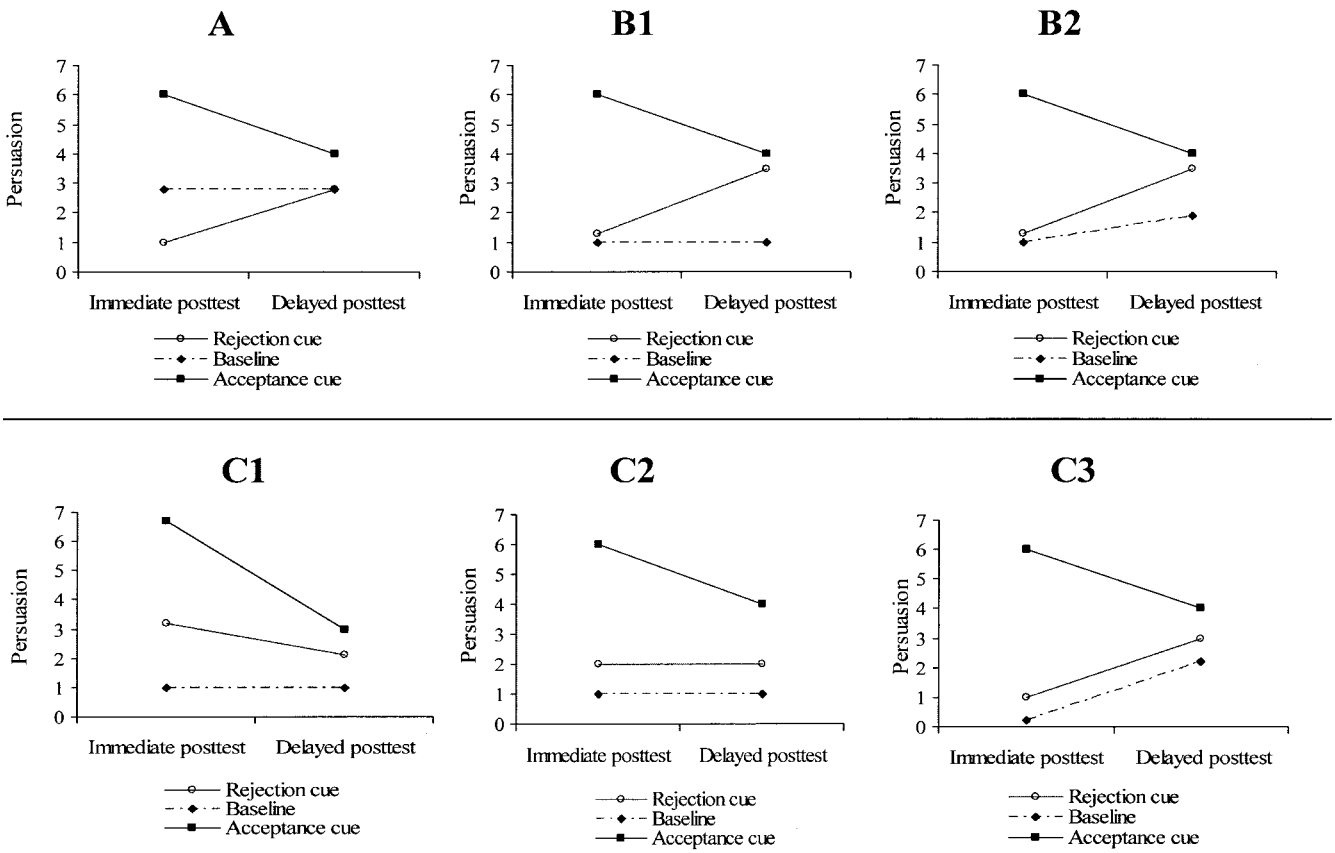

Figure 1. Persistence of persuasion. A: nonpersisting boomerang effect. B: absolute sleeper effect. C: relative sleeper effect (for a review of other patterns, see Cook & Flay, 1978).

persuasion like the heuristic-systematic and elaboration-likelihood models have implications for the sleeper effect because they identify conditions in which the message arguments and the cue should have a combined impact to begin with.

Memory for the Source in Persuasion Models

The sleeper effect was first identified in the context of attempts to change the opinions and morale of the enlisted U.S. soldiers during World War II. Specifically, Hovland et al. (1949) measured opinions of soldiers either 5 days or 9 weeks after the presentation of an army propaganda film. They found that the difference in opinions of those who watched the film and those who did not watch the film was greater 9 weeks after presenting the film than after 5 days. Moreover, the relative difference in delayed persuasion across the control and the experimental groups was accompanied by a significant increase in persuasion in the experimental group, which Hovland et al. (1949) termed the *sleeper effect*.

The first attempts to explain the effect were consistent with the understanding of persuasion processes at that time. On the basis of their general analysis of learning and memory, Hovland and his colleagues initiated a program of research to study how recall of the message and the source induced the sleeper effect. They first hypothesized that message recipients forget the noncredible communicator as time goes by, and thus the initial message rejection subsides (e.g., Hovland & Weiss, 1951). However, later they proposed that message recipients may not entirely forget the cue, yet the association between the representations of the discounting cue and the message content may fade over time and produce a sleeper effect (Hovland & Weiss, 1951). These two formulations differ in that (a) *forgetting* implies that the traces of the cue disappear or become unavailable in memory over time, whereas (b) *dissociation* implies that the cue remains available in memory but is simply less easily retrieved (less accessible) in relation to the topic of the communication.

Decades later, Greenwald, Pratkanis, and colleagues argued that the impact of the message and the cue decay at different rates (Greenwald, Pratkanis, Leippe, & Baumgardner, 1986; Pratkanis et al., 1988). This hypothesis encompasses the possibility that the discounting cue can become either unavailable or inaccessible as time elapses. Given that researchers cannot conclusively demonstrate whether representations are unavailable or simply inaccessible in memory (Brown & Craik, 2000; Higgins, 1996), the three hypotheses seem very similar. We separately describe the implications of each hypothesis in the following sections.

Forgetting and dissociation hypotheses. According to the forgetting hypothesis, a discounting cue associated with a message initially decreases acceptance of the message. As time goes by, one may observe a delayed increase in persuasion if the recipient forgets the cue but recalls the merits of the message (Hovland et al., 1949). To test this hypothesis, Hovland and his colleagues

(Hovland & Weiss, 1951; Kelman & Hovland, 1953; Weiss, 1953) initiated a series of experiments in which participants received messages attributed to either trustworthy or untrustworthy sources and then completed measures of opinions as well as of recall of the message content and the source. Overall, messages with credible sources produced greater initial persuasion than messages delivered by noncredible sources. Over time, however, the impact of the messages presented by credible sources decayed, whereas the impact of the messages presented by noncredible sources either remained the same or increased slightly. Despite support for the sleeper effect at the level of attitude change in this series of studies, the recall measures indicated that recipients could still remember the noncredible sources of the messages at the time of the delayed follow-up. On the basis of this finding, Hovland and Weiss (1951) replaced the forgetting hypothesis with the dissociation hypothesis. According to the new formulation, the sleeper effect need not imply that the discounting cue becomes permanently unavailable in memory. A weakened association between the cue and the message may be sufficient for the sleeper effect to take place. As the association weakens over time, rendering the cue less accessible in relation to the communication topic, there may be a delayed increase in persuasion as long as the message arguments are still memorable. To this extent, factors that facilitate retention of the message content should create settings conducive to the sleeper effect.

Differential decay. A question that Hovland and his colleagues ignored is why, over time, the discounting cue becomes less accessible than the message even when both pieces are similarly effective at the onset. To address this point, Greenwald, Pratkanis, and their colleagues (Greenwald et al., 1986; Pratkanis et al., 1988) implemented a result-centered approach to identify the conditions under which the sleeper effect does and does not occur.

In a series of 17 experiments, Pratkanis et al. (1988) presented the discounting cue either before or after the message and found that the sleeper effect mostly emerged when the cue followed the message but not when the cue was first. To explain this finding, they proposed a modified forgetting hypothesis, suggesting that the sleeper effect takes place because the impact of the message and the cue decay at different rates. According to this explanation, the message and the cue act like two communications operating in opposite directions. The sleeper effect emerges when the impact of these communications is about equal, immediately following message exposure, but the impact of the cue later decays more rapidly than that of the message. However, the position of the discounting cue is essential to produce the effect because information presented first lasts longer, whereas more recent information dissipates more rapidly (Miller & Campbell, 1959). Thus, the sleeper effect should occur when the discounting cue appears at the end of a persuasive communication and stimulates a primacy effect of the message content.

Moderators implied or tested in the context of these hypotheses. The three aforementioned hypotheses imply that the sleeper effect occurs when the initial message is sufficiently strong to exert an influence even after time has elapsed. For instance, an absolute sleeper effect should take place when the cue initially suppresses the impact of a message that otherwise is persuasive (e.g., *initial change* or difference in discounting-cue conditions vs. baseline conditions = 0; see Panel B of Figure 1). Correspondingly, if there is considerable persuasion despite the cue (e.g., initial change > 0), there is little room for the sleeper effect to take place over time (Cook & Flay, 1978; Cook et al., 1979). In addition, according to the differential decay hypothesis, the presentation time of the cue is critical for the sleeper effect to occur. That is, the sleeper effect should occur only when the cue appears at the end of the communication, thus inducing message recipients to forget the cue.[2]

Joint Impact of the Message Arguments and the Discounting Cue in Models of Persuasion

The three hypotheses we have just reviewed all emphasize that adequate impacts of the message arguments and the discounting cue are essential to generate the sleeper effect. However, these models do not analyze the conditions under which the arguments contained in a message have most influence on recipients' attitudes, nor do they analyze the general conditions under which cues other than the arguments induce attitude change. Identifying these conditions has been the main objective of the heuristic-systematic model (Chaiken et al., 1989; Chen & Chaiken, 1999) and the elaboration-likelihood model (Petty & Cacioppo, 1986; Petty & Wegener, 1999).

The heuristic-systematic and elaboration-likelihood models suggest that people can change their attitudes on the basis of nonelaborative or elaborative processing of relevant information. When ability or motivation to think about a communication is limited, recipients are likely to form their attitudes on the basis of easy to process information such as the credibility of the communicator. In contrast, when recipients' ability or motivation to think about the issues are higher, recipients are likely to pay attention to the arguments in addition to the heuristic cue. Given these premises, message recipients who lack the ability or motivation to think about the communication should be more persuaded by credible sources than by noncredible sources. In contrast, recipients with higher ability or motivation should be more persuaded by strong arguments than by weak arguments, regardless of the credibility of the source.[3]

The heuristic-systematic and elaboration-likelihood models agree that an elaborative processing of the arguments contained in a communication increases the probability that the impact of a communication will last over time (for a review, see Petty, Haugtvedt, & Smith, 1995; see also Chen & Chaiken, 1999; Craik & Lockhart, 1972; Petty & Cacioppo, 1986). To this extent, the persistent impact of the content of a communication that is necessary for the sleeper effect is most likely to occur in the higher ability and motivation conditions that stimulate elaborative pro-

[2] When the cue induces acceptance, presenting the cue after the message arguments should induce decay of the influence of the acceptance cue to a greater extent than presenting the cue prior to the message arguments. Essentially, the influence of both types of cues should dissipate more quickly when the cue is positioned at the end of the message.

[3] Although high ability and motivation should result in the highest possible level of elaborative processing, typically researchers manipulate either ability or motivation (for a notable exception, see Albarracín & Kumkale, 2003). For instance, research documenting the effects of distraction (for a review, see Petty & Cacioppo, 1986) cannot always assume that all participants were highly motivated to think about the message. Nevertheless, high distraction significantly decreases systematic processing. Because both ability and motivation produce noticeable changes in amount of thought about the persuasive message, we examined the independent effects of ability and motivation.

cessing of the message arguments. By contrast, the influence of any discounting cue will be short lived to the extent that the cue is processed in an effortless fashion (for situations in which a cue may be processed in an elaborative fashion, see Petty & Cacioppo, 1986). Both of these premises are involved in the sleeper effect.

Heuristic-systematic model. According to Chaiken et al. (1989), people are cognitive misers, who think as little as necessary to achieve a confident decision. Consequently, these individuals process the information in an effortful fashion only to the extent that a heuristic is not available or is insufficient to provide a confident judgment. However, when recipients possess a heuristic and their level of desired confidence can be easily achieved by the application of the heuristic, they are unlikely to exert the effort to scrutinize the merits of the arguments in the communication.

To our knowledge, the sleeper effect has not been discussed in the context of the heuristic-systematic model. However, its implications are readily apparent. The discounting cue is likely to operate as a heuristic cue that leads message recipients to reject the advocacy via the application of a simple decision rule. Thus, people may reject the message even when cognitive resources and motivation are limited. Further, people are likely to also process the arguments contained in the communication in a systematic way when they desire a high confidence level (e.g., high processing motivation) and have the necessary ability to pay attention to the arguments contained in a communication. To the extent that heuristic and systematic processing can operate simultaneously (Chaiken & Maheswaran, 1994; Maheswaran & Chaiken, 1991), the heuristic-systematic model implies that the discounting cue and the arguments contained in a communication can both have an impact if processing ability and motivation are sufficient to allow recipients to reach systematic processing with the objective of forming highly confident judgments.

Elaboration-likelihood model. According to Petty and Cacioppo (1986), a persuasive communication can stimulate recipients to generate issue-relevant thoughts and to change attitudes in line with these thoughts (central route to persuasion). However, when people do not have the ability or the motivation to think about the issues discussed in the message, they may still use cues (e.g., source credibility) that can help them to make a decision without having to think much about the issues at hand (peripheral types of processing). Central types of processing are likely to occur when the ability and the motivation to think about a communication are higher, whereas peripheral types of processing are likely to take place when ability and motivation are lower.

The implications of the elaboration-likelihood model for the sleeper effect have been discussed in several reviews of the model (e.g., Petty & Cacioppo, 1986; Petty, Wegener, Fabrigar, Priester, & Cacioppo, 1993). In general, because the necessary processing of the message arguments should occur under higher ability and motivation, and the required processing of the discounting cue as a peripheral cue should occur under low ability or motivation, it might appear that the conditions for the sleeper effect would never occur. After all, both a central, long-lasting influence of the arguments and a peripheral, short-lasting influence of the discounting cue are necessary for the sleeper effect to take place. However, Petty and Cacioppo (1986) specified the conditions that could elicit the effect. When the discounting cue precedes the arguments of the message, recipients may not pay attention to the arguments because the cue suggests that the upcoming information is unreliable (see also Eagly & Chaiken, 1993; Priester, Wegener, Petty, & Fabrigar, 1999). Alternatively, the cue rather than the message may be ignored if message recipients start processing the arguments and see merit in them despite the initial expectations induced by the discounting cue. In each case, either (but not both) the discounting cue or the message would have an impact, thus preventing the sleeper effect from emerging.[4]

When the discounting cue follows the message arguments, it is also possible that the cue might be judged as irrelevant and therefore ignored (Petty & Cacioppo, 1986). For example, recipients who first form a strong attitude about the issue on the basis of the message arguments may later resist the influence of the discounting cue. Under these circumstances, there is little room for the sleeper effect to take place. Nevertheless, when the cue comes last, the message recipients may have a hard time ignoring the cue after receiving arguments that appeared convincing. In this situation, the cue may entirely suppress the impact of the message, facilitating the occurrence of the sleeper effect.

