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Environmental Sustainability and Behavioral Science: Meta-Analysis of Proenvironmental Behavior Experiments

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Abstract

To provide practitioners with useful information about how to promote proenvironmental behavior (PEB), a meta-analysis was performed on 87 published reports containing 253 experimental treatments that measured an observed, not self-reported, behavioral outcome. Most studies combined multiple treatments, and this confounding precluded definitive conclusions about which individual treatments are most effective. Treatments that included cognitive dissonance, goal setting, social modeling, and prompts provided the overall largest effect sizes (Hedge's $g > 0.60$). Further analyses indicated that different treatments have been more effective for certain behaviors. Although average effect sizes are based on small numbers of studies, effective combinations of treatments and behaviors are making it easy to recycle, setting goals for conserving gasoline, and modeling home energy conservation. The results also reveal several gaps in the literature that should guide further research, including both treatments and PEB that have not been tested.

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Environmental problems are becoming more acute with each passing year. On a global scale, these problems include climate change; armed conflicts over resources, particularly oil; and pollution of the air, water, and soil. The ultimate impacts of these problems are drastic changes to quality and quantity of all life, including human life. The likelihood and severity of these problems have been documented elsewhere (e.g., Halweil et al., 2004; Nickerson, 2003; Oskamp, 2000; Vlek & Steg, 2007) and detailing them is beyond the scope of this research report; however, they are the driving force for this project.

There are many possible approaches to help ameliorate environmental problems: government policy, international agreements, corporate leadership, educational programs, technological innovations, and so on. But one area that should not be overlooked is small-scale or low-cost attempts to influence individuals' behavior. Most people are not in positions of power where they can directly influence government or corporate policy, but all people consume materials and energy in their daily lives, and as such, each person can choose to adopt behaviors that are comparatively better for the environment. These behaviors are called proenvironmental behaviors (PEB), although they also are referred to as conservation behaviors, environmentally friendly behaviors, environmentally significant behaviors, environmentally sustainable behaviors, and responsible environmental behaviors.

It is true that all people can start doing new PEB to reduce their environmental impact, but it is also true that the vast majority of people do not do as much as they could. Thus, applied psychologists and practitioners of the environmental movement—be they researchers, teachers, government employees, leaders of not-for-profit environmental organizations, or other concerned advocates—have the task of motivating people to engage in more PEB. There are many techniques that can be used to get people to engage in more PEB—providing information or instruction, creating incentives, making it easier or more convenient, providing feedback, and so on—but no comprehensive review compares these various techniques and provides practitioners of the movement with guidance about which techniques are effective for which behaviors. In this report, we review the literature in which researchers have attempted to increase people's PEB, and we offer a complete list of treatments or interventions that have been used in the literature, their relative effectiveness, and the efficacy of combinations of treatments and PEB. Our

ultimate goal is to provide practitioners with useful information about how to promote PEB.

Research on PEB has utilized both correlational and experimental methodologies. For the correlational studies, the data are collected by using surveys that ask participants about various psychological processes and how often they engage in PEB. A recent meta-analysis of 46 published correlational reports identified eight psychological constructs that are related to PEB: problem awareness, internal attribution, social norms, feelings of guilt, perceived behavioral control, attitudes, moral norms, and intentions (Bamberg & Möser, 2007; see also Hines, Hungerford, & Tomera, 1987). Other variables on which research has been done include values (e.g., Schultz et al., 2005), personality variables (Balderjahn, 1988; Shackelford, 2006), and identity processes (e.g., Clayton & Opotow, 2003), and it is reasonable to imagine that there may be additional psychological constructs that need to be explored.

The experimental studies focus on treatments, interventions, or manipulations that can encourage people to do more PEB. In the typical experiment, one or more interventions are compared with a control group to determine how much desired behavior they generate, and the outcome variable is an observable behavior performed in the context of normal everyday life. Examples of these experimental manipulations include putting recycling bins in individuals' offices versus a central location, providing feedback regarding energy use in a household, offering incentives like free bus tickets to encourage use of public transportation, and putting stickers on light switches reminding people to turn off lights when they leave the room. Because the correlational studies have already been meta-analyzed (Bamberg & Möser, 2007), the present meta-analysis focuses exclusively on intervention-based research in which various factors thought to increase PEB are manipulated and behavioral outcomes are measured objectively.

Reviews of the Literature

There is no clear beginning to the environmental movement, but credit is often given to leaders like Aldo Leopold and Theodore Roosevelt for being the fathers of the movement in the early parts of the 20th century. Similarly, it is difficult to trace the beginning of the movement within the scientific psychology community. Some of the earliest work was done on purchasing beverages in returnable bottles (Geller, Wylie, & Farris, 1971) and on increasing bus ridership (Everett, 1973; Everett, Hayward, & Meyers,1974), although the classic social psychology work on teaching housewives to prepare organ

meats during the war could be interpreted as PEB (Lewin, 1947). By the 1980s, enough research had been generated on PEB that reviews of it were meaningful, including two seminal books (Cone & Hayes, 1980; Geller, Winett, & Everett, 1982). In this section, first, we summarize the quantitative reviews of the experimental literature on PEB; second, we examine narrative reviews that have looked at the literature as a whole; and third, we examine narrative reviews that focused on specific PEB, namely, recycling and conserving energy.

In each of the reviews discussed below, the researchers proposed a list of possible interventions or treatments for promoting PEB. None of these earlier publications described how they generated their lists of interventions, but it can be assumed that they used an inductive method of examining all the studies and then creating a list that comprehensively captured all of the treatments. We prepared a list of all the treatments that had been generated by earlier reviewers and then analyzed all studies to determine which types of interventions they used.

Table 1 provides a summary of interventions based on previous literature reviews as well as our more comprehensive list based on those reviews and more recent literature. The columns in the table are for the categories of interventions that we developed, and in each cell are the terms that other researchers have used to describe each intervention. Table 1 also reports the mean effect size for each of the treatments based on the current meta-analysis. It is important to note that none of the earlier reviews included all of the treatments that we found in the literature, but collectively the earlier reviews provide a list as complete as the one that we developed. All studies that otherwise met our inclusion criteria (presented below in the Method section) utilized interventions from the list presented in Table 1. (Below in the Results section, we present our findings on the types of interventions that were found in the literature, including examples of research that used each type of intervention.)

Quantitative reviews. Only two previously published reviews have attempted to quantify the effectiveness of the various treatments for promoting PEB. Hines et al. (1987) quantitatively reviewed 128 studies on PEB using either the correlational or experimental approach. The majority of their report focused on correlational studies, and they devoted only one paragraph and one table to the analysis of the experimental studies. Unfortunately, they provided few details of exactly how they arrived at their results. They did not provide a list of the studies that were analyzed nor did they describe how the outcome statistics were computed, but they did note that the majority of the studies dealt with energy consumption and littering (littering has been excluded from the current meta-analysis, and the rationale for this exclusion is in the

Table 1. Types of Interventions and Weighted Average Effect Sizes (When Reported) as Identified by This Study and Other Researchers