Because the elaboration-likelihood model suggests that the message arguments have a greater impact when people have higher ability and motivation to think about them, and the discounting cue may have an impact only when it appears after the message, Petty and Cacioppo (1986, p. 183) predicted that the sleeper effect should emerge only when processing ability and motivation are higher and when the cue follows the message.[5] As a partial test of this prediction, Priester, Wegener, Petty, and Fabrigar (1999) presented research participants with the discounting cue after the message arguments and found that the sleeper effect emerged only for those recipients who had high chronic motivation to think about the message (i.e., high need for cognition; Cacioppo, Petty, Feinstein, & Jarvis, 1996). In combination with Pratkanis et al.'s (1988) experiments, these findings suggest that for the sleeper effect to occur, the cue should follow the arguments and processing ability and motivation should be high. However, the present meta-analysis is the first piece of research to directly test this prediction.

Summary of Moderators Implied by the Different Models

To summarize, the forgetting, dissociation, and differential decay hypotheses all imply that the sleeper effect should emerge when the message exerts a sufficiently strong impact and the cue effectively suppresses this impact. Furthermore, the differential decay hypothesis suggests that the sleeper effect should take place when the discounting cue follows the message, and the elaboration-likelihood model assumes that the sleeper effect

[4] Petty and Cacioppo (1986) also stated that presenting a discounting cue prior to the message arguments could "bias" processing of these arguments. To this extent, it is possible that recipients who believe that the message might be invalid might still scrutinize the arguments while they test the hypothesis that the message is invalid. Consequently, the recipients could agree with the message less but nevertheless have a good reception and later recall of the arguments.

[5] These predictions may appear to be post hoc, given that the elaboration-likelihood model postulates a trade-off between elaborative and nonelaborative processing (see Petty & Cacioppo, 1986). However, it should be noted that Petty and colleagues have revised the trade-off principle in recent presentations of the elaboration-likelihood model (e.g., Petty & Wegener, 1999) to recognize that both types of processes can coexist.

should emerge when the cue follows the arguments and the message recipients have enough ability and motivation to process the communication. These three factors were considered as moderators of the sleeper effect in our meta-analysis. In addition, we examined the extent to which the time of presentation of the cue and the processing ability and motivation of the recipients moderated the sleeper effect by mediating influences on the amount of initial impact of the message and the cue. The amount of initial impact, in turn, was inferred from the amount of initial change when the message and the cue were both present relative to the amount of change in no-message and message-only conditions. Imagine that agreement with a given advocacy is greater when the message is presented without a discounting cue than when no message is presented at all. Then imagine that the agreement is the same when the message is accompanied by a discounting cue as when no message is presented. In these situations, one can reasonably infer that the message initially had an impact but that this impact was suppressed by the discounting cue. This rationale was the basis for some of our analyses.

The Present Meta-Analysis

A survey of the literature revealed 24 eligible reports on the sleeper effect, which contained 72 independent experiments or data sets. The major goals of the present meta-analysis were threefold. We first aimed to assess the magnitude of the sleeper effect by synthesizing the research findings. Next, we assessed the role of the initial impact of the communication and reviewed all available evidence about the recall of the message arguments and the discounting cue. Finally, we examined the prediction that the sleeper effect is a function of the interaction between the presentation time of the discounting cue and the level of the recipients' ability and motivation to think about the communication. In doing this, we took into account other methodological differences in the studies we analyzed. For instance, researchers have created different types of discounting cues, including sources of dubious credibility as well as direct assertions that the message was false. These different kinds of manipulations of the discounting cue could yield different results, an issue that was never examined previously. Similarly, the time of the delayed follow-up may exert an influence because longer times offer more opportunities for the cue to decay, or it may exert no influence, assuming that both the cue and the message arguments decay equally as time goes by. We examined the influence of these and other factors in exploratory moderator analyses.

Method

Sample of Studies

We retrieved reports related to the sleeper effect that were available by March 2003 by means of multiple procedures. First, we searched computerized databases, including PsycINFO (1887–2003), *Dissertation Abstracts International* (1861–2003), Educational Resources Information Center (1967–2003), and the Social Sciences Citation Index (1956–2003), using the key words, *sleeper effect*, *delayed-action*, *credibility*, *source credibility*, *source expertise*, *attitude change*, *discounting cue*, *attitude persistence*, *attitude maintenance*, *persuasion*, *propaganda*, *attitude and memory*, *attitude and retention*, *attitude and decay*, and *persuasion and decay*. Because researchers often use the terms *opinion* and *belief* instead of *attitude*, we conducted searches using these substitute terms as well.

Second, after identifying the core body of reports, we examined the references of these reports and other relevant reviews to retrieve additional reports that were not included initially (e.g., Capon & Hulbert, 1973; Cook & Flay, 1978; Eagly & Chaiken, 1993; Hovland et al., 1953; McGuire, 1968, 1985). We also searched the Social Sciences Citation Index to locate all the reports that cited the reports that were already in the database.

Third, we manually searched volumes of *Personality and Social Psychology Bulletin* (1974–2002), the *Journal of Personality and Social Psychology* (1965–2002), and the *Journal of Abnormal and Social Psychology* (1953–1964).

Fourth, we requested unpublished reports through the electronic mailing lists of the Society for Personality and Social Psychology and the Association for Consumer Research. At this stage, we also searched the Internet-based conference proceedings database of the Association for Consumer Research.

Fifth, we searched various other Internet-based databases to locate theses and dissertations from universities outside of the United States, including the *Index to Theses* (available at http://www.theses.com) and *Foreign Doctoral Dissertations Database* of the Center for Research Libraries (available at http://www.crl.edu). We also searched the databases maintained by the Institute for Psychology Information in Germany (available at http://www.zpid.de) to locate studies conducted in German-speaking countries.[6]

Finally, we searched the *ComAbstracts* database (available at http://www.cios.org). This database surveys the scholarship in the communications field and provides the most comprehensive coverage of the journals in this field.

Selection Criteria

We used the following criteria to select studies for inclusion in the meta-analysis.

1. We only included studies that involved the presentation of a communication containing persuasive arguments. Thus, we excluded studies in which the participants played a role or were asked to make a speech that contradicted their opinions. We also excluded developmental studies involving delayed effects of an early event (e.g., child abuse), which sometimes are also referred to as *sleeper effects*.

2. We only included the studies in which the researchers measured persuasion at least twice after the presentation of a persuasive message. We included studies even if the relevant dependent measures at the different time points were obtained from different samples (e.g., Schulman & Worrall, 1970; Weiss, 1953).

3. We included studies only if they involved successful manipulations of discounting cues. This criterion led to the exclusion of studies from three reports (i.e., Collamore, 1994; Matice, 1978; McDermott & Hylton, 1980) in which, according to the manipulation checks, the intended discounting cues operated as acceptance cues (e.g., the intended noncredible source was reported to be highly credible).

4. Traditionally, researchers and textbook writers have described the sleeper effect as a delayed increase in the impact of a message that is attributed to a low-credibility source. Thus, we incorporated longitudinal studies that used noncredible sources even if the researchers did not have the explicit objective of testing the sleeper effect.

5. Finally, because we aimed for precise estimation of the sleeper effect, we only included studies that provided adequate descriptive and inferential statistics to calculate effect sizes for change in persuasion over time. This criterion led to the exclusion of studies from six reports (i.e., Chaiken, 1980; Holt & Watts, 1973; Lariscy & Tinkham, 1999; Pratkanis, 1981; Pratkanis et al., 1988; Watts & Holt, 1979). To deal with the problem of missing information, we obtained and reanalyzed raw data whenever possible by contacting the primary authors or by locating the theses and dissertations describing the data (e.g., Maddux, 1979; Papageorgis, 1962;

[6] We thank Tina Ristikari for information about these databases.

Weber, 1972). These procedures allowed us to include seven data sets that would have been otherwise excluded.

Our selection criteria led to a database of 72 data sets (*k*) reported in 24 independent reports. Six reports contributed 1 data set, and 11 reports contributed 2 data sets. The remaining 7 reports each contributed 3 or more data sets. Most notably, Pratkanis et al.'s (1988) article included 17 experiments, of which we could include 12 in the meta-analysis.[7] We represented the effects from different experiments or data sets as distinct provided that the samples were statistically independent.[8]

Moderators

For descriptive purposes, we recorded (a) the year and (b) the source (i.e., journal article, unpublished dissertation or thesis, other unpublished document) of each report, as well as (c) the sample composition (i.e., high school students, university students, other) and (d) the country in which the study was conducted. We also coded each experiment in terms of the moderators suggested by theory.[9] Specifically, we recorded the presentation time of the discounting cue as before the message, after the message, both before and after the message, and as part of the message arguments.[10]

In addition, we used three indicants of the recipients' ability and motivation to receive the communication. To measure the ability (opportunity) of recipients to think about the message, we recorded whether the message was repeated or presented only once (see, e.g., Cacioppo & Petty, 1979, 1985). We also recorded recipients' prior knowledge of the message topic as a factor that increases recipients' ability as well as motivation to think about the message (for reviews, see Petty & Cacioppo, 1986; Wood, Rhodes, & Biek, 1995). In line with other meta-analyses (i.e., Eagly et al., 1999; B. T. Johnson & Eagly, 1989), we estimated the recipients' prior knowledge on the issue (little or none, moderate, high) from the characteristics of the issue or the researchers' comments in the Method section of the report. Further, to capture the recipients' motivation to think about the communication, we coded the outcome relevance of the issues covered in messages, following previous meta-analyses (e.g., Eagly et al., 1999; B. T. Johnson & Eagly, 1989; Wood & Quinn, 2003). We coded studies with messages that were consequential for important immediate goals of the recipients as high in outcome relevance (e.g., tuition increase at the recipients' school), and we coded studies with messages that were not consequential for the research participants as low in outcome relevance (e.g., tuition increase at a distant university or civil rights movements in another country).[11]

We also made attempts to retrieve data about the recall/recognition of the discounting cue, which is presumably critical to the sleeper effect. These data were available when researchers questioned participants at the time of the delayed posttest and asked them to indicate who the source of the communication was or what type of information appeared at the end of the communication. Whenever possible, we retrieved the percentage of participants in a sample who correctly recalled or recognized the discounting cue at the time of the delayed posttest, and we used this proportion in supplementary moderator analyses. We also recorded whether the study included a measure of recall/recognition of the message. If a measure of message recall/recognition was included in the study, we recorded its type (i.e., free recall, multiple choice, true–false, mixed).

We recorded several other characteristics of the experiments including (a) the design of the experiments, (b) the nature of the attitudinal issues, (c) the measurement of persuasion, and (d) the context of the experiment. To characterize the design of each study, we coded (a) the number of attitude or belief assessments after message exposure, (b) whether time was a within- or between-subjects variable, (c) the number of days between message exposure and each attitude or belief assessment, (d) the type of discounting cue used in the study (i.e., source credibility, message-disclaimer narratives, reactance-inducing statements),[12] (e) the number of messages participants received, (f) the message length in words, (g) the message sidedness (i.e., one sided, two-sided), (h) the number of arguments contained in the message, (i) the message modality (i.e., print, text presented on the computer, audio, film and/or slides, or multimedia presentations), and (j) whether the study included message-only and no-message control groups.

To record the nature of the attitudinal issues used in studies, we recorded (a) the issue domain of each message (i.e., sociopolitical, health and biology, moral issues, environment, consumption, education, cultural truisms, mixed and other). We also recorded researchers' notes on (b) the extent to which the target message was discrepant (counterattitudinal) from the initial attitudes of recipients (i.e., low or no discrepancy, moderate discrepancy, high discrepancy).

With respect to the measurement procedures, we recorded (a) the type of measure used to assess persuasion (i.e., attitude, belief),

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[7] Note that Pratkanis et al. (1988) reported effect sizes, namely slopes of change over time calculated over more than two time points. Because we were interested in the amount of change between two time points ($M_{\text{delayed}} - M_{\text{immediate}}$ posttest), we could not use these effect sizes. Instead, we calculated effect sizes on the basis of means and the available information to derive error terms. However, five of their experiments did not contain this information and were therefore excluded.

[8] We first computed a weighted-average effect size for the sleeper effect, representing each of the 24 reports with a single effect size. Thus, when a study involved multiple experiments testing the sleeper effect, we averaged the effects reported in the different experiments of that study. Next, we computed average effect sizes by treating each data set within a study as a separate study provided that the samples from each data set were independent. The estimates derived by using both procedures were very similar. Thus, in the analyses we report, we treated each data set as a separate unit to maximize statistical power.

[9] Pratkanis et al. (1988) noted that a facilitating condition for the sleeper effect was to ask participants to rate the source credibility immediately after presenting information about the source. However, we could not assess this moderator because of lack of information about the order of the items in the postexperimental questionnaires of the studies we summarized.

[10] In Hovland and Weiss (1951), the discounting cue was presented along with the message arguments, on the same page of the printed material. Thus, the discounting cues in this study could not be coded as coming before or after the message. In Papageorgis (1963), the discounting cue consisted of reservations about the conclusions of the message inserted in the body of the message. Therefore, the discounting cue was embedded within the text of the message.

[11] In one study, both kinds of messages were used but the results were not reported separately for each kind of message (Pratkanis et al., 1988, Experiments 10–11). We classified this study as mixed in outcome relevance.

[12] Readers should note that different effect sizes were obtained for discounting and acceptance cues within each study, as opposed to treating different data sets as representing either type of cue.

(b) the number of items in each measure of attitudes or beliefs, (c) whether researchers reported the reliability of multiple-item persuasion measures, and (d) the type of scale used in each persuasion measure (i.e., single-item dichotomous, single-item polichotomous, multiple-item dichotomous, multiple-item polichotomous). Finally, we recorded (e) the setting of each study (i.e., laboratory or classroom, theater, other) and (f) the similarity of the delayed measurement setting to the setting of the message presentation and the immediate follow-up.

Studies were coded independently by G. Tarcan Kumkale and another graduate student. Agreement between coders was 100% in all dimensions of coding, except for three dimensions that required making more indirect inferences (i.e., recipients' estimated prior knowledge on the issue, outcome relevance, and message discrepancy). Agreement on these dimensions was 80% ($\kappa = .68$), 85% ($\kappa = .68$), and 65% ($\kappa = .45$), respectively. Thus, except for the message discrepancy variable, the agreement between coders was satisfactory (Orwin, 1994). We resolved disagreements by discussion and consultation with colleagues. Characteristics of the individual studies included in this review are presented in Table 1. The studies often contained several independent data sets, such as different messages and different experiments. The characteristics that distinguish different data sets within a report appear in the second column of the table.