Review	Easy	Prompts	Justifications	Instructions	Feedback	Rewards	Social modeling	Cognitive dissonance	Commitment	Goals	Other treatments
Current	$N = 19$ $g = 0.49$	$N = 44$ $g = 0.62$	$N = 44$ $g = 0.43$	$N = 50$ $g = 0.31$	$N = 60$ $g = 0.31$	$N = 36$ $g = 0.46$	$N = 26$ $g = 0.63$	$N = 13$ $g = 0.93$	$N = 32$ $g = 0.40$	$N = 15$ $g = 0.69$	
Hines et al. (1987)			Appeals, $g = 1.96$	Information, $g = 1.06$	Feedback, $g = 0.58$	Incentives, $g = 1.90$					
Hornik, Cherian, Madansky, and Narayana (1995)	Proximity, frequency, distribution, $g = 0.38, 0.60, 0.34$	Prompts, raffles, contests, $g = 0.35$	Ecological concern, $g = 0.70$	Knowledge, $g = 1.28$		Monetary incentives, $g = 0.68$	Perceived social influence, $g = 0.95$		Commitment, $g = 0.95$		Locus of control, $g = 0.63$, personal satisfaction, $g = 0.80$
Dwyer, Leeming, Cobern, Porter, and Jackson (1993)	Environmental alteration		Written activator	Written activator	Feedback	Rewards, penalties	Demonstration, oral activator, "most effective"		Commitment, "most effective"	Goals, "most effective"	
De Young (1993)		Prompting	Declarative knowledge	Procedural information	Feedback	Material incentives	Modeling, social support/ pressure		Commitment		
Kazdin (2009)			Education, knowledge, information		Feedback or knowledge of results	Incentives, disincentives	Use of the media, social marketing				Message framing, decisions making, religion and ethics, special contexts
Steg and Vlek (2009)			Knowledge	Persuasion			Social support, role models	Persuasion	Commitment		Implementation intentions
Lehman and Geller (2004)	Environmental design	Prompting	Information, education		Feedback	Rewards	Modeling		Commitment		

(continued)

Table 1. (continued)

Review	Easy	Prompts	Justifications	Instructions	Feedback	Rewards	Social modeling	Cognitive dissonance	Commitment	Goals	Other treatments
Porter, Leeming, and Dwyer (1995)	Environmental alteration, "increase recycling"	Prompts, "increase behavior"			Feedback	Rewards, penalties			Commitment, "increased recycling"	Goal setting, "promising technique"	
Schultz, Oskamp, and Mainieri (1995)	Remove barriers, "increase behavior"	Prompts, "increase behavior"			Feedback	Rewards, "effects short lived"	Normative influence, "increase behavior"		Commitment, "increase behavior"	Goal setting, "increase behavior"	
Cook and Berrenberg (1981)	Making alternatives available		Persuasive communication	Procedural knowledge	Feedback	Incentives, disincentives	Social incentives, disincentives, and Modeling	Attitude-behavior consistency			
Abrahamse, Steg, Vlek, and Rothengatter (2005)			Information	Information	Feedback, "has merit"	Rewards, "effective but short lived"	Modeling		Commitment	Goal setting	

Note: The first row of this table summarizes the results of this meta-analysis (it is not a summary of the other studies). *N* indicates total number of comparisons that used each treatment as a primary factor; most studies confounded multiple treatments and, thus, there is some double counting. *g* is the weighted mean effect size (weighted by the inverse of the variance of the estimate of the effect size).

Method section). The correlations that Hines et al. reported have been converted to standardized mean differences and reported in Table 1. Because we do not know either the sample or the method that was used in this study, it is difficult to interpret these results and compare them to our own.

Hornik, Cherian, Madansky, and Narayana (1995) synthesized the literature that focused on just recycling behavior; they reported that they used 67 empirical studies to derive these results, but they did not publish the list of studies. They classified the interventions into 4 types (intrinsic incentives, extrinsic incentives, internal facilitators, and external facilitators), and within these 4 categories, they found a total of 11 specific types of interventions. For each type of intervention, they computed a sample weighted mean correlation. These mean correlations were converted to standardized mean differences and are reported in Table 1.

Narrative reviews on the general PEB literature. Many other researchers have attempted to make sense of the diverse body of the PEB literature by doing narrative reviews. In some cases, these reviews offer qualitative conclusions about which interventions seem most effective. See Table 1 for summaries of these reviews. Dwyer, Leeming, Cobern, Porter, and Jackson (1993) reviewed the experimental research on interventions to promote PEB. They reviewed 54 studies that used eight different types of interventions to foster PEB. In comparing these intervention types, Dwyer et al. concluded that "commitment, demonstration, and goal-setting strategies were generally most effective in encouraging environmentally responsible behavior" (p. 275), but they did not provide the data necessary to quantify the degree of effectiveness of each kind of intervention strategy. Dwyer et al.'s is the most recent project that is similar in scope to the current meta-analysis. Of the 54 published studies they reviewed, we excluded 15 of them because they either pertained to littering or did not contain sufficient data to compute effect sizes.

Although De Young (1993) did not set out to comprehensively analyze the PEB literature, he used a 3 × 2 theoretical framework to develop a list of possible behavior change techniques. He posited that there are two sources of change (either internal or environment/others) and three broad categories of interventions (information, positive motivation, and coercion), and in this framework, he located 19 possible types of treatments and 10 of those map directly onto the treatments reviewed in this meta-analysis. Nine of the treatments come from what he called internal source of change, and as such, they are difficult to directly manipulate in an intervention (e.g., personal insight, feelings of remorse, intrinsic satisfaction). The purpose of De Young's work was to identify the psychological factors around long-term behavior change, and this important topic is discussed further in the future research section below.

Several recent articles have also reviewed the field. Kazdin (2009) reviewed a set of treatments for promoting PEB. He acknowledged that his list is more illustrative than comprehensive, and he did not attempt to form quantitative comparisons between the interventions. Steg and Vlek (2009) reviewed studies relevant to two broad categories of treatments—informational strategies and structural strategies—but they did not attempt to be comprehensive or quantitative. Below, we build on these two strategies and add two additional categories of social-psychological processes and monitoring. Lehman and Geller (2004) divided the treatments into two categories—antecedent and consequent strategies—but again, they did not attempt to do a comparative or quantitative review. Finally, the American Psychological Association (APA) task force (Swim et al., 2010) produced a comprehensive analysis of the psychological factors involved in the issue of climate change. Again, their narrative review contains a wealth of information, but it did not attempt to systematically analyze the treatments used to promote PEB.

Reviews on recycling. Research on recycling constitutes a large part of the literature on PEB. Three comprehensive reviews on studies that used recycling as a dependent variable were published in 1995. First, the work by Hornik et al. (1995) was mentioned above. Second, Porter, Leeming, and Dwyer (1995) analyzed 27 empirical articles. They reported effectiveness of the treatments, but only in the metric of the dependent variable (typically participation rates and pounds of recycled materials), and as such, these values are difficult to compare to our results. They acknowledged the difficulty in comparing treatments but noted that verbal prompts by block leaders are more effective than written prompts, written commitments are more effective than verbal commitments, feedback is effective but not as effective as goal setting, and rewards such as lotteries were more effective than immediate, smaller, or group rewards.

Third, Schultz, Oskamp, and Mainieri (1995) analyzed 31 published studies, identified 7 types of interventions, and concluded that all of these interventions produce significant increases in recycling behavior. They listed all of the studies they reviewed, but of the 72 treatments in the 31 studies, only 32 treatments reported sufficient data to compute effect sizes, and they did not synthesize the results for comparative purposes.

An important variable in recycling that is left unanswered by these reviews is the nature of the behavior. There are three different kinds of recycling behaviors that have been examined in the literature: curbside recycling where materials are accumulated at home for a period of time and then left at the curb for collection, central location recycling where materials are accumulated at home and then taken to a central location for processing, and public recycling where there is no accumulation of materials at all, rather, the individual makes an

instantaneous decision to put recyclable material, such as beverage containers, Styrofoam plates, or newspapers, in a recycling bin versus depositing them in the trash or leaving them as litter. To date, no study has attempted to analyze the differences in these three types of behaviors.

Reviews of energy conservation. Like recycling, energy conservation is an important subset of PEB. Several narrative reviews have been published on research done in the 1970s and 1980s (Aronson & Gonzales, 1990; Farhar & Fitzpatrick, 1989; Kempton, Darley, & Stern, 1992; McClelland & Canter, 1981; McDougall, Claxton, Ritchie, & Anderson, 1981; Ritchie & McDougall, 1985; Samuelson, 1990; Seligman, 1985; Stern, 1992; Winkler & Winett, 1982). Cook and Berrenberg (1981) published one of the most comprehensive lists of treatments for encouraging energy conservation, and although they provided many examples of research on these treatments, they did not attempt to evaluate them comparatively.

More recently, Abrahamse, Steg, Vlek, and Rothengatter (2005) reviewed 38 published studies of household energy conservation. Of the 73 comparisons they found in the literature, they were able to compute standardized mean differences for only 11 of them and therefore did not do a quantitative meta-analysis. In terms of the effectiveness of the treatments, Abrahamse et al. summarized their results by noting that information does not necessarily lead to behavior change, rewards are effective but short-lived, and feedback is effective, particularly when given frequently.

Of the 38 published studies included in Abrahamse et al., we excluded 7 studies because they were published prior to 1980, and we excluded an additional 10 studies because they either did not use an observed real-world behavior (rather, they relied on self-reports of behavior) or we could not compute the effect size from the data provided, and we excluded 1 study because we could not obtain it in English. We were able to find ways to compute effect sizes for the remaining 20 studies, and they are included in this meta-analysis. It is also important to note that Abrahamse et al. focused exclusively on household energy conservation. While this is an important place to start, people can also save energy by their behaviors in public places (e.g., turning off lights when leaving offices). An important detail that is unaddressed in the literature is the differences between household energy conservation and public energy conservation.

Recall that our goal is to provide practitioners with useful information about how to promote PEB. There are several reasons that the reviews discussed above might not be helpful to a practitioner. First, most of the reviews were not able to form quantitative conclusions or otherwise create meaningful comparisons of the effectiveness of the treatments. Applied psychologists looking to

do work in this important area currently have no definitive source for comparing the relative effectiveness of the interventions. Furthermore, very little quantitative work has been published since 1995. Second, several of the reviews did not provide the list of studies that were examined and did not define the treatments that were found. Third, the reviews on particular behaviors (recycling and energy conservation) left unanswered questions about subtle differences in types of PEB (curbside vs. central location vs. public recycling, and household vs. public energy conservation). The current meta-analysis aims to build on this earlier research and provide some guidance for practitioners who are interested in designing programs that foster PEB.

Research Questions of the Meta-Analysis

For this project, we sought to bring the environmental research in psychology up to date with a comprehensive meta-analysis. The quantitative comparisons derived from meta-analysis provide much more useful information than typical narrative reviews because they can turn vague terms like *more effective than* into actual measurable and quantifiable differences. The goal of this project is to provide researchers, practitioners, and environmentalists with the information necessary to design effective interventions for addressing environmental problems. To this end, we aim to answer three questions. First, what treatments have been tested? Second, what is the relative average efficacy of each treatment? Third, are certain treatments more effective for promoting certain types of behaviors?

Method

Meta-analyses typically involve three major steps (Cooper, 2010). First, conduct an extensive literature search to find all of the relevant studies that meet the specified inclusion criteria. Second, evaluate and code the studies for types of treatment and calculate effect sizes. Third, statistically analyze the coded features. These three steps form the basic framework to describe our research method.

Literature Search

Inclusion criteria. The focus of this meta-analysis is on the experiments that have been done to increase PEB. To be included in this meta-analysis, each experiment must have met four criteria. First, it must have been an experiment that made a comparison between groups (treatment vs. control) or made a

comparison across time (pretest–posttest, baseline-treatment, or ABA design). Many of these experiments used a compound design that included both a between-groups and across-time comparison.

Second, the outcome variable must have been a measured or observed behavior in a real-world setting. Research that relied on self-report, behavioral intentions, or staged laboratory behaviors was excluded (e.g., prisoner's dilemma, simulation games, and discrete choice scenarios). Furthermore, the behavior must have been relevant to environmental concerns, and most of the behaviors concerned increasing recycling, conserving energy, conserving water, and making efficient transportation choices. The particulars of these types of behaviors are discussed in greater detail below.

Third, the report must have provided quantitative information that allowed the effect size to be computed. The effect size could be computed directly from sample sizes, means, and standard deviations or estimated from statistical tests or proportions. Below, the details of how effect sizes were computed are presented.

Fourth, the report must have been published after 1980. This date is somewhat arbitrary, but studies published in the last 30 years are the most useful to practitioners designing programs going forward from here. Furthermore, many of these older studies did not report sufficient data for calculating effect sizes or provide enough details about the treatments.

Exclusions. Three specific types of research were excluded. First, research that focused on reducing littering was excluded. Littering is not conceptually similar to PEB because it is already illegal in many places, it is discouraged by social norms, and the underlying concern—although perhaps not the manifest consequences—is more focused on aesthetics than the environment. Slogans like "Keep America beautiful" and "Don't mess with Texas" do not carry an environmental connotation as much as they carry an aesthetic one.

Second, research on environmental education through the formal school system was excluded. Much research has been done on environmental education, particularly in primary school (for reviews, see Boerschig & De Young, 1993; Leeming, Dwyer, Porter, & Cobern, 1993; Zelezny, 1999). The nature of these interventions—where students were captive audiences, where the teachers had some degree of control and authority over the students, where students were not typically independent agents of their own behavior in the same way that college students and adults were, and where the interventions were typically over a much longer period of time (a unit on energy conservation may last a week or two in science class)—was too dissimilar from other research to include in this report.

Third, experimental interventions that are well outside of the abilities and resources of typical applied researchers and practitioners were excluded. The primary purpose of this project was to provide practitioners with useful information about techniques that they can use to promote PEB, so we chose to focus on practical, reasonable interventions. Most practitioners do not have the resources to engage in large-scale interventions that government administrators and corporate executives can. Government administrators can create programs like "Cash for clunkers" and daylight savings time (California Energy Commission, 2001; Kellogg & Wolff, 2008), and corporate executives of utility companies can offer rebates of hundreds or thousands of dollars per household for energy-conserving technological upgrades, but the average practitioner operates with much fewer resources.

Literature search process. To collect the published literature, we used three techniques. The first technique was a keyword search in PsycInfo, ABI/Inform, EconLit, ERIC, and Sociofile using the terms *ecological behavior*, *conservation and environment*, *environmental problems*, *energy conservation*, and *recycling*. The second technique was to search the reference lists of articles found using the databases. The third technique, used to collect the unpublished literature, was to post notes on two major discussion boards related to environment and psychology and to send personal emails to the authors who frequently appeared in our list of articles. Over the course of 3 years, we estimate that we communicated with approximately 30 leading researchers in this field. Although these communications generated a large amount of interest in this study, no researchers provided unpublished studies that ultimately met our criteria for being included in the study. These efforts yielded a more comprehensive list of studies than had been reviewed to date, although it is possible that studies using different keywords or not included in past reviews have been omitted from the current meta-analysis.

A liberal inclusion criterion was used for the initial phase, and any articles that were judged to be possibly relevant were obtained. In total, 292 articles reporting experiments, 170 articles reporting surveys, and 130 nonempirical articles (review or theoretical) that were relevant to the research questions were located. The obtained articles were then further analyzed to confirm that they met the criteria for inclusion. A total of 87 reports published between 1980 and 2009 met the criteria, and they had a total of 253 treatments for which effect sizes could be computed.

Literature Evaluation: Treatments and Effect Sizes

Coding treatments. Any one study could report several treatments because two or more treatments were often compared with a control group. For every

treatment reported in each study, two coders independently determined the nature of the intervention. Conceptually, the coders had to identify what kind of treatment the study employed. The intellectual task was to determine the differences between, on one hand, the control group or the pretest or baseline period and, on the other hand, the treatment condition or what happened after the treatment was administered.

All differences between the control group/pretest and treatment group/posttest were considered to be the intervention. Very few interventions in the studies employed one single identifiable treatment from our master list of 10 treatments. Rather, most studies employed several treatments, and thus, the treatments were confounded together. For example, the experimental group might have received both rewards for recycling and feedback about how much they recycled, whereas the control group received neither of these things. Or the experimental group might have received both instructions on how to recycle and prompts for when to recycle, whereas the control group received nothing. There is no way to determine the separate effects of the instructions versus the prompts. These confounded interventions were present in 78% of the studies, presumably because researchers combined several treatments together to maximize the potential effect on behavior.