Dependent Measures and Computation of Effect Sizes

We calculated effect sizes for (a) persuasion and (b) recall/recognition of the message content. Calculations were based on the data described in the primary reports as well as on the available responses of the authors to requests of further information. We also attempted to calculate effect sizes for participants' recall/recognition of the message source, but studies generally lacked the information necessary to compute effect sizes representing change in recall/recognition. The effects we calculated represented different comparisons. A graphic representation of the effect size categories appears in Figure 2. Vertical dotted arrows indicate differences among experimental and control groups at each time of measurement. For example, we calculated the difference in agreement with the message between discounting-cue and baseline conditions at the immediate posttest, which allowed us to examine the possibility of a nonpersisting boomerang effect. Horizontal dotted arrows stretching from one time point to another denote change between two time points. These effects were available for both persuasion and message recall/recognition.

Effect Sizes at Each Time and Over Time

We calculated cross-sectional effect sizes for the differences among experimental and control groups at each time point by subtracting the mean of one group from the mean of another group and dividing the difference by the pooled standard deviation. (Information about the meanings of the signs of these effect sizes is presented as it becomes relevant.)

To represent change over time within each condition—discounting cue, acceptance cue (e.g., highly credible source), message-only control, no-message control, or baseline—we subtracted the mean at the earlier time point from the mean at the later point, and divided the difference by their pooled standard deviation. Thus, positive effect sizes indicate increases in the dependent measure (persuasion or recall/recognition) over time, whereas negative effect sizes represent decreases over time.

The effect size calculated for each of the relevant differences was *g*. If the data sets involved between-subjects comparisons (either cross-sectional effects or longitudinal designs with different samples of participants at each time; see Cook & Campbell, 1979), we estimated the effect size variance following the procedures described by Hedges and Olkin (1985). If an effect size corresponded to a within-group difference over time, we calculated the effect size variance by taking into account the correlation between repeated measures (Morris, 2000; Morris & DeShon, 2002; for detailed information on procedures for combining results from independent-groups vs. repeated measure designs, see Morris & DeShon, 2002).[13] We converted all *g*s into *d*s to correct for sample-size bias (Hedges & Olkin, 1985). Finally, so as to give more weight to data sets with larger sample sizes, we weighted each effect size by the reciprocal of its variance before we computed weighted average effect sizes (d_+).

Analyses of Effect Sizes

There are two major models used in meta-analysis: fixed effects and random effects. Because they are based on different assumptions,[14] they have different implications for the aggregation of

[13] Whenever possible, to derive the correlations necessary to compute the variances of the effect sizes, we used the available descriptive and inferential statistics provided in the reports ($k = 15$), or we reanalyzed the raw data ($k = 8$). In addition, we derived these correlations by reconstructing the analysis of variance tables from designs with within- and between-subjects factors that were reported in an additional set of five studies (Seignourel & Albarracín, 2002). From these sources, we obtained a total of 42 correlations and averaged them as suggested by Morris and DeShon (2002). The weighted-average correlation between persuasion at two different times was moderate in size ($Mr = .42$). Nonsignificant heterogeneity statistics suggested that the correlations were similar across experimental conditions and across measurement times. Thus, we used this weighted-average correlation in estimating variance of the effect sizes. To examine the impact of imputation, we used four other correlations (i.e., $r = 0$, $r = .22$, $r = .62$, $r = .82$) and conducted all the analyses separately with each correlation. The estimates that we obtained using different correlations were almost identical, as were significance tests and confidence intervals (CIs) around weighted-average effect sizes. Specifically, weighted average effect sizes for changes in the discounting-cue conditions ranged from 0.08 to 0.09 when we used the different correlations. The differences in confidence intervals were also negligible. That is, the lower bound of the CI ranged from 0.01 to 0.02, and the upper bound of the CI ranged from 0.13 to 0.15. As for the homogeneity statistics, heterogeneity increases as the size of the imputed correlation increases. Except for the case of $r = 0$, however, the homogeneity statistics indicated significant variability among effect sizes. In sum, because the results of these analyses did not vary significantly as a function of the imputed correlation, we only report the results of the analyses that we conducted using the average correlation of .42.

[14] In fixed-effects models, the reviewer assumes that the studies in the meta-analysis share a common true effect size and that the observed differences across studies are due to sampling error alone. Thus, differences observed across studies are attributed solely to within-study variation (chance). In meta-analytic reviews, effect sizes are weighted by the inverse of their variances to give more weight to effect sizes that are more reliably estimated. In fixed-effects models, weights take only the within-study variation into account (or the sampling error). Random-effects models, however, are based on the assumption that there is a distribution of effect

effect sizes and the modeling of the variability among the pooled effects. A random-effects approach allows for generalizations to a broader universe of studies than a fixed-effects approach, typically at the expense of statistical power. Nevertheless, there seems to be no quick answer to the question of which model to choose in synthesizing a literature (see Cooper & Hedges, 1994; Hedges & Olkin, 1985; Hedges & Vevea, 1998; Raudenbush, 1994; Wang & Bushman, 1999).[15] To benefit from the strengths of both models, we chose to aggregate the effect sizes and to conduct analyses using both approaches.

In fixed-effects models, we calculated the weighted average effect sizes following the procedures described in Hedges and Olkin (1985). In random-effects models, we aggregated the effect sizes with the hierarchical linear modeling approach to meta-analysis (Byrk & Raudenbush, 1992; Raudenbush, 1994; see also Hedges & Vevea, 1998). Along with weighted average effect sizes, we computed homogeneity statistics. For both fixed- and random-effects models, the Q value of an average effect is an index of variability distributed as a chi-square with $k - 1$ degrees of freedom, where k is the number of data sets. A significant homogeneity statistic indicates that the effect sizes comprising the weighted-average effect size may be coming from different populations. Similarly, when Q is obtained to describe the variance unaccounted for by the predictors in a regression equation, Q_E (error) has an approximate chi-square distribution with $k - 1 - p$ degrees of freedom, where p is the number of predictors. A significant Q in this case implies that the model has significant unaccounted variance. The tests of homogeneity are identical in both types of models (Hedges & Vevea, 1998; Raudenbush, 1994).

Results

The data analysis included a description of the experiments we summarized, an estimation of overall effects, moderator analyses, and tests of mediation.

Sample of Studies and Data Sets

Descriptive characteristics of the data sets included in the present meta-analysis appear in Table 2. The mean year of publication was 1979. Most of the studies were conducted in the United States with student samples. In general, participants received the discounting cue before the message, and target messages were repeated more than once.[16] The messages used in the studies typically involved issues that were not consequential for important immediate goals of the recipients (i.e., low outcome relevance).

In all the data sets in our review, researchers measured persuasion at least twice after message exposure. About one tenth of the data sets also contained measures taken at more than two time points. Designs were generally within subject with an average of over 2 weeks between the immediate and the delayed measurements. In most data sets, researchers manipulated source characteristics to operationalize discounting cues, but at times they used postmessage disclaimers and other reactance-inducing statements. Almost all of the messages were one sided and presented as a written text, and most of the written messages had one or two pages. About half of the data sets included baseline control conditions in which no message was presented. Given the average size of 27 participants in the discounting-cue conditions in our synthesis, the typical sleeper effect study lacked the statistical power to detect a small effect such as $d = 0.20$.

The data sets involved messages dealing with a wide range of issues from different domains. Half of the data sets measured persuasion by means of attitude measures, whereas the other half measured change in beliefs. Researchers generally failed to report the reliability of these measures. It is important to note that researchers measured recall/recognition of message content and recall/recognition of the discounting cue 40% and 22% of the times, respectively. In assessing recall/recognition of message content, researchers used free-recall protocols in about 70% of the data sets. Recognition measures (i.e., multiple choice or true–false) were also commonly used. In assessing recall/recognition of the discounting cue, researchers almost exclusively used free-recall measures. Almost all of the studies were conducted in laboratory or classroom settings. Finally, in about half of the data sets, researchers obtained the delayed measurements in a setting different from the setting in which the message was presented and the immediate measures were obtained, telling participants that the follow-up sessions were part of a different study.

Overview of the Average Effect Sizes

A thorough understanding of the sleeper effect requires examining (a) the between-conditions differences at each time point, as well as (b) the within-condition changes that take place over time. Between-conditions analyses are necessary to rule out the possibility of a nonpersistent boomerang effect; within-condition analyses are necessary to examine the magnitude of change and to

(*text continues on page 156*)

sizes, meaning that the population parameter is not a fixed value but a random variable with its own distribution. Thus, the differences in effect sizes among studies are due not only to sampling error (i.e., within-study variation) but also to other factors such as measurement error and the between-studies variance component reflecting random differences across studies (τ). In random-effects models, weights reflect both the within-study variation as well as the between-studies variation. Thus, in the case of heterogeneity among effect sizes ($\tau > 0$), random-effects models give wider CIs than fixed-effects models. When τ is equal to zero, the fixed- and random-effects models reveal identical mean estimates and identical CIs around means. When τ is not significant, suggesting homogeneity or model fit, the two approaches yield very similar results (Hedges & Vevea, 1998). The τ values representing the goodness of fit are distributed as chi-squares. The inferential test statistic representing the significance of this indicator is the well-known Q statistic. In the present study, we only report the Q values.

[15] Hedges and Vevea (1998) suggested that the choice of estimation method should primarily depend on the type of inference goals. According to them, fixed-effects procedures are more appropriate for meta-analysts who want to make inferences about the particular set of studies to be synthesized in the meta-analysis. Random-effects procedures are more appropriate for meta-analysts who want to make inferences about the population from which the particular sample of studies was drawn.

[16] The fact that target messages were generally repeated more than once may be surprising to some readers. The repetition was typically justified by telling participants that the study was to first evaluate the content and then the style of a communication. Other times, researchers told participants that the study concerned verbal learning and that a text would be repeated prior to testing participants' memory for its content.

Table 1
Studies Included in the Meta-Analysis

Study	Exp. and/or data set	Discounting-cue conditions: Change over time *d* (95% CI)	Discounting-cue conditions: Amount of initial change *d* (95% CI)	Msg. rep.	Outcome relevance	Knowledge on issue	Type of cue	Measure of content r/r	Recipients with r/r of cue (%)
		Presentation time of discounting cue: After the message							
Florack et al. (2003)	The description of the cue was long.	0.02 (−0.63, 0.67)	0.52 (−0.20, 1.26)	No	Low	Moderate	Source credibility	—	—
	The description of the cue was short.	0.14 (−0.57, 0.86)	0.94 (0.19, 1.70)	No	Low	Moderate	Source credibility	—	—
Gruder et al. (1978)	Exp. 1: The communication advocated only 4 days of work a week.	0.44 (0.14, 0.75)	−0.01 (−0.41, 0.40)	Yes	High	High	Disclaimer	—	—
	Exp. 1: The communication advocated prohibiting a left turn when traffic lights are red.	0.00 (−0.36, 0.36)	0.33 (−0.07, 0.74)	Yes	High	High	Disclaimer	—	—
	Exp. 2: The cue was a disclaimer note indicating that the message conclusion was false without restating it.	0.05 (−0.22, 0.33)	0.40 (0.03, 0.77)	Yes	High	High	Disclaimer	—	6
	Exp. 2: The cue was a reactance-inducing statement.	0.17 (−0.10, 0.44)	0.34 (−0.02, 0.70)	Yes	High	High	Reactance	—	3
	Exp. 2: The cue was a disclaimer note restating the conclusion of the message and declaring that it was wrong.	0.23 (−0.06, 0.51)	0.15 (−0.22, 0.51)	Yes	High	High	Disclaimer	—	3
	Exp. 2: The cue was manipulated by presenting reactance-inducing statements along with a disclaimer note, which restated the conclusion of the message and declared that it was wrong.	0.39 (0.10, 0.67)	−0.02 (−0.38, 0.34)	Yes	High	High	Disclaimer and reactance	—	3
	Exp. 2: The cue was manipulated by presenting reactance-inducing statements along with a disclaimer note, which declared that the conclusion of the message was wrong without restating it.	0.44 (0.07, 0.82)	0.07 (−0.31, 0.44)	Yes	High	High	Disclaimer and reactance	—	0.5
Kumkale et al. (2003)	Single exp.; data sets were broken down only by the position of the cue.	0.47 (0.01, 0.93)	—	Yes	High	High	Source credibility	Only delayed measure	—
Mazursky & Schul (1988)	Exp. 1: No instructions.	−0.26 (−0.83, 0.31)	—	No	Low	High	Disclaimer	At both time points	—
	Exp. 1: Participants were asked to imagine themselves using the advertised product and to think about the arguments contained in the communication.	0.59 (0.01, 1.16)	—	No	Low	High	Disclaimer	At both time points	0
	Exp. 2: The same instructions in Exp. 1 were given after the cue.	0.12 (−0.39, 0.63)	—	No	Low	High	Disclaimer	—	—
	Exp. 2: The same instructions in Exp. 1 were given before the cue.	0.77 (0.25, 1.29)	—	No	Low	High	Disclaimer	—	—
Moscovici et al. (1981)	The message was attributed to a flexible minority.	−0.23 (−0.71, 0.24)	—	No	Low	Moderate	Reactance	—	—
	The message was attributed to a rigid minority.	0.40 (−0.11, 0.91)	—	No	Low	Moderate	Reactance	—	—

Table 1 (*continued*)

Study	Exp. and/or data set	Discounting-cue conditions: Change over time *d* (95% CI)	Discounting-cue conditions: Amount of initial change *d* (95% CI)	Msg. rep.	Outcome relevance	Knowledge on issue	Type of cue	Measure of content r/r	Recipients with r/r of cue (%)
		Presentation time of discounting cue: After the message (*continued*)							
Pratkanis et al. (1988)	Exp. 7	0.26 (−0.18, 0.71)	—	No	Low	Moderate	Source credibility	—	—
	Exp. 10–11	−0.02 (−0.26, 0.21)	—	No	Low	Moderate	Source credibility	—	—
	Exp. 13	0.02 (−0.46, 0.50)	0.22 (−0.41, 0.84)	No	Low	Moderate	Source credibility	—	—
	Exp. 14	0.08 (−0.20, 0.35)	0.03 (−0.33, 0.38)	No	Low	Moderate	Source credibility	—	—
	Exp. 15	0.04 (−0.42, 0.50)	0.09 (−0.50, 0.68)	No	Low	Moderate	Source credibility	—	—
	Exp. 16	0.01 (−0.33, 0.35)	0.04 (−0.39, 0.48)	No	Low	Moderate	Source credibility	—	—
	Exp. 17: Participants received a 300-word message arguing against a 4-day work week.	0.23 (−0.05, 0.50)	0.00 (−0.27, 0.27)	Yes	High	High	Disclaimer and reactance	—	8.4
	Exp. 17: Participants received a 1,000-word message arguing against a 4-day work week.	0.30 (0.00, 0.59)	0.00 (−0.29, 0.29)	Yes	High	High	Disclaimer and reactance	—	8.4
Priester et al. (1999)	Recipients' need for cognition was low.	−0.17 (−0.83, 0.50)	—	No	Low	Moderate	Disclaimer	—	—
	Recipients' need for cognition was high.	0.61 (−0.11, 1.34)	—	No	Low	Moderate	Disclaimer	—	—
Weber (1971)	Single exp.; data sets were only broken down by the position of the cue.	−1.07 (−1.89, −0.25)	—	Yes	Low	Low	Source credibility	—	—
Weiss (1953)	Single exp. with single position of discounting cue.	−0.25 (−0.83, 0.33)	—	Yes	Low	High	Disclaimer	At both time points	6
		Presentation time of discounting cue: Before the message							
Falk (1970)	Single exp. with single position of discounting cue.	0.22 (−0.10, 0.55)	0.37 (−0.32, 1.06)	No	Low	High	Source credibility	—	—
Gillig & Greenwald (1974)	Exp. 1–3: Participants received defense arguments prior to the message.	−0.07 (−0.40, 0.27)	—	No	Low	Moderate	Source credibility	—	—
	Exp. 1–3: Participants did not receive defense arguments prior to the message.	0.15 (−0.17, 0.47)	—	No	Low	Moderate	Source credibility	—	—
Hannah & Sternthal (1984)	Exp. 1	0.82 (0.11, 1.53)	—	No	Low	Moderate	Source credibility	At both time points	—
	Exp. 2: The diagnosticity of the cue was low.	−0.07 (−1.00, 0.85)	—	No	Low	Moderate	Source credibility	At both time points	—
	Exp. 2: The diagnosticity of the cue was high.	1.13 (0.14, 2.13)	—	No	Low	Moderate	Source credibility	At both time points	—