To account for this confounding of treatments, the coding system used a 4-point scale such that a value of 3 indicated the treatment was of primary importance and a central focus of the intervention, a 2 indicated it was of secondary importance, a 1 indicated that it was a minor factor but still a difference from the control condition, and a 0 indicated it was not a factor. Primary factors were easily identifiable from the content of the report; they were the major factors that the researchers were testing. Secondary and minor factors required the coders to make a judgment about how important these treatments were. Thus, the coding system was somewhat subjective in that the two coders had to evaluate the difference between primary, secondary, and minor factors. To minimize the effects of this subjectivity, the two coders' independent results were compared, and the differences were discussed and resolved with the assistance of a third person who was experienced in meta-analyses. These confounded treatment studies cause some problems in the statistical analyses but offer a unique strength, as discussed below.

Computing effect sizes. For the results of each treatment, the standardized mean difference was computed. Technically, Hedge's g was computed, which is mathematically very similar to the standardized mean difference d with a slight correction to make it unbiased (Rosenthal, 1994, Equation 16-28) and, therefore, a little more conservative than d. The difference between these two statistics is negligible for our purposes. For an experiment with 50 participants in each of the control and treatment groups, if $d = 0.400$, then $g = 0.396$.

In many cases, the effect size could be computed directly from the means, standard deviations, and sample sizes provided in the article. In some cases, D-Stat software (Johnson, 1989) was used to compute the effect size from counts/frequencies, *t, F, r,* and *p* statistics. In cases where nonsignificant results were reported without any supporting statistics, the effect size was estimated as 0.00.

The standard research design for computing *g* is the two-group, posttest-only design, which is also called the treatment–control design. This sample had 104 treatments that employed this design, and the computation of the effect size was relatively straightforward in these cases. However, *g* can also be computed for single group, pretest–posttest designs, of which there were 75 treatments. The most sophisticated type of design combines these two designs into the treatment–control pretest–posttest design, and there were 74 treatments that used this type of design. For these designs, we computed the pre–post effect size for both the treatment group and the control group, and then subtracted the control group effect size from the treatment group (Morris & DeShon, 2002). By doing so, we corrected for whatever effect time had on the treatment. (The potential problems caused by analyzing these three types of designs together are discussed in the limitations section of the Discussion.)

After the effect sizes for all treatments were computed, seven values were found to be outside of the range of +2.50 standard deviation units or greater than a raw value of 2.88 (Bachman & Katzev, 1982; Iyer & Kashyap, 2007; Jacobs, Bailey, & Crews, 1984; Van Houten, Nau, & Marini, 1980; Van Houten, Nau, & Merrigan, 1981; Werner, Byerly, White, & Kieffer, 2004; Werner, Stoll, Birch, & White, 2002). If the data were normally distributed, only two or three values should have been greater than this value. To reduce the effect of these outliers, their values were changed to be equal to a raw score of 2.88.

Data Analysis and Interpretation

As is typical in meta-analyses, we formed a data set in which the types of intervention and types of behaviors constituted the independent variables and the effect size was the dependent variable. Effect sizes can be combined or averaged across studies by weighting each study by the inverse of the variance of the effect size's estimate (which is closely related to the sample size) and averaging together to form the weighted average effect size (Cooper, 2010). We computed the average effect size for each type of treatment and then directly compared the average effect sizes to see which treatments were most effective.

The standardized mean difference effect size can be interpreted in two conceptually different ways. First, the standardized mean difference is the difference

between the treatment group and the control group expressed in standard deviation units. An effect size of 0.00 means that there were no differences between the group means. An effect size of 0.20 means that the treatment group scored 0.20 standard deviation units higher than the control group. An effect size of 0.50 means that the treatment group scored 0.50 standard deviation units higher than the control group. The general guidelines for interpreting effect sizes of 0.20, 0.50, and 0.80 are small, medium, and large, respectively (Cohen, 1988), but Cohen and others (e.g., Cooper, 2010) have strongly cautioned against using these qualitative terms for interpreting results in particular topic areas.

Second, effect sizes indicate how much more behavior people in the treatment group performed compared with the average person in the control group; this information can be derived from the standard normal distribution table, and it is called the U_3 statistic (Cohen, 1988). An effect size of 0.00 means that 50% of the people in the treatment group performed more behavior than the average person in the control group. Said another way, this means that 0% of the people performed more behavior because of the treatment or the treatment had no effect. An effect size of 0.20 means that 58% of the people in the treatment group performed more behavior than the average person in the control group or that 8% of the people performed more behavior than they would have been expected to without the treatment. An effect size of 0.50 means that 69% of the people in the treatment group performed more behavior than the average person in the control group or 19% of the people performed more behavior as a result of the treatment. An effect size of 0.80 means that 79% of the people in the treatment group performed more behavior than the average person in the control group or that 29% of the people performed more behavior. Statistical Package for the Social Sciences (SPSS) was used for all meta-analytic computations.

Results

Question 1: Types of Treatments

All studies included in the meta-analysis are listed in the reference section with an asterisk next to them, and following each citation, the list of treatments used in that study are indicated. We found 10 types of treatments in the literature, developed definitions for them, and found representative examples of how the treatments were employed in the studies. These 10 types of treatments could be grouped into four larger sets: convenience, information, monitoring, and social-psychological processes. There is no underlying theoretical

basis for these categories; we only present them as a conceptual way of organizing the list of 10 treatments. Other organizational systems may be equally valid.

The set of treatments that centered around convenience includes the treatments of making it easy and providing prompts. *Making it easy,* or changing situational conditions, involved making behaviors easier to do, such as moving recycling bins to a more convenient location or providing low-flow shower heads to conserve water (e.g., Brothers, Krantz, & McClannahan, 1994; Ludwig, Gray, & Rowell, 1998; Van Houten et al., 1981). *Prompts* were noninformational reminders that focused only on when to perform the next specific action, like "turn off lights when leaving room" or "put recyclables out tomorrow" (e.g., Austin, Hatfield, Grindle, & Bailey, 1993; Krendl, Olson, & Burke, 1992; Luyben, 1980a).

The set of treatments that focused on information included two types of treatments. *Justifications* were the reasons for performing a specific behavior (also called declarative information or why-to information), such as information about how much material is dumped in landfills that could be recycled (e.g., Burn & Oskamp, 1986; Smith & Bennett, 1992; Thompson & Stoutemyer, 1991). In contrast, *instructions* indicated how to perform a specific behavior (also called procedural information), such as use the blinds to reflect the sun to keep the office cooler or sort plastics according to their codes (e.g., Staats, van Leeuwen, & Wit, 2000; Werner, Rhodes, & Partain, 1998).

The set of treatments that focused on monitoring included two types of treatments. *Feedback* provided information about the extent to which a behavior has been performed by participants in an earlier time frame. Typically, feedback treatments were done over a course of time with distinct periods, like monthly electricity billing, such that the participants learned how they did last month and accordingly adjusted their behavior in the coming month (e.g., Brandon & Lewis, 1999; DeLeon & Fuqua, 1995; Katzev & Mishima, 1992; Sexton, Johnson, & Konakayama, 1987; Siero, Bakker, Dekker, & van den Burg, 1996). *Rewards* or incentives were any kind of monetary gain that people received as a result of participating in the experiment. These gains included not only cash but also coupons, rebates, bus passes, gifts, prizes, and so on (e.g., Diamond & Loewy, 1991; Hake & Zane, 1981; Jacobs, Fairbanks, Poche, & Bailey, 1982; Slavin, Wodarski, & Blackburn, 1981).