(*table continues*)

Table 1 (*continued*)

Study	Exp. and/or data set	Discounting-cue conditions: Change over time *d* (95% CI)	Discounting-cue conditions: Amount of initial change *d* (95% CI)	Msg. rep.	Outcome relevance	Knowledge on issue	Type of cue	Measure of content r/r	Recipients with r/r of cue (%)
		Presentation time of discounting cue: Before the message (*continued*)							
Hennigan et al. (1982)	Participants processed the message with the expectation that they would receive further information about the message. The interval between measurements was 2 weeks.	0.03 (−0.32, 0.38)	0.20 (−0.39, 0.80)	Yes	Low	High	Source credibility	—	—
	Participants processed the message with the expectation that they would transmit its content to another person. The interval between measurements was 2 weeks.	0.11 (−0.24, 0.47)	0.41 (−0.20, 1.02)	Yes	Low	High	Source credibility	—	—
	Participants processed the message with the expectation that they would receive further information about the message. The interval between measurements was 5 weeks.	−0.31 (−0.69, 0.06)	0.24 (−0.36, 0.83)	Yes	Low	High	Source credibility	—	—
	Participants processed the message with the expectation that they would transmit its content to another person. The interval between measurements was 5 weeks.	0.13 (−0.23, 0.48)	0.27 (−0.33, 0.86)	Yes	Low	High	Source credibility	—	—
	Participants processed the message with the expectation that they would receive further information about the message. The interval between measurements was 9 weeks.	0.04 (−0.30, 0.39)	0.10 (−0.49, 0.69)	Yes	Low	High	Source credibility	—	—
	Participants processed the message with the expectation that they would transmit its content. The interval between measurements was 9 weeks.	0.23 (−0.15, 0.60)	0.42 (−0.18, 1.01)	Yes	Low	High	Source credibility	—	—
H. H. Johnson et al. (1968)	The message argued against frequent tooth brushing.	−0.04 (−0.39, 0.32)	—	No	High	Moderate	Source credibility	At both time points	—
	The message argued against the use of X-rays to detect tuberculosis.	−0.13 (−0.47, 0.21)	—	No	Low	Moderate	Source credibility	At both time points	—
H. H. Johnson & Watkins (1971)	The message was not repeated.	−0.08 (−0.63, 0.47)	—	No	Low	Moderate	Source credibility	Only immediately after exposure	—
	The message was repeated five times.	−0.08 (−0.63, 0.47)	—	Yes	Low	Moderate	Source credibility	Only immediately after exposure	—
Kelman (1958)	Single exp. with single position of discounting cue.	0.19 (−0.14, 0.52)	—	No	High	High	Source credibility	—	—
Kelman & Hovland (1953)	The discounting cue was reinstated right before the delayed measurement.	−0.21 (−0.56, 0.14)	—	No	High	High	Source credibility	Only delayed measure	—
	The discounting cue was not reinstated.	0.11 (−0.19, 0.41)	—	No	High	High	Source credibility	Only delayed measure	—
Kumkale et al. (2003)	Single exp.; data sets were broken down only by the position of the cue.	−0.45 (−1.08, 0.17)	—	Yes	High	High	Source credibility	Only delayed measure	—
Maddux & Rogers (1980)	The source was not an expert on the issue but was physically attractive.	−0.61 (−1.31, 0.01)	—	No	High	High	Source credibility	At both time points	—
	The source was neither an expert on the issue nor physically attractive.	−0.19 (−0.80, 0.42)	—	No	High	High	Source credibility	At both time points	—

Table 1 (*continued*)

Study	Exp. and/or data set	Discounting-cue conditions: Change over time *d* (95% CI)	Discounting-cue conditions: Amount of initial change *d* (95% CI)	Msg. rep.	Outcome relevance	Knowledge on issue	Type of cue	Measure of content r/r	Recipients with r/r of cue (%)
		Presentation time of discounting cue: Before the message (*continued*)							
Olson & Cal (1984)	Single exp. with single position of discounting cue.	−0.09 (−0.54, 0.37)	—	No	High	Moderate	Source credibility	Only delayed measure	—
Pratkanis et al. (1988)	Exp. 1	−0.07 (−0.70, 0.56)	—	No	Low	Moderate	Source credibility	—	—
	Exp. 2	0.03 (−0.60, 0.65)	—	No	Low	Moderate	Source credibility	—	—
	Exp. 3	−0.06 (−0.69, 0.56)	—	No	Low	Moderate	Source credibility	—	—
	Exp. 4	−0.01 (−0.64, 0.62)	—	No	Low	Moderate	Source credibility	—	—
	Exp. 7: The interval between the presentation time of the cue and the message was brief (one unit).	0.15 (−0.29, 0.59)	—	No	Low	Moderate	Source credibility	—	—
	Exp. 7: The interval between the presentation time of the cue and the message was longer (two units).	−0.15 (−0.59, 0.29)	—	No	Low	Moderate	Source credibility	—	—
	Exp. 10–11	0.03 (−0.21, 0.27)	—	No	Mixed	Moderate	Source credibility	—	—
	Exp. 17	−0.08 (−0.35, 0.19)	—	Yes	High	High	Disclaimer and reactance	—	—
Schulman & Worrall (1970)	Single exp. with single position of discounting cue.	−0.23 (−0.63, 0.17)	—	No	Low	Moderate	Source credibility	At both time points	8
Watts & McGuire (1964)	Single exp. with single position of discounting cue.	−0.05 (−0.36, 0.25)	—	No	Low	Moderate	Source credibility	At both time points	36
Weber (1971)	Single exp.; data sets were only broken down by the position of the cue.	−0.68 (−1.48, 0.12)	—	Yes	Low	Low	Source credibility	—	—
Weber (1972)	The source was repeated 22 times in the message. The interval between measurements was 3 weeks.	−0.10 (−0.53, 0.34)	0.73 (0.30, 1.42)	Yes	Low	Low	Source credibility	At both time points	—
	The source was repeated 22 times in the message. The interval between measurements was 7 weeks.	−0.76 (−1.33, −0.18)	0.67 (0.02, 1.31)	Yes	Low	Low	Source credibility	At both time points	—
	The source was repeated twice in the message. The interval between measurements was 3 weeks.	−0.08 (−0.55, 0.39)	0.57 (−0.10, 1.23)	Yes	Low	Low	Source credibility	At both time points	—
	The source was repeated twice in the message. The interval between measurements was 7 weeks.	0.02 (−0.56, 0.60)	0.30 (−0.40, 0.99)	Yes	Low	Low	Source credibility	At both time points	—
		Discounting cues were presented along with the message arguments (printed on the same page of the written material)							
Hovland & Weiss (1951)	The message discussed the future of movie theaters.	0.13 (−0.26, 0.52)	—	No	Low	Moderate	Source credibility	At both time points	—
	The message discussed the use of antihistamines.	0.14 (−0.26, 0.53)	—	No	Low	Moderate	Source credibility	At both time points	—
	The message discussed the building of atomic submarines.	0.28 (−0.08, 0.64)	—	No	Low	Moderate	Source credibility	At both time points	—

(*table continues*)

Table 1 (*continued*)

Study	Exp. and/or data set	Discounting-cue conditions: Change over time *d* (95% CI)	Discounting-cue conditions: Amount of initial change *d* (95% CI)	Msg. rep.	Outcome relevance	Knowledge on issue	Type of cue	Measure of content r/r	Recipients with r/r of cue (%)
Discounting cues were presented along with the message arguments (printed on the same page of the written material) (*continued*)									
Hovland & Weiss (1951) (*continued*)	The message discussed the cause of a then current steel shortage.	0.32 (−0.11, 0.75)	—	No	Low	Moderate	Source credibility	At both time points	—
Discounting cues were embedded within the text of the messages									
Papageorgis (1963)	The message argued that finding new oil deposits is as much a matter of chance as it was 20 years ago.	−0.09 (−0.70, 0.53)	—	No	Low	Low	Reservations	At both time points	—
	The message argued that life expectancies are longer in cities than in rural areas.	0.24 (−0.37, 0.86)	—	No	Low	Low	Reservations	At both time points	—

Note. Dashes indicate that the data were not provided in the original report. For the effect sizes (*d*s) representing change in persuasion over time, positive effect sizes indicate increase in persuasion in the direction of advocacy; negative effect sizes indicate decay (or movement in the direction opposite to that of the advocacy). For the effect sizes addressing the amount of initial change, which corresponds to the difference in attitudes between the discounting-cue and the no-message control conditions immediately after exposure, positive effect sizes indicate that the attitudes of the recipients in discounting-cue conditions were more positive in the direction of advocacy than the baseline attitudes. Exp. = Experiment; CI = confidence interval; Msg. rep. = message repetition; r/r = recall/recognition.

determine whether a sleeper effect might be absolute or relative (see Figure 1). In light of these requirements, we first examined whether discounting cues led to a decrease in agreement with the communication (boomerang effect). Next, for each condition, we examined the overall changes in persuasion from the immediate to the delayed posttest. If there is no evidence of a boomerang effect, an absolute sleeper effect is indicated by a significant longitudinal increase in persuasion in discounting-cue conditions that is not observed in the baseline. Assuming decay in acceptance-cue conditions, a relative sleeper effect occurs when there is (a) less decay or stability in discounting-cue conditions relative to acceptance-cue conditions or (b) a significant longitudinal increase in persuasion in discounting-cue conditions that does not differ from changes in baseline attitudes.

Ruling out a nonpersisting boomerang effect. To determine whether a delayed increase in persuasion represents an absolute sleeper effect, one needs to rule out a nonpersisting boomerang effect, which takes place when a message initially backfires but later loses this reverse effect (see Panel A of Figure 1). This possibility is ruled out if the initial agreement with the message in discounting-cue conditions is equal to or greater than the agreement in the baseline conditions. Table 3 summarizes the relevant statistics. As can be seen from the table, there was no evidence that receiving the discounting cue generated (negative) movement away from the message advocacy relative to the baseline. Instead, recipients of discounting cues were persuaded in the direction of the advocacy despite the discounting cue. Furthermore, the homogeneity statistics suggest that this pattern was fairly consistent across data sets. In Table 1, we provide the individual effect sizes entering into these analyses. These statistics include only one effect size reflecting less agreement with the advocacy in discounting cue than in no-message control conditions, and this effect had a negligible magnitude ($d = -0.02$). All other effects indicated equal or greater agreement with the advocacy in discounting-cue conditions than in no-message control conditions.

Average sleeper effect. Relevant statistics corresponding to average changes in persuasion from the immediate to the delayed posttest appear in Table 4, organized by the different conditions we considered (i.e., acceptance cue, discounting cue, no-message control, and message-only control). In Table 4, positive effect sizes indicate increases in persuasion over time, negative effect sizes indicate decay in persuasion, and zero effects denote stability in persuasion. CIs that do not include zero indicate significant changes over time. The first row of Table 4 shows that recipients of acceptance cues agreed with the message less as time went by (fixed effects: $d_+ = -0.21$; random effects: $d_+ = -0.23$). In contrast to the decay in persuasion for recipients of acceptance cues, there was a slight increase in persuasion for recipients of discounting cues over time ($d_+ = 0.08$). Of importance, change in discounting-cue conditions significantly differed from change in acceptance-cue conditions (fixed effects: $B = -0.29$, $SE = 0.04$, $Q_B(1) = 58.15$, $p < .0001$; $Q_E(123) = 193.82$, $p < .0001$).

Given the absence of a boomerang effect and significantly more decay in acceptance-cue conditions than in discounting-cue conditions, the next step was to examine the magnitude and type of sleeper effect in our synthesis. To determine whether an absolute sleeper effect was present, we needed to make sure that the increases in persuasion in discounting-cue conditions did not occur in baseline (no-message control) conditions. As can be seen from Table 4, however, although the average change in baseline condi-

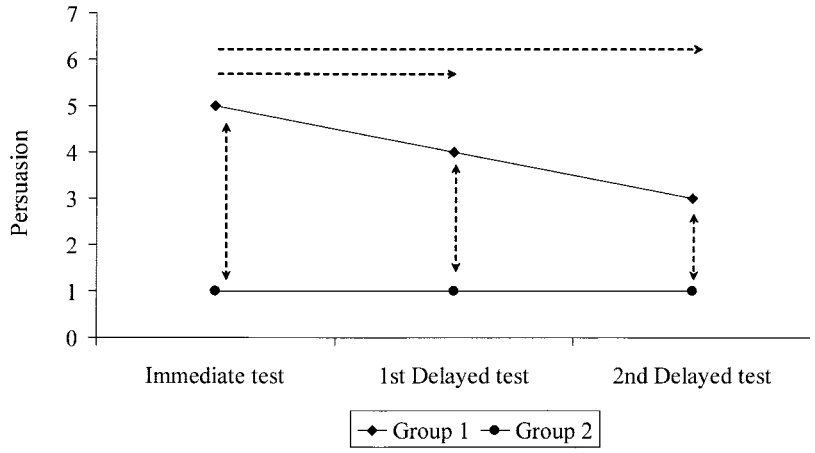

Figure 2. Representation of the effect sizes calculated from each study. Vertical arrows indicate effect sizes calculated for differences among experimental and control conditions. Horizontal arrows indicate effect sizes calculated for within-condition changes over time.

tions did not differ significantly from zero, the average change in baseline attitudes was identical to the change in discounting-cue conditions ($d_+ = 0.08$ in both cases). This similarity suggests that the increase in persuasion for recipients of discounting cues cannot be regarded as an absolute sleeper effect (see Panel B1 of Figure 1); instead, our overall results are best described as a relative sleeper effect, such as the one represented in Panel C3 of Figure 1.