The set of treatments that focused on social-psychological processes included four types of treatments. All of these treatments somehow involve people interacting. Typically, this interaction is initiated by someone who wants to encourage someone else to engage in PEB. *Social modeling* included any kind of passing of information via demonstration or discussion in which the initiators indicate that they personally engage in the behavior, also. In psychology,

terms to describe this passing of information include modeling, norms, or diffusion (e.g., Burn, 1991; Cobern, Porter, Leeming, & Dwyer, 1995; Hopper & Nielsen, 1991; Winett, Leckliter, Chinn, Stahl, & Love, 1985). Treatments that utilized *cognitive dissonance* accessed preexisting beliefs or attitudes and attempted to make participants behave in ways that were consistent with those beliefs to reduce the dissonance. These treatments also included "foot in the door" treatments where experimenters asked participants to engage in a small act first and subsequently asked them to engage in a larger act. Conceptually, foot-in-the-door and cognitive dissonance were closely related (e.g., Aitken, McMahon, Wearing, & Finlayson, 1994; Dickerson, Thibodeau, Aronson, & Miller, 1992; Katzev & Johnson, 1984). *Commitment* was operationalized as asking participants to make some sort of verbal or written commitment to engage in a behavior, most frequently by signing a pledge card (e.g., DeLeon & Fuqua, 1995; McCaul & Kopp, 1982; Werner et al., 1995). *Setting goals* was the process of asking participants to aim for a predetermined goal, like reducing their electricity consumption by 20% (Bamberg, 2002; Hamad, Bettinger, Cooper, & Semb, 1980-1981; Katzev & Johnson, 1984; Van Houwelingen & Van Raaij, 1989).

Table 1 lists all 10 treatments and includes the number of times each treatment was the primary focus of a study. There is some double counting here because researchers often combined two treatments either in factorial designs or as a confound (combining treatments to increase an intervention's impact). To further examine which treatments were used in combination and the effectiveness of these combinations, Table 2 is a matrix that lists all 10 treatments as both the columns and rows, and it includes the number of times each treatment was included as a primary or secondary treatment in a study (coded as a 2 or 3 on our 4-point scale); these data are found along the diagonal of the table and are in boldface. Table 2 also shows the cross-tabulation of the number of times the treatments were combined as primary and/or secondary treatments in the studies; these data are found in the off-diagonal cells and are in italics. The remaining data in Table 2 are the average effect sizes for each combination of treatments (normal font), which are discussed in a later section.

Question 2: Treatment Type as a Moderator

Descriptive statistics and combining effect sizes across studies. The set of 253 effect sizes is presented in Figure 1 as a histogram (seven outliers were adjusted to a raw value of 2.88). The unweighted mean (and *SD*) of this set of effect sizes is 0.64 (0.74). When weighted by the inverse of the variance of each effect size, the average effect size in the data set is 0.45, with a 95%

Table 2. Cross-Tabulation of Treatments That Occurred Primarily or Secondarily, Showing Weighted Average Effects Size (First Value in Each Cell) and Number of Comparisons (Listed in Parentheses)

Treatment	1	2	3	4	5	6	7	8	9	10
1. Easy	0.46 (**35**)									
2. Prompts	1.10 (*21*)	0.81 (**57**)								
3. Justifications	0.03 (*8*)	0.78 (*9*)	0.37 (**85**)							
4. Instructions	0.09 (*11*)	0.51 (*12*)	0.44 (*44*)	0.31 (**94**)						
5. Feedback	0.28 (2)	0.63 (*4*)	0.32 (*14*)	0.28 (*20*)	0.48 (**64**)					
6. Rewards	1.39 (2)	0.28 (*1*)	0.18 (*9*)	0.47 (*14*)	0.52 (*10*)	0.37 (**44**)				
7. Social modeling	–0.06 (*3*)	0.35 (*5*)	0.43 (*24*)	0.48 (*20*)	0.48 (*13*)	0.39 (*6*)	0.44 (**56**)			
8. Cognitive dissonance	(*0*)	1.74 (*1*)	0.89 (*9*)	1.23 (*3*)	0.41 (*1*)	0.33 (*1*)	0.83 (*5*)	0.89 (**16**)		
9. Commitment	–0.04 (*4*)	(*0*)	0.24 (*14*)	0.36 (*15*)	0.66 (*6*)	1.23 (*3*)	0.26 (*14*)	0.56 (*5*)	0.37 (**39**)	
10. Goal setting	(*0*)	(*0*)	0.39 (*3*)	1.31 (*7*)	0.34 (*9*)	0.78 (*8*)	0.58 (*4*)	0.27 (2)	0.71 (*12*)	0.51 (**25**)

Note: Each treatment was coded for its use in each study using a 4-point scale. Since most treatments were confounded, this table shows the frequency and effect of confounding treatments. Values on the diagonal (in boldface) are for all studies in which the treatment was either a primary or secondary treatment. Values off the diagonal (in italics) are for all studies which combined two treatments as either primary and/or secondary. Values in parentheses are the number of comparisons that were combined for each weighted average effect size.

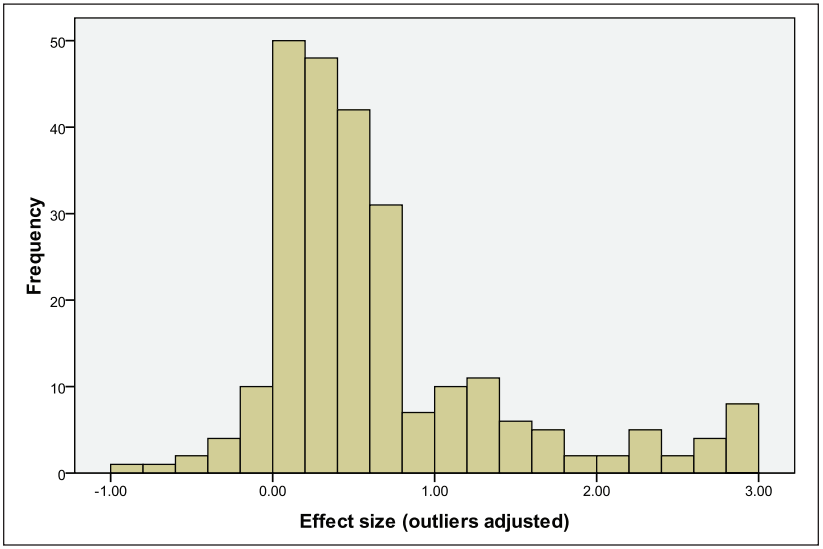

Figure 1. Histogram of effect sizes
Note: Seven effect sizes were found to be greater than +2.50 *SD* units. These values were adjusted to be equal to that value ($g = 2.88$).

confidence interval of [0.43, 0.47]. The weighted average effect size was also computed for each treatment (when it was evaluated as a primary treatment of the study) and is shown in Table 1. There is some double counting here because of the confounding of treatments. Earlier we mentioned that an effect size of 0.50 means that 69% of people in the treatment group performed more behavior than the average person in the control group. An effect size of 0.45 means that 67% of people in the treatment group performed more behavior than the average person in the control group.

Moderator analysis. Following the procedure described in Cooper (2010), we tested the homogeneity of the entire set of effect sizes, and the set of effect sizes was determined to be heterogeneous, Q_{total} ($df = 252$) = 2995, $p < .001$. Given this heterogeneity, it was appropriate to test for moderators.

The first moderator that we were interested in was type of treatment. Inspecting the weighted average effect sizes shown in Table 1, there were three groups of effectiveness. The most effective treatments were cognitive dissonance, setting goals, social modeling, and prompts, all with weighted average effect sizes greater than 0.60. The middle group included making it easy, rewards, justifications, and commitment, all with effect sizes in the 0.40's.

Finally, two manipulations had much lower effect sizes, instructions and feedback.