Summary and variability of the overall effect. The overall analyses identified a relative sleeper effect in persuasion but no absolute sleeper effect. The latter was not surprising because the sleeper effect was expected to emerge under specific conditions. In line with our expectations, the homogeneity statistics indicated that the changes in discounting-cue conditions displayed significant variability across datasets (see Table 4). The effect sizes comprising the average effect size ($d_+ = 0.08$) are shown in Figure 3. The heterogeneity apparent from the figure suggests that it would be misleading to assume that the sleeper effect is relative in all conditions, justifying the use of moderator analyses.[17] The moderator analyses had the potential of identifying conditions that stimulate increased persuasion (absolute sleeper effect) rather than stability or even decay in persuasion over time.

Moderator Analyses

Although overall effects have descriptive value, the variability in the change observed in discounting-cue conditions makes it unlikely that the same effect was present under all conditions. Therefore, we tested the hypotheses that the sleeper effect would be more likely (e.g., more consistent with the absolute pattern in Panel B1 of Figure 1) when the initial impact of the message and the cue is larger (rather than smaller), when the discounting cue follows (rather than precedes) the message arguments, and when participants have higher (rather than lower) ability and motivation to think about the message at the time they receive it.

Amount of Initial Change in Response to the Communication

One conclusion from all models of persuasion applicable to the sleeper effect is that the effect is most likely to emerge when the message arguments are strong enough to persuade its recipients but the discounting cue is strong enough to initially suppress the effect of those arguments. When these two conditions are met, the attitudes of the recipients of discounting cues should not be different from baseline attitudes, indicating zero initial change (Cook et al., 1979; Gruder et al., 1978). Of course, absence of change may also result from the use of an ineffective message. However, in our review, messages without the discounting cues led to greater agreement with the communication than control baseline conditions (see the Appendix),[18] suggesting that the messages were indeed effective when presented alone.

Given that the message-only conditions induced significantly greater agreement than control conditions, it was appropriate to

[17] Although there were no extreme effect sizes, two effect sizes (i.e., $d = 1.13$, $d = -1.07$) were relatively distant from the rest of the effect sizes, thus appearing as potential outliers. We used a normal quantile plot to see if these two effect sizes could be classified as outliers. (At the end of the Results section, we present a detailed description of this procedure when we address the issue of publication bias in our review.) The results showed that these effect sizes were outliers. Removal of these two effect sizes did not affect the average effect size (fixed effects: $d_+ = 0.08$, CI = 0.03, 0.13, $Q = 95.20$, $p < .02$, $k = 70$), although it reduced the variability among effect sizes, $\chi^2_{\text{difference}}(2) = 11.82$, $p < .01$. Nevertheless, despite the reduction in heterogeneity, the effect sizes showed considerable variability across studies after excluding the two extreme effects.

[18] The Appendix presents the results for the differences among experimental and control groups at the immediate and delayed posttests. As shown, messages were more persuasive when they were associated with acceptance rather than discounting cues. However, messages accompanied by acceptance cues were not more persuasive than messages presented without cues (i.e., message-only control conditions). We presume that recipients assumed that the sources of communications used in these studies were credible even when the researchers provided no explicit information about credibility. Finally, at the time of the delayed posttest, recipients of different types of persuasive messages were still more persuaded than participants who did not receive a message (baseline conditions). Thus, regardless of the type of cue accompanying the message, the impact of persuasive communications in these experiments did not disappear within the time period observed.

Table 2
Descriptive Summary of Study Characteristics

Characteristic	Value	k[a]
General characteristics		
Median year of report	1982	72
Mean year of report (range = 1951–2003; *SD* = 13 years)	1979	72
Source (%)		
Journal article (%)	88	72
Dissertation, thesis, and other unpublished material	12	72
Sample composition (%)		
High school students	7	72
University students	88	72
Randomly selected adults	5	72
Data sets collected in the United States (%)	89	72
Theoretical moderators		
Presentation time of discounting cue (%)		
Before the message	53	72
After the message	39	72
Simultaneously with the message (e.g., Hovland & Weiss, 1951)	6	72
Discounting cue was embedded within the text of the message	1	72
Ability to think about the issue		
Mean no. of message repetitions (range = 1–5; *SD* = 0.79)	1.49	72
Message repetition (%)		
Yes	60	72
No	40	72
Recipients' prior knowledge on the issue (%)		
Low or none	14	72
Moderate	51	72
High	35	72
Motivation		
Outcome relevance (%)		
Low	71	72
High	28	72
Mixed or no basis for judgment	1	72
Recall/recognition of message content and discounting cue		
Data sets including measures for content memory (%)	40	72
Type of content-memory measure (%)		
Free recall	69	29
Multiple choice	21	29
True–false	3	29
Mixed	7	29
Data sets including measures for memory of the discounting cue	22	72
Type of cue-memory measure (%)		
Free recall	96	22
Multiple choice	4	22
Other characteristics of the experiments		
Design		
No. of posttests (time points) after message exposure (%)		
Two	68	72
Three	6	72
Four	4	72
Multiple measurements in a single session (e.g., Pratkanis et al., 1988)	22	72
Percentage of data sets in which the time variable was a within-subject factor	85	72
Mean interval between message exposure and first delayed test (days; *SD* = 16)	18	72
Mean interval between message exposure and second delayed test (days; *SD* = 10)	24	7
Basis for manipulation of the discounting cue (%)		
Source credibility	71	72
Message disclaimer	15	72
Combination of message disclaimer and reactance-inducing statements	7	72
Presentation of reactance-inducing statements	4	72
Inclusion of reservations in text	3	72
Mean message length in words (range = 72–1,000; *SD* = 329)	510	60 (12)
Mean no. of messages participants received (range = 1–32; *SD* = 9.50)	4.74	66 (6)
Data sets with one-sided messages (%)	96	72
Mean no. of arguments in messages (range = 2–10; *SD* = 2.70)	5	42 (30)

Table 2 (*continued*)

Characteristic	Value	k[a]
Other characteristics of the experiments (*continued*)		
Design (*continued*)		
Message modality (%)		
Written text	75	72
Audio (e.g., tape, radio)	8	72
Film and/or slides	8	72
Mixed media	8	72
Data sets involving an acceptance-cue group (%)	71	72
Data sets involving a message-only control group (%)	44	72
Data sets involving a no-message control group (%)	42	72
Median sample size for discounting-cue groups (range = 9–80)	27	72
Characteristics of the attitudinal issue		
Issue domain (%)		
Sociopolitical	32	72
Consumption	15	72
Environment	4	72
Cultural truisms	13	72
Education	7	72
Health and biology	3	72
Mixed and other	26	72
Discrepancy between message position and initial attitudes of recipients (%)		
Low or none	19	72
Moderate	15	72
High	18	72
No basis for judgment	47	72
Measurement of persuasion		
Type of measure (%)		
Attitude	56	72
Belief	44	72
Median no. of items (range = 1–18)	4.20	72
Data sets reporting reliability information for multiple-item measures (%)	33	54
Type of scale (%)		
Single-item dichotomous scale (i.e., agree–disagree)	6	72
Single-item polichotomous scale	19	72
Multiple-item dichotomous scale	1	72
Multiple-item polichotomous scale	74	72
Context of experiment (%)		
Message exposure and immediate posttest setting		
Laboratory or classroom	94	72
Recipients' house	6	72
Setting of delayed and immediate measurements (repeated measures only)		
Same	42	61
Different	52	61
Mixed	3	61
Not reported	3	61

Note. k = number of applicable data sets from which the relevant descriptive information was drawn.
[a] Numbers in parentheses indicate the number of applicable data sets from which the relevant information could not be drawn.

assume that any null effect of those messages in the presence of a discounting cue was due to the cue's suppression of the effect of the message.[19] Nevertheless, the average initial impact of the communications with discounting cues was not zero (see Table 3), but according to the CI frequently ranged from about $d = 0.10$ to $d = 0.29$. Table 1 includes the individual effect sizes contributing to these average effects. As can be seen, there were effect sizes as large as $d = 0.90$. Therefore, these data provided the ideal conditions to examine the possibility that greater impact of the message and the cue as represented by smaller initial change in discounting-cue conditions would elicit stronger sleeper effects.

Figure 4 displays the relation between the initial and longitudinal change in discounting-cue conditions and suggests that the

[19] The assumption that lack of initial change reflects the simultaneous impact of the message and the discounting cue is reasonable when the message alone induces greater agreement with the topic than the lack of the message (control or baseline conditions), but the message has no effect when it is accompanied by the cue. However, lack of initial change could conceivably reflect absence of both message and cue effects, making our reasoning somewhat tenuous. However, given the data on no-message and message-only conditions, this alternative interpretation is not plausible.

Table 3
Ruling Out the Possibility of a Boomerang Effect

	Fixed effects		Random effects				
Follow-up	d_+	95% CI	d_+	95% CI	Model-fit Q	k	N
Immediate	0.19	0.10, 0.28	0.19	0.09, 0.29	21.79	26	1,547
Delayed	0.30	0.20, 0.40	0.29	0.16, 0.40	28.53	22	1,535

Note. The table presents weighted average effect sizes (d_+s) representing differences between discounting-cue and no-message control conditions. Positive effect sizes indicate that the participants in discounting-cue conditions were persuaded in the direction of advocacy despite the discounting cue. The model-fit Q statistic is an index of homogeneity of the effect sizes included in d_+. In these analyses, the nonsignificant Q values suggest that the effect sizes were relatively homogenous. CI = confidence interval; k = number of data sets.

magnitude of the sleeper effect was indeed greater when the initial change was closer to zero. In contrast, when recipients were initially persuaded by arguments, presumably because of the ineffectiveness of the discounting cue, we observed longitudinal decay rather than increases in persuasion. The fixed-effects analyses that pertain to this association yielded strong support for our prediction ($B = -0.48$, $SE = 0.16$, $p < .01$, $k = 26$), as did random-effects analyses ($B = -0.48$, $SE = 0.17$, $p < .02$, $k = 26$). These findings therefore support previous claims by Cook and his colleagues (Cook et al., 1979; Gruder et al., 1978) that the discounting cue must be effective for the sleeper effect to manifest itself.

The Influence of Cue-Presentation Time and Recipients' Ability and Motivation to Think About the Message

To examine the influence of the cue-presentation time and the recipients' ability and motivation to think about the message, we first computed average effects and CIs for each presentation time of the cue and for each level of ability and motivation. These analyses appear in Table 5, organized by whether the cue was presented first or last and by the levels of the indicants of ability and motivation to process the message. In these analyses, the results of fixed- and random-effects models on weighted average effect sizes and CIs were almost identical because of considerable homogeneity among the effect sizes within each cell. Thus, for presentational purposes we report the results of only the random-effects models. As shown by the CIs in the table, the sleeper effect emerged only when the discounting cues followed the message arguments, and when message repetition, issue relevance, and prior knowledge were high.

To formally examine the interaction between the cue-presentation time and the different indicators of ability and motivation, we separately regressed change in persuasion in discounting-cue conditions on the time of the cue presentation and each indicator of ability and motivation as well as the relevant interaction terms. These analyses are summarized in Table 6. As can be seen from Table 6, regardless of the estimation methods that we used, the time of presentation of the cue interacted with both message repetition and outcome relevance of the issue. The convergence of these results, given moderators that had varying degrees of association with each other, strengthens the conclusions from these analyses.[20]

In sum, the moderator analyses identified the conditions in which the sleeper effect was most likely to occur: when motivated and able recipients of the message processed the discounting cue after having received the arguments. The average effect in these conditions was 0.25 ($p < .01$). This increase in persuasion over time was significantly greater than the baseline increases we reported ($d_+ = 0.08$, *ns*), suggesting absolute sleeper effects.[21] However, because we included 12 of Pratkanis et al.'s (1988) experiments in these analyses, we verified that the observed findings were not guided primarily by these experiments. The findings of supplementary analyses, excluding those 12 studies, support the conclusions inferred from Table 6. For the sake of brevity, however, these analyses are not described in more detail here, but are available from us.

In the analyses depicted in Table 6, we assumed that the use of personally relevant messages, well-known issues, and repeated exposure to the target message would enhance elaborative processing of the message. Nevertheless, the indicators that we used in these analyses did not adequately capture other manipulations of ability and motivation that were used in the literature. Two studies in which the discounting cue followed the message arguments are worth considering in addition to the moderator analyses we described. Mazursky and Schul (1988) used special instructions to enhance elaborative processing of a communication about a new type of car. Specifically, the researchers asked the recipients to imagine that they were personally driving the car and then to write down their thoughts about the specific arguments of the message. Under these high-elaboration conditions, they observed a significant delayed increase in persuasion over time ($d = 0.59$, CI = 0.01, 1.16). However, recipients in low-elaboration conditions, who did not receive these instructions, were less persuaded as time went by ($d = -0.26$, CI = -0.83, 0.31). Similarly, Priester et al. (1999) hypothesized that the sleeper effect would occur only when participants had high chronic motivation to think about information. To test this hypothesis, they measured recipients' level of need for cognition and found evidence of the sleeper effect among those recipients who were high in need for cognition ($d = 0.61$, CI = -0.11, 1.34) but not among recipients who were low in this trait ($d = -0.17$, CI = -0.83, 0.50). Therefore, findings that could not be meta-analyzed are also in line with our hypotheses.

Cue-Presentation Time and Ability and Motivation in Relation to Initial Change

Our analyses had shown that the sleeper effect is contingent on the time of presentation of the discounting cue, the processing ability and motivation of participants, and the amount of initial change. Next, we were interested in determining whether the combination of cue-presentation time and ability and motivation

[20] The indicants of ability and motivation to think about the communications were correlated with each other at varying degrees of association (i.e., the correlation between message repetition and outcome relevance was .33, $p < .01$; the correlation between message repetition and prior knowledge was .48, $p < .01$; the correlation between outcome relevance and prior knowledge was .63, $p < .01$).

[21] We conducted formal tests to see if these differences in slopes of change over time were significant. In each case, the increase in discounting-cue conditions was significantly greater than the increase in baseline attitudes, $B > 0.16$, $SE < 0.08$, $Q_B(1) > 5.80$, $p < .02$.

Table 4
Change in Persuasion Over Time

Condition	Fixed effects d_+	Fixed effects 95% CI	Random effects d_+	Random effects 95% CI	Model-fit Q	k	N
Acceptance cue	−0.21	−0.27, −0.15	−0.23	−0.31, −0.15	76.36**	47	1,589
Discounting cue	0.08	0.03, 0.13	0.08	0.02, 0.13	107.51**	72	2,480
No-message control	0.08	−0.02, 0.20	0.08	−0.02, 0.20	9.19	13	425
Message-only control	−0.11	−0.21, −0.02	−0.11	−0.21, 0.01	20.10	17	517

Note. Effect sizes represent change in persuasion over time across conditions. Effect sizes are positive if there is an increase in persuasion from the immediate to the delayed measurement (e.g., sleeper-effect pattern), and negative if there is decay in persuasion over time. The model-fit Q statistic is an index of homogeneity of the effect sizes included in the weighted average effect size (d_+). Significant Q values indicate rejection of the homogeneity hypothesis. CI = confidence interval; k = number of data sets.
** $p < .01$.

influenced the sleeper effect by increasing the joint influence of the message arguments and the discounting cue, which was implied from the low amount of initial change. If this prediction were true, then amount of initial change should mediate the combined influence of the time of presentation of the cue and the level of ability and motivation on the sleeper effect. To formally test the predicted pattern, we created an indicator variable with two levels representing the absence or presence of conditions eliciting the sleeper effect (see Table 5). Thus, we coded data sets having the cue last and high ability and motivation as conditions in which the sleeper effect occurs.[22] Correspondingly, we coded all other data sets as conditions in which the sleeper effect does not occur. We then used this indicator variable as a predictor of amount of initial change and of increase in persuasion over time in discounting-cue conditions. The path models for each indicator of ability and motivation appear in Figure 5 and show that initial persuasion was greater when the discounting cue preceded the message or when the cue followed the message but ability or motivation were low ($d_+ = 0.29$, CI = 0.15, 0.43, $Q = 11.22$, *ns*, $k = 17$) than when the cue followed the message and ability and motivation were high ($d_+ = 0.11$, CI = −0.01, 0.23, $Q = 6.65$, *ns*, $k = 9$).[23] The formal test of the difference across these groups of data sets was significant, $Q_B(1) = 3.52$, $p < .05$; $Q_E(24) = 17.87$, $p > .80$. In combination with the finding that the amount of initial change correlated with the sleeper effect, these results justified a test of mediation.

1.2	
1.1	3
1.0	
0.9	
0.8	2
0.7	8
0.6	0, 1
0.5	9
0.4	4, 4
0.3	0, 2, 2, 9
0.2	2, 3, 3, 3, 4, 6, 8
0.1	1, 1, 2, 3, 3, 4, 5, 5, 7, 9
0.0	0, 1, 2, 2, 3, 3, 3, 4, 4, 5, 8
-0.0	1, 2, 4, 5, 6, 7, 7, 7, 8, 8, 8, 8, 9, 9
-0.1	0, 3, 5, 6, 7, 9
-0.2	0, 1, 3, 3, 6
-0.3	1, 2
-0.4	5
-0.5	5
-0.6	2, 8
-0.7	6
-0.8	
-0.9	
-1.0	7
-1.1	
-1.2	

Figure 3. For the discounting-cue conditions, stem-and-leaf plot of weighted effect sizes (*d*s) representing changes in persuasion from the immediate to the delayed posttest.

To formally examine amount of initial change as a mediator of the combined effects of the time of presentation of the cue and ability and motivation on the sleeper effect, we used weighted least squares regressions analogous to the ones described by Baron and Kenny (1986). We used our dummy-coded variable representing the conditions that elicited the effect in Table 5 versus all other conditions. Amount of initial change was included as a mediator, and amount of delayed change was the outcome variable. In Figure 5, values outside of the parentheses are univariate unstandardized regression coefficients, whereas values in parentheses are unstandardized path-analytic coefficients. As shown in Figure 5, the magnitude of the sleeper effect was greater when the discounting cues followed the persuasive arguments and ability and motivation were high (repetition, high prior knowledge, and high outcome relevance) relative to the other conditions (Path x), and this change was partially mediated by the magnitude of initial change (Paths y and z). Of importance, however, the analysis showed that conditions in which the cue followed the message and ability and motivation were high continued to influence the sleeper effect above and beyond the initial impact of the message argu-

[22] Note that the procedures we implemented were the only ones possible given the available number of data sets in each condition. Future research should replicate these analyses.

[23] This analysis was conducted on the subset for which scores of initial impact were available was smaller. Unfortunately, in this smaller subset of data, all conditions in which the cue was last and repetition was high were also the conditions in which prior knowledge and outcome relevance were both high. To this extent, the analysis represents the effect of all indicators of ability and motivation to think about the communication.

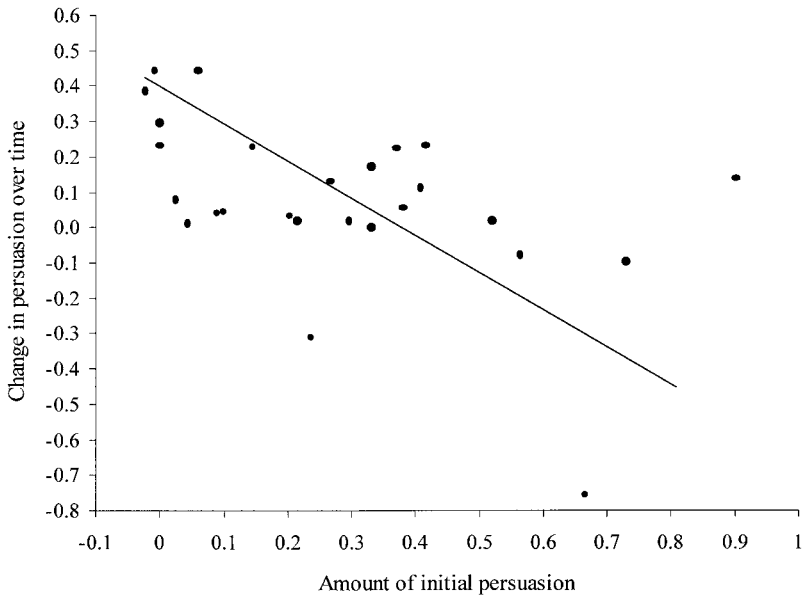

Figure 4. A scatter plot of the relationship between immediate attitude change and delayed attitude change for recipients of discounting cues. Data points are weighted effect sizes (*d*s).

ments and the cue. Consistent with this evidence of partial mediation, the Sobel (1982) test of mediation was not significant, $t(25) = 1.69$, $p < .11$. Next, we discuss the potential role of cue recall/recognition as a complementary mediating mechanism involved in the sleeper effect.

Effects on Recall/Recognition

Various persuasion models have considered the memory mechanisms underlying the sleeper effect, assuming that the sleeper effect occurs because people do not recall the discounting cue at the time of the delayed follow-up. Unfortunately, in the earlier studies of the sleeper effect reported by Hovland and Weiss (1951), recall was combined for conditions that did and did not show the effect, preventing firm conclusions about the role of recall in this phenomenon. However, in this review, we were able to retrieve the percentage of participants who recalled/recognized the cue from 11 data sets (see Table 1). We thus regressed delayed change in persuasion in discounting-cue conditions on this percentage, using the same procedures used for the other moderator analyses. These analyses suggested that, as expected, the recall/recognition of the discounting cue had a negative association with the sleeper effect (fixed effects: $B = -0.01$, $SE = 0.01$, $\beta = -.49$, $p < .05$, $k = 11$), with the effect being larger when recall/recognition was smaller. In addition, a qualitative examination of the data suggested that participants recalled/recognized the discounting cue to a lesser extent when the discounting cue appeared last (e.g., Gruder et al., 1978; Mazursky & Schul, 1988; Pratkanis et al., 1988; which were also conditions in which ability or motivation were high) than when the cue appeared first (i.e., Schulman & Worrall, 1970; Watts & McGuire, 1964).

An alternative examination of the role of recall in the sleeper effect is to compare the delayed recall of the arguments with the delayed recall of the discounting cue. Such a relative test of recall should be useful to determine whether, at the time of the delayed follow-up, participants were more likely to spontaneously remember the message arguments or the discounting cue.[24] Although most of the studies failed to obtain the data that would allow for this comparison, our review included two experiments in which recall of source and message were both measured at the time of the delayed follow-up. The findings from an experiment conducted by Mazursky and Schul (1988, Experiment 1) as well as a recent experiment by Kumkale, Albarracín, and Del Vento (2003) indicate that at the time of the delayed follow-up, recipients of discounting cues indeed recalled more elements from the arguments than from the source and that this differential recall increased the size of the sleeper effect. Although these two studies cannot be meta-analyzed, the findings qualitatively support the hypothesis that memory processes underlie the effect.

Other Methodological Moderators of Sleeper Effect

On the basis of the study characteristics reported in Table 1, we identified other, potentially relevant moderators of the sleeper effect

[24] In this meta-analysis, we attempted to compute effect sizes for longitudinal changes in recall of the arguments as well as of the discounting cue. Although we could compute effect sizes for longitudinal changes in recall of the arguments from 16 data sets, it was not possible to derive effect sizes for longitudinal changes in recall of the discounting cue in these data sets. Therefore, we could not meta-analytically examine the role of relative recall in the emergence of the sleeper effect. Fixed- and random-effects analyses of the data on message recall/recognition revealed that among recipients of discounting cues, recall/recognition of the message content decayed considerably over time (fixed effects: $d_+ = -0.63$, CI $= -0.75, -0.48$; random effects: $d_+ = -0.81$, CI $= -1.18, -0.44$; $Q = 68.98$, $p < .001$; $k = 16$, $N = 392$). A similar decay pattern was evident for recipients of acceptance cues (fixed effects: $d_+ = -0.70$, CI $= -0.85, -0.55$; random effects: $d_+ = -0.90$, CI $= -1.31, -0.50$; $Q = 51.92$, $p < .001$; $k = 12$, $N = 349$).

Table 5
The Effects of Presentation Time of Discounting Cue and Ability and Motivation on the Sleeper Effect

Variable	Presentation before message				Presentation after message			
	d_+	95% CI	Q	k	d_+	95% CI	Q	k
Message repetition								
No repetition	$0.00_{a,b}$	−0.08, 0.09	16.94	23	0.11_b	−0.01, 0.22	17.84	16
Repetition	-0.06_a	−0.18, 0.05	16.81	14	0.24_c	0.15, 0.33	16.30	11
Outcome relevance of issue								
Low	-0.01_a	−0.09, 0.07	25.76	27	-0.09_a	−0.02, 0.21	18.88	16
High	-0.06_a	−0.19, 0.07	8.31	9	0.26_c	0.16, 0.36	9.02	10
Recipients' prior knowledge								
Low or moderate	-0.06_a	−0.15, 0.03	12.29	21	0.05_a	−0.08, 0.18	4.08	8
High	0.02_a	−0.08, 0.12	20.76	16	0.25_c	0.16, 0.34	24.32	18

Note. Positive effect sizes indicate increase in persuasion. Negative effect sizes indicate decay in persuasion. All of the results reported in this table are based on random-effects models. The model-fit statistic Q is an index of homogeneity of effect sizes included in the weighted average effect size (d_+). In each cell, the effect sizes comprising d_+ were homogeneous. Different subscripts indicate statistically significant differences. CI = confidence interval; k = number of data sets.

and conducted exploratory analyses. These moderators include (a) method of cue manipulation (i.e., source credibility vs. message disclaimer), (b) type of persuasion measure (measures of attitudes vs. measures of beliefs), (c) similarity of study settings across measurements (same or similar vs. different), (d) message discrepancy with the audience's attitudes (high, moderate, or low), (e) use of within- or between-subjects measures, and (f) time interval between immediate and delayed tests. Because these analyses were exploratory and large in number, we used Bonferroni procedures to ensure an alpha level of .05 across all analyses. Both fixed- and random-effects models identified strong effects of only the method of manipulating the discounting cue. According to the fixed-effects models, the sleeper effect emerged when researchers used message-disclaimer notes as discounting cues ($d_+ = 0.22$, CI = 0.13, 0.31, $Q = 23.72$, $p < .05$, $k = 15$) but not when they used noncredible sources as discounting cues, $d_+ = 0.02$, CI = −0.04, 0.08, $Q = 38.43$, *ns*, $k = 45$; $Q_B(1) = 13.39$, $p < .001$. Random-effects estimates provided converging evidence ($d_+ = 0.23$, CI = 0.11, 0.35 vs. $d_+ = 0.02$, CI = −0.04, 0.08). It should be noted, however, that the overall effect of source disclaimer was confounded with the presentation time of cue because message-disclaimer notes were presented after the message arguments in all cases. Therefore, we analyzed whether the sleeper effect emerged when the noncredible source was presented at the end. In these analyses, the increase in persuasion was significant when the noncredible source followed the arguments ($d_+ = 0.09$, CI = −0.03, 0.21, $Q = 6.00$, *ns*, $k = 9$) but not when the noncredible source preceded the arguments ($d_+ = -0.02$, CI = −0.09, 0.04, $Q = 34.60$, *ns*, $k = 36$; for across-conditions differences, $B = 0.11$, $SE = 0.06$, $p < .08$). Still, the effect of the disclaimer presented at the end tended to be stronger than the effect of the noncredible source presented at the end ($B = 0.13$, $SE = 0.09$, $\beta = .30$, $Q_B = 2.88$, $p < .09$). None of the other moderators, including time interval between measurements and recall/recognition of message content, had

Table 6
Influence of Timing of Discounting Cue and Ability and Motivation to Think About the Message on Change Over Time

Variable	Fixed effects		Random effects		Model fit	
	B	SE	B	SE	df	Q_E
Message repetition	0.15*	0.07	0.16*	0.07		
Presentation time of cue	−0.10	0.07	−0.10	0.07	59	62.83
Interaction term	−0.22*	0.10	−0.22*	0.10		
Outcome relevance of issue	0.18*	0.07	0.18*	0.07		
Presentation time of cue	−0.10	0.07	−0.10	0.07	58	63.15
Interaction term	−0.22*	0.10	−0.22*	0.11		
Recipients' prior knowledge[a]	0.22**	0.08	0.22**	0.08		
Presentation time of cue	−0.09	0.08	−0.09	0.08	59	59.57
Interaction term	−0.16	0.10	−0.16	0.10		

Note. Model-fit statistic (Q_E) is informative about whether or not significant systematic variation remains unexplained by the model. A significant Q_E value indicates that the model does not account sufficiently for the heterogeneity among effect sizes. B = unstandardized regression coefficient.
[a] In this analysis, we merged the two levels of the prior knowledge indicator (i.e., low or none, moderate) because of unbalanced cells.
* $p < .05$. ** $p < .01$.

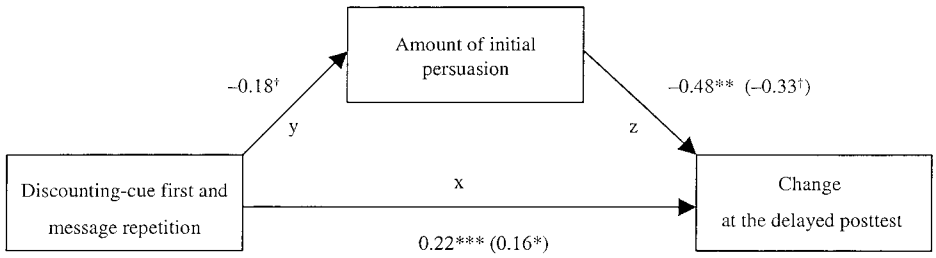

Figure 5. Path analysis of the impact of the presentation time of the discounting cue on the amount of initial and subsequent attitude change (fixed-effects models). The indicator variable was dummy coded (the condition in which the cue followed the arguments and ability and motivation were high = 1; other conditions of the design depicted in Table 5 = 0). † $p < .06$. * $p < .05$. ** $p < .01$. *** $p < .001$.

significant effects on change in persuasion in discounting-cue conditions.