We would like to have been able to statistically test—rather than just visually inspect—whether the different treatments produced different effect sizes. The research on PEB did not permit a statistical test because of the confounded treatments, which is an important limitation in this data set. The procedure for computing, if groupings within a set of effect sizes were different, required that each effect size could be uniquely assigned to a group, much like a quasi-experimental research design. However, 78% of the studies confounded two or more treatments, and as such, it was not possible to assign logical, mutually exclusive groupings. For example, would a study that uses both rewards and feedback be assigned to the rewards group or feedback group? The computations for partitioning the variance depended on forming mutually exclusive groupings, but the PEB data set did not permit this. The confounded nature of many of the treatments limits the statistical conclusions that can be drawn from the data.

However, the results still have practical value. For researchers or practitioners who are interested in doing similar studies fostering PEB, these results indicate what the expected effects should be. Table 2 shows which treatments were often paired together and which combinations were most effective according to their weighted average effect sizes. To interpret these data, we ignored cells with less than five effect sizes because we felt the estimates were not sufficiently well tested (i.e., Table 2, italicized values less than 5). We examined the remaining cells to find those with the largest effect sizes. The largest effect sizes are in the cells that combine prompts and making it easy, justifications and prompts, justifications and cognitive dissonance, and three cells that involve goal setting and rewards, instructions, and commitment.

Question 3: Efficacy of Treatments for Each Type of PEB

Following Cooper (2010), we determined that the variation in the entire set of effect sizes is too great to be because of sampling error alone, Q_{total} ($df = 252$) = 2,995, $p < .001$, and it was appropriate to search for moderators. A second moderator of interest was the type of PEB. Earlier, we introduced four major categories of PEB (recycling, conserving energy, conserving water, and conserving gasoline), but even within these categories, the variation in behaviors was too broad, and so we further subdivided the data set.

The moderator variable of the type of behavior can be used to divide the studies into mutually exclusive groupings (e.g., no studies that looked at recycling also looked at energy conservation). From these mutually exclusive

Table 3. Tests of Homogeneity in the Set of Effect Sizes When Grouped According to Type of Behavior

Type of behavior	*df*	*Q*	*p*
Recycling	110	2,130	<.001
Energy conservation	80	302	<.001
Water conservation	25	49	<.01
Gasoline conservation	14	128	<.001
Curbside recycling	51	392	<.001
Central location recycling	17	80	<.001
Public places recycling	40	1,038	<.001
Home-energy conservation	62	157	<.001
Adoption of new technology	1	1.50	ns
Public places energy conservation	13	79	<.001

Note: *Q* was computed following Cooper (2010). The *Q* statistic is distributed as a χ^2 statistic with $n - 1$ degrees of freedom.

groupings, a test of homogeneity of effect sizes can be performed, as illustrated in Cooper (2010). Table 3 summarizes these tests. We first divided the set of effects sizes according to the four major types of behavior, and these tests are the first four rows of Table 3. In all cases, there is more variance than would be expected due to sampling error alone, and so the search for additional moderators is appropriate. For recycling and energy conservation, we further subdivided the behaviors into more detailed types of behaviors, and again there was significant variation within these subdivisions, as shown in the last six rows of Table 3. Adoption of new technology as a form of energy conservation was only used in two studies, and this behavior was the only one which did not have significant variation.

The statistically significant variation within the groups of behaviors could have been anticipated because even within each group of behaviors, the treatments used to foster the behaviors varied. But the confounded nature of the treatments prevented further tests from being done. However, the data are still useful to practitioners.

The number of treatments and weighted average effect sizes for the nine behaviors are shown in the rightmost column of Table 4. This column can be thought of as the average effect size for each behavior. Furthermore, the average weighted effect size for each primary treatment's effect on each PEB was computed. These data are presented in the cells of Table 4. Although no

Table 4. Number of Comparisons and Weighted Average Effect Size for Each of the 12 Behaviors by Primary Treatment

Type of PEB	Easy	Prompts	Justifications	Instructions	Rewards	Social modeling	Cognitive dissonance	Feedback	Commitment	Setting goals	Total
Public recycling	8	19	3		2	2	4	3	3		41
	1.46	0.95	1.71		0.73	0.93	1.09	0.48	1.08		1.05
Public-energy conservation	2	5	1	3		1		4			14
	2.88	0.54	1.63	0.25		0.25		0.38			0.44
Water conservation	4	6	11	4	3	1	2	10	1		26
	0.31	0.56	0.21	0.18	0.20	1.15	0.52	0.18	0.41		0.39
Curbside recycling	3	8	14	12	4	9	2	3	13		52
	2.33	0.17	0.29	0.37	0.62	0.57	0.59	0.36	0.30		0.30
Central recycling				1	6	3		5	4	4	18
				2.88	0.61	0.28		0.26	0.14	0.50	0.49
Home energy conservation		3	9	25	6	4	5	32	7	3	63
		0.00	0.17	0.17	0.45	0.74	0.29	0.28	0.55	0.31	0.26
Home energy adoption	1		1								2
	0.01		0.14								0.03
Gasoline conservation	1			2	11			2	2	5	15
	2.24			2.23	0.43			2.04	2.23	1.31	0.47
Other behaviors		3	5	3	4	6		1	2	3	20
		0.38	0.47	0.66	0.23	0.34		1.54	0.54	0.46	0.32
Total	19	44	44	50	36	26	13	60	32	15	253
	0.49	0.62	0.43	0.31	0.46	0.63	0.94	0.31	0.40	0.64	0.45

Note: The upper value in each cell is the number of treatments that were used on each behavior, the lower value in each cell is the weighted average effect size. Treatments (columns) are arranged in order from low engagement to high engagement; behaviors (rows) are arranged in order from least effortful to most effortful.

statistical test can be done to determine which values are significantly greater than other values, the magnitudes of the effect sizes across each row provide a sense of the relative effectiveness of each treatment for promoting a particular PEB. Finally, some cells in Table 4 are blank. These are the combinations of treatments and PEB that have not yet been reported in the literature.

Discussion

Summary of Results

The goal of this project is to provide practitioners of the environmental movement with useful information regarding how to best promote PEB. The data set is rich with recommendations that practitioners can use to inspire people to engage in more PEB, even though sophisticated statistical tests could not be performed. Tables 1, 2, and 4 present slightly different perspectives on the literature. Table 1 presents the 10 treatments and the overall weighted average effect size for each treatment, and this table shows the most effective treatments overall are using cognitive dissonance, setting goals, using prompts, and using social modeling. Although previous reviews of the literature have attempted to evaluate the effectiveness of different treatments for promoting PEB, Table 1 represents the first time a complete and quantitative assessment of the various treatments has been assembled. Researchers doing similar experiments now have some comprehensive, general expectations of what results to expect.

Because most researchers have used multiple treatments in their studies, we cross-tabulated all of the treatments and found that six combinations are particularly effective, as shown in Table 2: rewards and goals, instructions and goals, commitment and goals, prompts and making it easy, prompts and justifications, and dissonance and justifications. No prior research has attempted to systematically document the synergistic effects of multiple treatments. It is interesting to note that three of these six combinations involve goal setting even though goal setting is not among the set of four most effective treatments shown in Table 1. Certainly, psychology has a rich research history on the effectiveness of goal setting (e.g., Locke & Latham, 2002), and recent work has looked at how various kinds of goals influence environmental behavior (Lindenberg & Steg, 2007).

Tables 1 and 2 present slightly different results. Recall that we used a 4-point coding system to rate the treatments as of primary, secondary, or tertiary importance, or not included. Table 1 shows the average weighted mean effect size for each treatment when it was the primary treatment. In contrast,

the values along the diagonal in Table 2 are the average weighted mean effect sizes for each treatment when it was rated as either the primary or secondary treatment used in each study.

Finally, Table 4 presents the treatments cross-tabulated with the behaviors. The most important conclusion from these data—and a unique contribution to the literature—is that there is no one treatment (a "silver bullet") that is highly effective across all the possible PEB. Certain treatments seem to be more effective for certain behaviors. For example, treatments that are effective for promoting home energy conservation are social modeling and commitment. In contrast, treatments that are effective for promoting curbside recycling are making it easy and rewards. To effectively promote certain PEB, practitioners need to match the treatment to the behavior.