Assessment of Publication Bias

We assessed the likelihood of publication bias using two graphical methods. First, we examined the funnel plot of effect sizes representing change over time in discounting-cue conditions (see Figure 6). In the absence of bias, the plot takes the form of a funnel centered on the mean effect size, with smaller variability as the sample size increases. Instead, in the presence of publication bias, there is a distortion in the shape of the funnel. If the true effect size is zero and there is publication bias, the plot has a hollow in the middle. If the true effect size is not zero, the plot tends to be asymmetrical, having a large and empty section where the estimates from studies with small sample sizes and small effect sizes would otherwise be located. The funnel plot in Figure 6 shows that the effect sizes were distributed fairly symmetrically around the mean weighted effect size. This plot thus suggests no publication bias in our meta-analysis.

The funnel plots are helpful for exploratory purposes, but they are limited because of the subjectivity involved in evaluating the shapes of the distributions. A better graphical method to identify publication bias is the normal quantile plot (Wang & Bushman, 1998). In a normal quantile plot, the observed values of a variable (in this case, the effect sizes for discounting-cue conditions) are plotted against the expected values if the sample were drawn from a normal distribution. If the sample is from a normal distribution, data points cluster around a straight line, whereas presence of publication bias implies deviation from a normal distribution. If the standardized effect sizes follow a straight line and fall within the 95% CIs in the plot, one should conclude that the distribution is normal and no publication bias exists (Wang & Bushman, 1998). Figure 7 shows the normal quantile plot of the longitudinal effect sizes in discounting-cue conditions synthesized in this review. As can be seen, the standardized effect sizes follow a straight line and fall within the 95% CIs. The two effect sizes outside of the CIs were outliers and were excluded from our analyses (see Footnote 17). Therefore, the results reported in this review may be interpreted as representing the sleeper effect research in a relatively unbiased way.

Discussion

Research on the sleeper effect has undoubtedly produced the most impressive collection of cumulative experiments in the do-

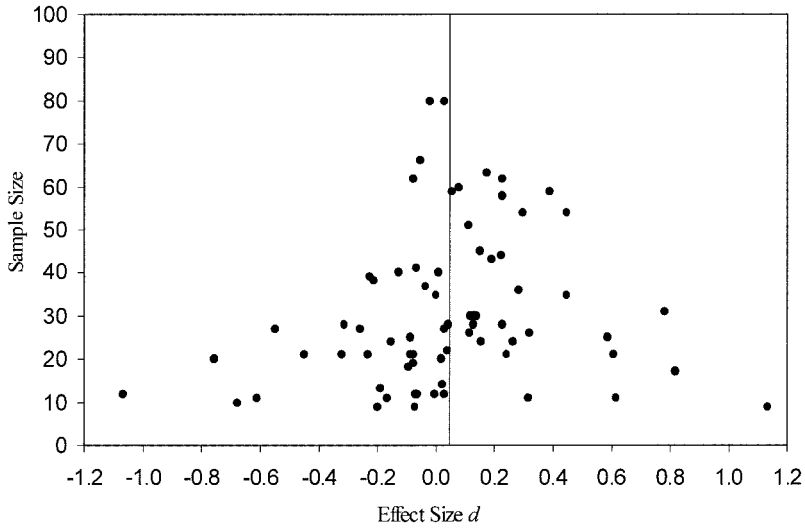

Figure 6. Funnel plot of effect sizes against sample sizes. The effect sizes represent the magnitude of change in persuasion in discounting-cue conditions from the immediate to the delayed posttest. The solid vertical line represents the weighted average effect size.

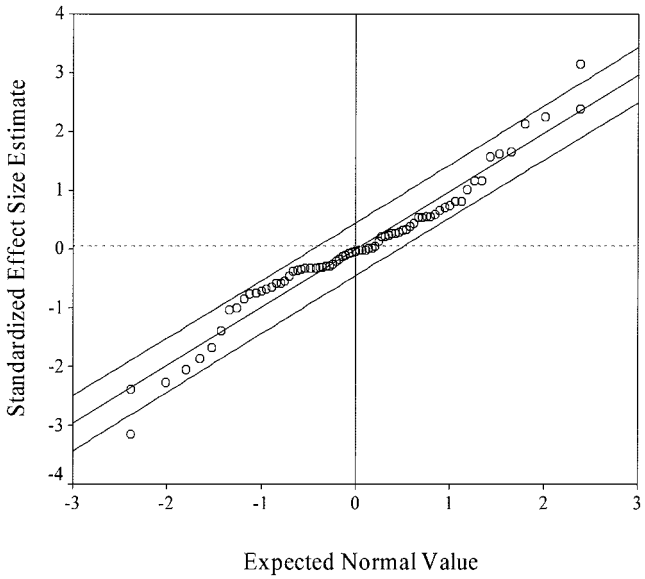

Figure 7. Normal quantile plot of the effect sizes representing the magnitude of change in persuasion in discounting-cue conditions from the immediate to the delayed posttest.

main of persistence of persuasion (see Table 1). This research has allowed investigators to estimate longitudinal change in attitudes and beliefs under various conditions, including various types of topics (Hovland & Weiss, 1951), alternative instructional sets (Hennigan, Cook, & Gruder, 1982), high and low chronic motivation to process information (Priester et al., 1999), and different placement of the discounting cue in the communication (Pratkanis et al., 1988). Despite the variety of prior findings in the literature, our work advances knowledge about attitudes and persuasion in important ways.

To begin with, our meta-analysis is the first to statistically test an assumption that all models of persuasion share when considering the sleeper effect: Both the message and the cue need to have strong, antagonistic impacts for the sleeper effect to emerge. In this regard, we drew from Cook et al.'s (1979) hypothesis that when both the arguments and the discounting cue are effective, the initial impact of the communication is null. Moreover, we have provided the first statistical test for the possibility that the amount of initial change has an inverse association with the sleeper effect. Perhaps a statistical necessity, in our review, as amount of change increased, the sleeper effect weakened (see Figure 4). Consistent with the need for adequate tests of the sleeper effect, when recipients presumably failed to discount the message initially, there was no delayed increase in persuasion. However, the magnitude of the sleeper effect was much larger when the discounting cues were strong enough to suppress the immediate impact of the messages. These findings confirm that a great deal of the controversy about the existence and magnitude of the effect (see Capon & Hulbert, 1973) was likely due to the difficulties in identifying the right conditions and not to an intrinsic "unreliability" of the phenomenon (see Cook et al., 1979; Gruder et al., 1978).

Another important contribution of our meta-analysis is that it is the first research synthesis to examine predictions of the elaboration-likelihood and the heuristic-systematic models. The reported findings clearly suggest that as conceptualized by the heuristic-systematic model, the systematic influence of the persuasive arguments can coexist with the heuristic influence of the discounting cue. Although our work in no way addresses the key assumptions of these theories, our findings also support prior assertions by Petty and Cacioppo (1986) about the conditions that elicit the sleeper effect. Among several other possibilities, these researchers predicted that the effect could emerge when message recipients are sufficiently able and motivated to process the arguments contained in the message and when the cue appears at the end of the message, thus preventing biased processing as well as complete disregard of the cue. Our meta-analysis shows that this prediction is viable. As summarized in Panel A of Figure 8, we found stronger, more absolute sleeper effects when the discounting cue was last and when the message was repeated rather than presented only once, the topics were personally relevant rather than irrelevant, and the prior knowledge of the audience was high rather than low. However, we found greater stability in persuasion both when the cue was first, and when the cue was last but ability or motivation were low (see Panels B–D in Figure 8). Future research might provide confirmation of the interaction between the position of the cue and processing ability and motivation within the context of particular experiments. Moreover, an accumulation of such interaction tests at the level of primary experiments should allow future reviews to examine the pooled magnitude of the interaction as evident within studies, an analysis that was not possible given the current state of the literature.

Although social psychologists have conceptualized the mechanisms that mediate the sleeper effect, the most important mechanisms have gone largely unexplored for a number of years. In this context, a significant contribution of our work is to clarify these mechanisms. Because primary research never examined the amount of initial impact as a mediator, our review is the first to explicate the role of the persuasive impact of the message arguments and the cue in producing the effect. Furthermore, because most research on the sleeper effect has failed to measure participants' recall/recognition of the discounting cue, a comprehensive review was necessary to reconstruct the patterns of recall that elicit the sleeper effect. Thus, we have provided the most reliable evidence to date that the sleeper effect is also a function of diminished recall/recognition of the discounting cue as time goes by.

There are also several empirical advantages of our review relative to past research on the sleeper effect. Although the prior primary research was vast, our review is the first to test the influence of various moderators that no one has yet examined, including outcome relevance, prior knowledge, type of persuasion measure, and message discrepancy with the audience's attitudes. It shows that prior knowledge about an issue and outcome relevance increased delayed persuasion in the way described in Figure 8. Equally important, our analyses show that the effect generalizes across different levels of discrepancy between the advocacy and prior attitudes and is apparent when one measures evaluations of the issue as well as beliefs about the probability of certain attributes or consequences of the issue. Furthermore, our analyses were all based on fixed- and random-effects assumptions and yielded converging results, which bolsters confidence in the review's findings.

Another important methodological finding is that the length of time between the presentation of the communication and the delayed measures of persuasion did not moderate the effect. This

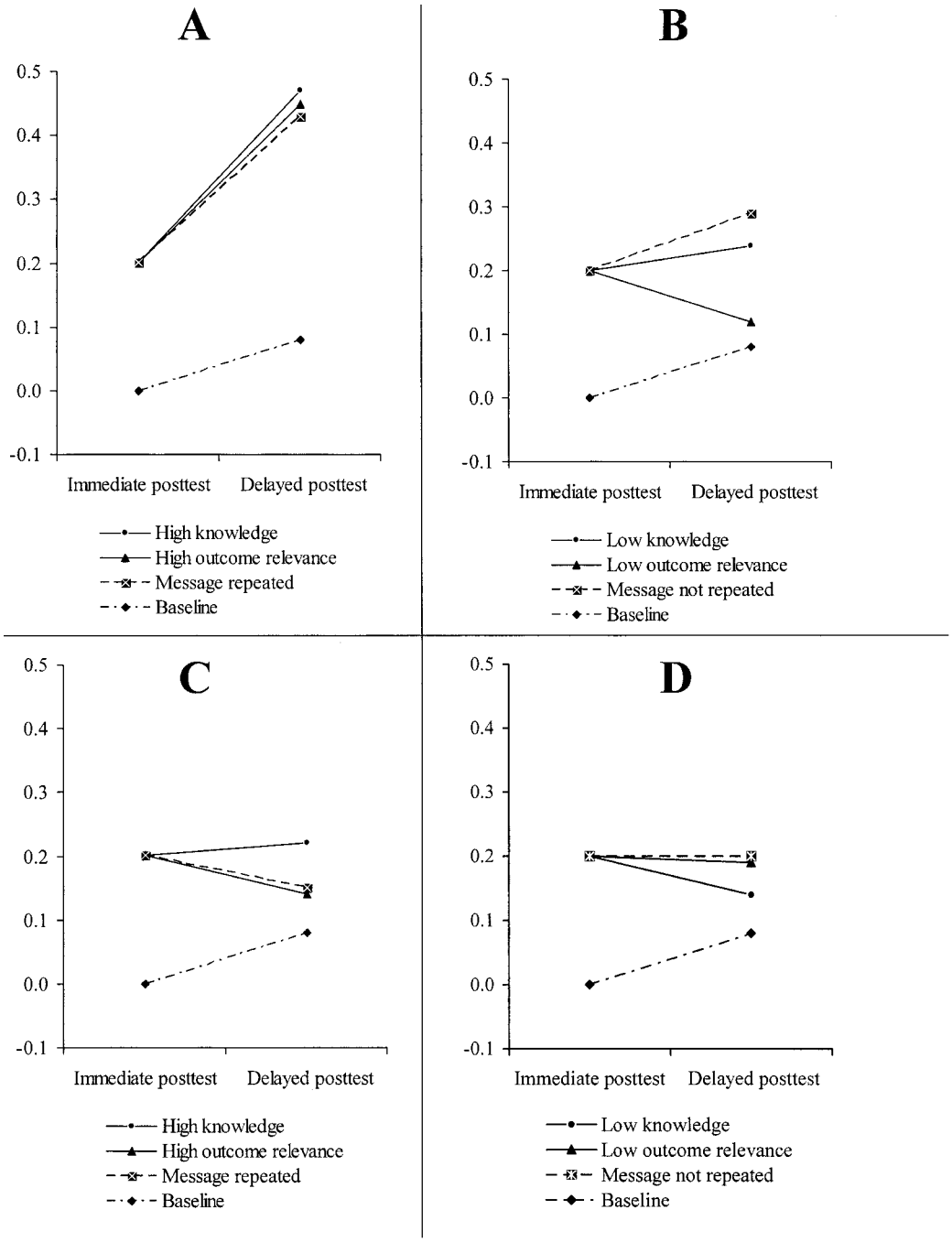

Figure 8. Summary of the effects of time of cue presentation and processing ability and motivation. A: cue-after-message and high ability and motivation condition. B: cue-after-message and low ability and motivation condition. C: cue-before-message and high ability and motivation condition. D: cue-before-message and high ability and motivation condition.

finding suggests that, although time is likely to decrease persuasion when the cue and the message have the same implications, when the cue and the message arguments have antagonistic effects, longer times do not elicit changes in persuasion. Presumably, longer times reduce the recall of the message arguments as much as the recall of the source instead of only decreasing the recall of the source, as would be necessary for time to moderate the sleeper effect. Future measures of differential recall of the message and the source may clarify this possibility.

The Magnitude and Importance of the Sleeper Effect

This meta-analysis indicates that in conditions of higher ability and motivation in which the discounting cue was first, the average

sleeper effect was $d_+ = 0.25$, and the effects fell between 0.15 and 0.36 95% of the time. To this extent, the effect was most often small to moderate, even when the discounting cue followed the message and the processing ability and motivation of participants were high. Of course, researchers do not ignore small effects, because even small effects may imply that most recipients of persuasive communications change their attitudes and behaviors in ways that have real consequences for their lives, as well as the lives of others (for meta-analyses of the attitude–behavior relation, see Albarracín, Johnson, Fishbein, & Muellerleile, 2001; Kraus, 1995; Sheppard, Hartwick, & Warshaw, 1988). However, two considerations seem relevant in assessing the importance of the effect. First, any factor that increases initial message impact, as well as variables that increase initial suppression of the message effect, should stimulate sleeper effects above and beyond a *d* of 0.25. For instance, an examination of the second column of Table 1 suggests that two data sets had sleeper effects despite the fact that the discounting cue preceded the message. (One of these effects was excluded as an outlier; the other was included in our analyses.) In both of these experiments, Hannah and Sternthal (1984) ensured maximal attention to the message and to the cue by using various strategies such as asking several questions about each piece of information just prior to the administration of the attitude or belief measures. In addition, the average magnitude of the sleeper effect in the conditions in which the effect does occur is the same average magnitude of the decay observed in acceptance-cue conditions. Thus, the longitudinal change involved in the sleeper effect is no smaller than other trajectories of change.