This situation is probably due to important differences in the types of behavior; certainly, there are substantial differences in the forethought and effort required to perform the different behaviors. With these differences in mind, an interesting pattern emerges in the data. We did not expect to find this pattern, so we advanced no a priori hypotheses nor is there sufficient data to statistically test the model, but it is worth mentioning. Some treatments engage the participants at a minimal level (e.g., prompts, procedural information, justifications), whereas other treatments require participants to make a much more engaged effort (e.g., goal setting, commitment). We ranked the level of engagement of the treatments in this order from least to most: making it easy, prompts, justifications, instructions, rewards, social modeling, cognitive dissonance, feedback, commitment, and goal setting. Similarly, the behaviors can be arranged along a qualitative scale of forethought and effort. We ranked the behaviors from least to most effortful: public recycling, public energy conservation, water conservation, curbside recycling, central recycling, home energy conservation, home energy adoption, and gasoline conservation. The columns and rows in Table 4 are arranged to reflect these rankings. The pattern that emerges is that the largest effect sizes generally appear along the diagonal, meaning that low-engagement treatments are appropriate for low-effort behaviors and that high-engagement treatments are effective for high-effort behaviors.

Recommendations for Practitioners

Based on all these data, our recommendation to practitioners designing programs to foster PEB is to first select the type of behavior to target, then use Table 4 to determine which interventions are likely to be particularly effective. Table 2 can then be used to select complementary treatments that are consistent

with the situation or goals of the program, keeping in mind our rough guideline that more involved treatments are needed to foster more effortful behaviors. For example, practitioners aiming to promote home energy conservation can use Table 4 to note that social modeling is an effective treatment for the behavior, and then use Table 2 to note that goal setting and feedback treatments work well in combination with social modeling.

Computing the effectiveness of a PEB treatment has another layer of complexity. Some PEB are easier to perform than others, and some PEB have greater environmental benefit than others. It is good that people recycle aluminum beverage cans as they leave a lecture hall, and this seems to be easy behavior to foster. However, it is much more important that people insulate their attics, install high-efficiency appliances, and drive fuel-efficient cars, and these behaviors are very hard to promote. The effectiveness of a program to promote PEB should be measured in environmental impact, not just in amount of changed behavior. As such, there is no agreed upon way of assessing the environmental impact of the various behaviors—particularly when direct and indirect usages of energy and resources are being accounted for (see Abrahamse et al., 2005; Kok, Benders, & Moll, 2006)—but the concept of the ecological footprint is a good way to begin to quantitatively compare various behaviors (Wackernagel & Rees, 1996; although this method is not without its critics, see Fiala, 2008). The idea of the footprint is that the amount of productive land and sea area needed to support each behavior can be calculated and then compared. The carbon footprint is an analogous concept based on carbon emissions. Computing the environmental impact of various behaviors is beyond the scope of this research report, but a list of the 27 most effective actions people can do to address climate change has been developed (Gardner & Stern, 2008).

Is it worth going to the trouble to get people to do these simple things for the environment? Do things like using low-flow shower heads, changing driving habits, and unplugging devices that draw phantom power really make a difference in the outlook for the future? If everyone did all these things, would it make a difference? The answer is unequivocally yes. The interventions analyzed in this report and the behaviors they target have substantial environmental impact. Reasonably achievable emissions reductions—the technical term for what we can get people to reasonably do without large scale government regulation—from 17 everyday, household behaviors can save an estimated 123 million metric tons of carbon emissions per year or about 7% of U.S. national emissions (Dietz, Gardner, Gilligan, Stern, & Vandenbergh, 2009). Applied practitioners with only modest resources can, thus, have a very important and significant impact on our environmental situation.

Limitations of the Meta-Analysis

One of the major limitations of this meta-analysis was due to the large number of confounded treatments. The high frequency of confounded treatments is probably born from the fact that most interactions where one person is encouraging another person to do more of a new behavior would naturally utilize several treatments. It is virtually impossible to ask someone to engage in a new behavior without also telling them why to do it (justification for engaging in the behavior), how to do it (instructions or procedural information), and when to do it (provide prompts or reminders). Furthermore, when encouraging people to engage in new behavior, it is also reasonable to make the behavior convenient to perform (making it easy) and to provide some sort of demonstration (social modeling). Thus, the act of requesting someone to engage in a new behavior logically and reasonably includes several treatments, and the best results probably occur when multiple treatments are combined.

There is no way to separate out the effects of each treatment when they are combined or confounded. As mentioned previously, if a study used both rewards and feedback, it is not possible to determine the unique effects of either rewards or feedback. The high frequency of confounded treatments limited some statistical analyses, but it did permit us to explore which treatments are effective when paired together. There are several other limitations, also.

Missing studies and fail-safe N. Meta-analyses are only valid to the extent that they include all the experiments that meet the inclusion criteria. It is highly likely that some studies that should have been included were not located, quite possibly because they were never published because they showed nonsignificant results. How many nonsignificant findings would be required to nullify these results? This value is called the fail-safe N. The fail-safe N is just an algebraic weighting of all the known studies versus those that are unknown (Orwin, 1983). That is, to reduce our current overall finding from $g = 0.45$ to $g = 0.00$, there would have to be an equivalent 253 treatments that showed the exact opposite results (that the treatment groups engaged in *statistically significantly less* desired behavior than the control groups). Alternatively, to reduce the findings of $g = 0.45$ to a marginal value of $g = 0.05$, there would have to be more than 2,000 treatments from studies that had nonsignificant results. We have no way of knowing how many other valid studies have been done on this topic, but we feel it is unlikely that that there exists another whole library of studies with negative results or null results that have not found their way into the published literature. It is conceivable that there are thousands of unpublished studies on this topic, especially when you imagine all the projects

by students as well as seasoned researchers, but the likelihood that the vast majority of those studies have null results seems small.

Search for deeper moderators. At the outset of this project, we envisioned being able to do sophisticated tests for moderators. We started with the entire sample of studies on environmental behaviors, and we identified the moderator of type of treatment (Tables 1 and 2), and we identified the moderator of type of environmental behavior (Table 4). We also examined these two moderators together (Table 4).

But we also thought that we would be able to go deeper; we wanted to test for moderators that would influence the type of treatment. For example, we hypothesized that things like the size of the reward, the frequency of the feedback, the source of the information, and the nature of the goals being set would influence the amount of behavior produced. When we started looking at moderators, it quickly became clear that there was no objective way to measure these things.

For example, even something as apparently simple as rewards was very complex. Most studies that employed rewards did not use consistent cash payouts to participants; rather, they frequently used contests, lotteries, and various prizes. The values of these rewards were very difficult to compare. For example, how do vouchers at a health food store compare to half-priced bus tickets? How much value would college students place on the chance to win a pizza party compared to that which members of the general public would place on a small tree? Other types of interventions were even more challenging to comparatively evaluate. How much stronger is a letter from the university president than a letter from a faculty member? How much stronger is moving recycling bins into individual offices compared to posting signs above a central recycling bin?

Because we could not answer these questions objectively and reliably, we modified the coding system to use the 4-point scale. We evaluated each treatment and attempted to determine the primary, secondary, and tertiary treatments. This simple system allowed us to stay within the boundaries of what the literature showed.

Noncomparable effect sizes. The sample of studies included three primary types of designs (treatment–control, pretest–posttest, and combination treatment–control pretest–posttest) that involve slightly different approaches to computing the effect sizes. We followed the instructions provided by Morris and DeShon (2002) for computing these different effect sizes. However, directly comparing effect sizes from between-groups and within-groups designs requires a slight correction to the effect sizes based on the correlation

between the pretest and posttest scores. This formula is relatively easy to employ if the correlation between the pretest and posttest scores is provided. In the PEB literature, the correlation is not provided, and we had no way to estimate the correlation and correct for it.

Levels of analysis. Intuitively, one would assume that all studies analyzed in this report used people as the level of analysis, and studies that used larger samples should have greater weight in the aggregated findings. A study that used 100 people should count twice as much as a study that used 50 people. In this literature, it is not straightforward to count how many people are in each study. Some of the studies reported results using individual people as the level of analysis, and other studies reported results using households or families as the level of analysis. Still other studies reported results across time (e.g., how much recyclable paper was collected in the central bins in the copy room during the five baseline weeks and the five treatment weeks), and these studies make no mention of how many people participated in the research. We have formed the average weighted effect sizes based on the variance of the effect estimate, and as such, this variance is an estimate of the accuracy of the effect size based on the number of observations made. The variance is still a useful estimator of accuracy based on the number of observations made, and it is unaffected by the level of the observations, be they at the individual, household, or time levels of analysis.

Stochastically dependent effect sizes. Meta-analysis can be conceptualized and computed as a special case of multilevel modeling (Goldstein, Yang, Omar, Turner, & Thompson, 2000). Multilevel modeling allows the variance at different levels to be computed and accounted for. For this meta-analysis, we have used the treatment level as the highest level of analysis, and we have assumed that the effect sizes for the various treatments are independent of each other.

More sophisticated analyses are possible. Technically, the effect sizes from one study that compares two or more treatments to the same control group are not independent of each other; rather, they are said to be stochastically dependent. Whatever sampling or measurement error is associated with the control group effects the effect sizes for all the treatment groups that are compared with that effect size. Furthermore, studies done by the same authors are not truly independent, either. Even if the studies are separated in time, studies that use the same materials, measures, methodologies, and researchers are likely to share some variance. It could even be argued that studies that are done in the same states or countries are not truly independent of each other in that culture may have an effect on how much PEB participants perform. An alternative way of describing the nesting, ranging from the most conservative way

to create independent data to the least conservative way, is the levels of laboratories, studies, samples, and treatments (Cooper, 2010).

There are very sophisticated methods of correcting for the dependence among effect sizes because of shared control groups, research labs, and cultures. These methods estimate the amount of variance shared at the various levels based on the linear regression model (Gleser & Olkin, 2007, eqn. 2.5) and the variance–covariance matrix of the effect sizes (Gleser & Olkin, 2007, eqn. 2.7). These corrections are only valid if the effect sizes for each treatment are homogeneous over the studies, which are tested by computing the *Q* statistic (eqn 2.8). Our overall *Q* was statistically significant, which meant that there was significant heterogeneity in the entire sample of effect sizes, and thus, the regression model and the ensuing corrections may not be appropriate for this data set.

Areas for Future Research

One of the important outcomes of a meta-analysis is that it helps locate gaps in the literature that are in need of more research. These gaps are most evident in Table 4. In terms of treatments, some of the most effective treatments have been studied the least, namely, cognitive dissonance and goal setting. Both of these manipulations have a long history in psychology of being effective techniques for changing behaviors (e.g., Festinger, 1957; Locke & Latham, 2002), and our results confirm that they are effective within the domain of PEB. However, their usefulness has been underutilized by researchers.

Our list of 10 treatments is based on what we found in the literature, but it is quite reasonable to imagine that other treatments exist outside of those previously reported in the literature. Some treatments that have not been tested include creating competitions (Dwyer et al., 1993) and drawing on intrinsic motivations (De Young, 1996, 2000). Seven constructs that either directly or indirectly affect intentions to perform PEB have been identified (Bamberg & Möser, 2007). Theoretically, each one of these constructs could be the focus of a treatment. For some of these constructs, treatments have already been developed and tested (e.g., presenting information about rational reasons increases problem awareness, using social modeling helps people internalize social norms, and both instructions and feedback can enhance people's perceived behavioral control). However, some components of Bamberg and Möser's (2007) model have not had treatments designed and experimentally tested to address them (e.g., developing internal attributions and creating feelings of guilt). Thus, although we have developed a comprehensive list of

the treatments found in the literature, the literature does not report all of the treatments that are possible. Researchers who are looking for new approaches for encouraging PEB should investigate some of these untested treatments.

In terms of behaviors, recycling has been studied the most, and although it is an important PEB, conservation of energy, water, and gasoline are all also important. With regards to gasoline, there has been considerable international political and economic turmoil surrounding the control of oil supplies in the last 20 years, and about 70% of the U.S. petroleum consumption is used for transportation (Energy Information Administration, 2009). Yet there has been practically no experimental research on treatments that help people lower their dependence on gasoline. Experiments with dependent variables concerning carpooling, using public transportation, and riding bicycles are almost completely absent from the psychology literature. One possible explanation for this absence is that field experiments are more difficult to complete than either laboratory-based experiments or correlational research with self-reported behaviors, and as such, field experiments are not the favored technique of researchers. Many correlational studies on this topic have appeared in the literature recently (Bamberg & Schmidt, 2003; Eriksson, Garvill, & Nordlund, 2006, 2008; Gardner & Abraham, 2007; Gärling & Loukopoulos, 2008; Jansson, Marell, & Nordlund, 2010; Thøgersen & Møller, 2008).

One important issue that the PEB literature only minimally addresses is the distinction between short-term and long-term behavior change. The typical experiment included in this meta-analysis only monitored behavior for 2 to 8 weeks. Of course, some behavior change is better than none, and there has to be a starting point. But while behaviors have to change, they have to stay changed (De Young, 1993). De Young developed five evaluation dimensions to assess long-term effectiveness of behavior change interventions: reliability, speed of change, particularism, generality, and durability. As research in this area moves forward, much more work needs to be done on the long-term effectiveness of behavior change techniques.

Adopting new, more efficient technologies is where substantial environmental benefits are generally reaped (Stern, 1992). Home energy adoption behaviors used to center around major capital investments like installing more insulation in the attic or updating to energy-efficient appliances. However, now there are several other relatively inexpensive and easy actions that can be done, including installing compact fluorescent light bulbs, low-flow shower heads, sink aerators, programmable thermostats, weather stripping, and storm doors. One area that is ripe for more research is field experiments on adopting these inexpensive technologies. For example, compact fluorescent light bulbs are now readily available, but consumers have been slow to switch to them,

even though each bulb saves more than US$50 in the course of its life. Psychologists have been even slower to find out why, with few research projects devoted to them, none of it met the inclusion criteria for the meta-analysis (Howard, Delgado, Miller, & Gubbins, 1993; Menanteau & Lefebvre, 2000; Reynolds, DeSisto, Murray, & Kolodinsky, 2007; Shresta, Karmacharya, & Sandasiri, 1999).

Unfortunately, there are even greater problems that are not addressed in the psychology literature. At the global level, environmental problems are driven by two forces: an ever-increasing human population and an ever-increasing desire for more material goods (Bandura, 2002; Ehrlich & Ehrlich, 2002; Swim et al., 2010). If human population continues to grow and if human aspirations continue to focus on increased material wealth, then humans are likely to cause our own ending. Psychological research has done very little to counter the trends of increasing population and increasing materialism. In fact, in the world of advertising and marketing, applied psychologists are working feverishly to accelerate and exacerbate materialistic desires.

The potential severity and consequences of environmental problems cannot be overstated; the world may be a much different place in 100 years, including being unable to support our current lifestyle and population. Although the problems seem massive, each individual can do something to help ameliorate them, and the cumulative effects of these minor actions can have a big impact. Our modern society seems to have little inclination to begin engaging in PEB, so it is up to the practitioners of the movement—applied psychologists—to spur on these behaviors. In this review, we have provided some insight into how to accomplish this crucial task.

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*References marked with an asterisk were used in the meta-analysis. References marked with asterisks are followed by parentheses with capital letters that indicate which treatments were examined in the experiment. The lettering convention corresponds to the first letter of each of the following words: Easy, Prompts, Justifications, Instructions, Rewards, Modeling, Dissonance, Feedback, Commitment, and Goals.

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