Consistency of Our Findings With Pratkanis et al.'s (1988) Findings

We also verified that our conclusions were consistent with Pratkanis et al.'s (1988) findings. Because these authors reported that the sleeper effect occurred when the cue was last but did not examine the influence of ability and motivation to think about the message, their findings may appear inconsistent with the conclusions from our meta-analysis. However, we believe that this discrepancy is only illusory. If one considers the direction of their effects, the results of the 25 tests they conducted were in line with the findings that we report in this synthesis. Moreover, if one meta-analyzes their experiments, it is possible to separate conditions of high and low ability in terms of experiments in which the researchers instructed participants to underline the main points of the message as they read them (high ability) or provided no such instructions (low ability). This procedure results in a weighted average effect size of 0.26 when the cue was last and ability was high, but the weighted average effect size ranges between 0.01 and 0.04 in all other conditions. Clearly, these results closely replicate the findings shown in Table 5. Moreover, even though Pratkanis et al. did not examine the influence of ability and motivation, they reported that underlining the message text should increase the impact of the message and, consequently, the sleeper effect. These findings are thus consistent with our conclusion that, on average, the effect is stronger and more absolute when the cue comes last and when people's ability and motivation are sufficiently high to permit adequate impact of the message arguments and the discounting cue.

The Underlying Mechanisms of the Sleeper Effect

The present meta-analysis is the first to clarify the message impact, recall, and judgment processes that produce sleeper effects in persuasion. At the same time, it points to deficiencies in the current literature that future research could correct.

Message impact mechanisms. Most theorizing about the sleeper effect has emerged from attempts to understand the initial impact of a persuasive communication. However, prior tests of the moderators did not clarify the role of the message impact in producing the effect. For example, Pratkanis et al. (1988) rigorously tested the possibility that the sleeper effect might take place when the discounting cue follows (rather than precedes) the message arguments and found that the presentation time of the cue was critical for the emergence of the sleeper effect. Our meta-analysis not only confirms these earlier conclusions but also reveals that the influence of the position of the cue on the sleeper effect is likely to be mediated by the amount of initial change the communication induces. When this change is close to null, the influences of the cue and the message presumably cancel each other out, thus generating the conditions for the cue to dissipate and for the initial impact of the message to become visible. To this extent, our work is the first to clarify the processes that mediate the influence of the time of presentation of the cue as well as the influence of processing ability and motivation. Further work should include checks for the initial impact of the communication by measuring change within subjects or by obtaining appropriate baseline measures from independent groups of participants.

Recall mechanisms. It may be discouraging to find out that many decades of research on an effect that hypothetically results from limitations in human memory has included almost no tests of recall/recognition of the discounting cue. However, our meta-analysis shows that the 11 data sets that included data on the delayed recall/recognition of the source indicated that, as one might expect, greater recall/recognition of the source was associated with a weaker sleeper effect. This conclusion may appear to contrast with Hovland and Weiss's (1951) conclusion that participants generally recalled the source at the time of the delayed follow-up. However, their data indicate, at best, two marginal sleeper effects out of four data sets. As a result, those researchers should have examined recall only in conditions in which the sleeper effect was present to begin with, as opposed to pooling the source-recall findings as they did. We hope that in the future, researchers investigating attitude change will be careful to include appropriate measures of recall and accessibility to test hypotheses about memory.

An important question with regard to the role of recall is that of whether our findings support the forgetting hypothesis or the dissociation hypothesis. In this article, we reported that when people had more difficulty in reporting the discounting cue, they were more likely to experience the sleeper effect. At first sight, it might appear that an inability to describe the discounting cue would be an indication that message recipients entirely forgot the discounting cue. As we mentioned before, however, the unavailability of representations in memory cannot be easily disentangled from the representations' reduced accessibility. More adequate tests of memory might be necessary to separate the two interpretations. For instance, people may not recall the discounting cue if asked to report it, giving the impression that the cue is unavailable. Nevertheless, they may recognize the cue in response to more

specific questions (recognition measures), implying that the cue is still accessible with appropriate retrieval cues. Such a pattern would indicate that the discounting cue is simply less accessible over time rather than completely unavailable. Future research with both types of measures should clarify this possibility.

Constructive judgment processes. Several decades of attitude research have considered the possibility that people who previously formed an attitude toward an issue can either retrieve and report this attitude again in the future or construct a new attitude online (for an analysis of the memory-based and online distinction, see Hastie & Park, 1986). The essence of the sleeper effect is that at the time of the delayed follow-up, people do not simply re-report the attitude they reported at the immediate follow-up. The interpretation of the effect can thus illuminate the controversy between memory-based and online processes.

In considering the problem of how judgments are generated, the sleeper effect could imply either that the message recipients do not form an attitude at the time of receiving the message or that the message recipients reconstruct their attitudes at the time of the delayed follow-up even when they previously formed an attitude online. In this regard, Mackie and Asuncion (1990) argued that an online construction of the attitude the second time is more likely when individuals did not form an attitude online at the time they received the communication. However, in the sleeper effect research, all participants were asked to report their attitudes in responding to the questionnaire administered at the immediate follow-up. Thus, the sleeper effect findings suggest that some attitude reconstruction takes place despite the presence of a prior attitude judgment in memory.

The question of why people reconstruct their attitudes even when they possess a prior attitude in memory exceeds the scope of our analysis (for an excellent analysis, see Schwarz & Bohner, 2001). However, three possibilities are worth mentioning. First, people may simply have difficulty retrieving their prior attitudes. As suggested by Bem and McConnell (1970), people often use whatever criterion comes to mind at the time they are asked to report their attitudes, and they even distort the recall of the earlier attitude using their present online responses. This hypothesis deserves further investigation but comes into question given research that multiple attitudes are often present in memory and that these multiple attitudes each emerge under specific conditions (see Wilson, Lindsey, & Schooler, 2000). Second, people may have little trust in attitudes that were based on conflicting information and may be eager to revise these attitudes when they have an opportunity. If this is the case, however, one should observe attitude stability when the (strong) messages we reviewed are accompanied by an acceptance cue, which was not the case in our meta-analysis.

Finally, people's overall judgments may be more influential early on, but the information on which those judgments were based may exert more influence as time goes by because of the differential weight of recency and frequency in recall (see Albarracín, Wallace, & Glasman, in press). According to Wyer and Srull (1989), individuals store and retrieve information in and from memory just as they store and retrieve folders in and from a box. People who generate a response on the basis of information about an object are likely to store that information in memory in the order in which it was processed. Moreover, Wyer and Srull argued that people retrieve information from memory in exactly the reverse order of how they store it, with people typically recalling their response before they recall the material on which they based that response. Therefore, it seems likely that people retrieve a response from memory before they recall the information associated with that response. Nevertheless, as time goes by, frequency predominates over recency (see Wyer & Albarracín, in press). Thus, although a recent judgment may take the upper hand over the associated information, more remote prior judgments may have a lesser weight than the information associated with that attitude if there are more pieces of information (e.g., persuasive arguments) than actual judgments. The relative influence of recency and frequency with respect to judgments and specific judgment-associated information is an issue that future research should address.

Other Considerations and Future Directions

Conditions for absolute persistence. One might expect that presenting the discounting cue prior to the message may lead to decreased persuasion over time (Eagly & Chaiken, 1993; Pratkanis et al., 1988; Priester et al., 1999). Such an expectation could be based on the fact that people might tune out as soon as they learn that the message source is not credible and thus not absorb the message arguments at all (Petty & Cacioppo, 1986). This meta-analysis, however, reveals that the recipients in these conditions were persuaded despite the discounting cue, and there was absolute persistence of persuasion in these conditions relative to conditions in which the cue appeared at the end.

There are two reasons why presenting a discounting cue at the beginning of a communication could increase persistence in persuasion. Both of these were discussed by Petty and Cacioppo (1986). First, recipients may disregard the discounting information once they start processing the cogent arguments of the message and see that they have merit despite the expectations induced by the discounting cue. If this is the case, then the discounting cue should have no impact on agreement with the advocacy when the cue appears at the beginning of the communication. Alternatively, the discounting cue may bias the processing of the persuasive arguments, thus decreasing initial persuasion but still allowing for elaborative processing of the arguments and ensuring stability in attitude change. If this possibility were viable, then one should observe attitude stability when the cue is presented first and ability and motivation to think about it are higher rather than lower. Correspondingly, one should observe decay when the cue is presented first and processing ability or motivation are low, because those should be the conditions that elicit peripheral, short-lasting processing of the discounting cue. Unfortunately, restrictions in the report of initial agreement with the message compared with the control groups for different levels of ability and motivation prevented us from examining this important question. However, a broader exploration of attitude change processes could do so.

Cognitive conflict and persuasion. Past research has shown that cognitive conflict of the kind present in the sleeper effect can enhance issue-relevant thinking (e.g., Albarracín, 2002; Baker & Petty, 1994; Berlyne, 1965; Chaiken & Maheswaran, 1994; Eagly, Chaiken, & Wood, 1981; Jonas, Diehl, & Bromer, 1997; Maheswaran & Chaiken, 1991; Petty, Fleming, Priester, & Feinstein, 2001). Thus, receiving a discounting cue first and then receiving cogent arguments may lead communication recipients to engage in further elaboration of those arguments. Importantly, our meta-analysis speaks to the adequacy of this hypothesis by allowing for a comparison of sound arguments accompanied by either credible

or noncredible information. This hypothesis implies that discounting cues should elicit more maintenance than acceptance cues, as was the case in our meta-analysis (see Table 4). Further research should examine the processes underlying this effect.

Structure of memory representations. It is puzzling that all prior theoretical formulations state that the discounting and acceptance cues are more prone to memory decay than the representations of the message. This assumption has never been examined either conceptually or empirically and might have hindered research progress. We believe that an understanding of the sleeper effect, as well as of other phenomena of attitude persistence, clearly necessitates an explication of the structure of the representations in memory. In this regard, Wyer and Srull's (1989) associative network model may provide insights into this question. The basic premise of this model is that representations are organized around a header that allows people to access information from memory. For instance, recipients of messages during a presidential campaign may organize the relevant information around the major issues a candidate discusses. If the candidate talks about abortion, the candidate's arguments about abortion may be stored under the header of an existing abortion bin. Although such a representation would contain information about the arguments as well as information about the candidate, the header would direct retrieval of the message arguments and reduce retrieval of source-related information (e.g., background of the candidate).

Imagine what might happen when the representation is organized around the identity of the source. In this case, people are likely to store the arguments of the persuasive message under a source header and consequently are likely to later recall the arguments with greater difficulty than the information about the source. To the extent that source information is more "memorable" or "accessible" than the arguments in these conditions, one might observe a delayed effect of the source cues as opposed to a delayed effect of arguments contained in the message. This possibility has never been tested, resulting in a relatively narrow analysis of potential increases in persuasion over time.

A theory of attitude-change maintenance and change. Several bodies of research and theory have identified mechanisms that have different implications for the change and maintenance of attitudes over time. These mechanisms entail recalling a prior attitude about an object (Fazio, Sanbonmatsu, Powell, & Kardes, 1986), considering online attitude-related information (Hovland et al., 1953), and evaluating the prior attitude in light of the attitude-related information (Anderson, 1981). However, until recently (Albarracín et al., in press) there has been no general model of attitude change that integrates these mechanisms or explicates the nature and implications of the processes that unfold when people evaluate their prior attitude vis-à-vis attitude-related information. Future research on the sleeper effect may be guided by a more thorough examination of these processes.

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(*Appendix follows*)

Appendix

Between-Conditions Differences in Persuasion at Each Time Point

Condition 1	Condition 2	Follow-up	Fixed effects d_+	Fixed effects 95% CI	Random effects d_+	Random effects 95% CI	Model-fit Q	k	N
Discounting cue	Message only	Immediate	−0.31	−0.40, −0.22	−0.32	−0.43, −0.21	35.76	27	1,972
		Delayed	−0.02	−0.11, 0.08	−0.02	−0.11, 0.08	34.88	25	1,815
Acceptance cue	Discounting cue	Immediate	0.45	0.37, 0.52	0.49	0.38, 0.60	80.53***	45	2,647
		Delayed	0.24	0.17, 0.31	0.25	0.15, 0.35	88.05***	47	2,861
Acceptance cue	Message only	Immediate	0.09	−0.02, 0.21	0.09	−0.02, 0.21	4.67	20	1,216
		Delayed	0.05	−0.07, 0.18	0.05	−0.07, 0.18	8.44	18	1,052
Acceptance cue	No message	Immediate	0.63	0.49, 0.78	0.74	0.48, 0.99	47.11***	16	824
		Delayed	0.30	0.16, 0.44	0.30	0.16, 0.44	11.43	16	824
Message only	No message	Immediate	0.56	0.42, 0.69	0.55	0.35, 0.75	31.09**	15	912
		Delayed	0.34	0.20, 0.47	0.32	0.15, 0.49	20.75	15	908

Note. Effect sizes were calculated by taking the mean in Condition 2 minus the mean in Condition 1. Positive numbers indicate that agreement with the advocacy was greater in Condition 1 than Condition 2, whereas negative values indicate that Condition 2 generated greater agreement with the message topic than Condition 1. The model-fit Q statistic is an index of homogeneity of the effect sizes included in the weighted average effect size (d_+). Significant Q values indicate rejection of the homogeneity hypothesis. CI = confidence interval; k = number of data sets.
** $p < .01$. *** $p < .001$.

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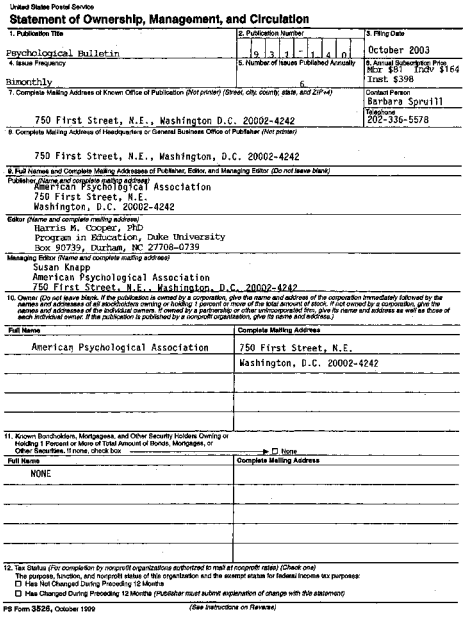

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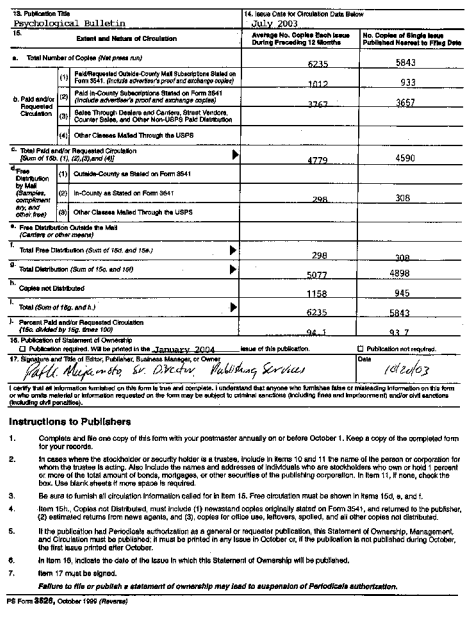

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