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Clinical Validation and Cognitive Elaboration: Signs That Encourage Sustained Recycling[1]

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Three field experiments coupled the clinical psychology concept of validation with Elaboration Likelihood Model–Heuristic-Systematic Model theorizing to increase the influence of persuasive messages on aluminum can recycling. Signs that validated students' complaints that aluminum can recycling was inconvenient, but persuaded them to recycle anyhow (validate–persuade) were expected to reduce reactance, increase scrutiny and cognitive elaboration, and result in longer term behavior change. Across these 3 experiments, signs influenced recycling relative to baseline; a persuasive message was more influential than convenience; and clinical validation received support as a way to increase message scrutiny, cognitive elaboration, and sustained behavior change.

Implementing recycling programs can provide financial benefits to the setting while simultaneously reducing the volume of waste and contributing to a recycling ethic. However, in public settings with many visitors or much turnover, it may be difficult to convince people that recycling should be done and difficult to teach them how and where to recycle. This series of experiments suggests that using effectively placed and developed signs can increase participation in recycling programs. Experiment 1 used combinations of environmental convenience and persuasive messages to increase recycling. Experiment 2 began to explore the possibility of using signs to increase cognitive elaboration, thereby potentially increasing attitude strength and chances of internalized recycling. Experiment 3 aimed to connect newly elaborated cognitions with sustained recycling, that is, recycling after signs had been removed. The atmosphere at the research site has been favorable toward recycling, but recycling rates have been low. Thus, we construe our project as having two goals: effecting modest levels of persuasion that convince people to begin recycling, and increasing the connection between attitudes and behavior. The research has less to say about creating new attitudes or persuading those who strongly oppose recycling.

The messages were developed using models of persuasion that have produced robust findings in laboratory and applied research. An implication of the models is that signs can have short- or long-term impacts on behavior. Short-term signs are often prompts that give simple directions. They allow people to react automatically, without a great deal of thought and with little or no change in thoughts or knowledge about the issue (cf. Werner, Rhodes, & Partain, 1998). Long-term signs get people to think about and remember the message and be more thoughtful in their behavioral response. This deeper processing should result in stronger, more accessible attitudes, and more enduring behavior change (Fazio, 1990, 1995; Petty, Haugvedt, & Smith, 1995).

This distinction has been developed in Petty and Cacioppo's (1986a, 1986b; Petty, 1994) Elaboration Likelihood Model (ELM) and in Chaiken's (1987) Heuristic–Systematic Model (HSM). Both models describe these two routes to persuasion. Petty and Cacioppo (1986a, 1986b) described the first as the "peripheral route" and contrasted it with the "central" or thoughtful route; Chaiken (1987) labels these "heuristic" and "systematic" processing, respectively. Although there are some differences between the two theories (Eagly & Chaiken, 1993; Johnson & Eagly, 1989; Petty, 1994) within the confines of this research, we treat them as essentially similar, and will use terms from the ELM because of its

[1]This research grew out of an interest in supporting recycling efforts at the university. In developing this work, we brought social psychological knowledge and theory to the problem; however, we were constrained by resources, policy, building codes, and so on, to design interventions that could be left in place to increase student recycling once our research was completed. Thus, by working with an existing program, we could take advantage of an opportunity to study behavior change with reduced ethical concerns about influencing behaviors without persons' awareness or permission; at the same time, the real world setting imposed some limits on us.

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closer ties to Greenwald's (1968) and Brock's (1967) early work on counterarguing and persuasion.

Both ELM and HSM theorists propose that people are more likely to scrutinize a message when they have the motivation and the ability to do so. For the most part, researchers have manipulated motivation when devising ways of increasing or decreasing message scrutiny (cf. Petty, Wells, & Brock, 1976; White & Harkins, 1994, on distraction as an ability impedance). These experimental manipulations generally increase the recipient's psychological involvement, such as convincing participants they will be affected by a policy change proposed in the persuasive message, telling them they will need to discuss the persuasive message with an experimenter, leading them to believe there are real consequences for their judgments, among other strategies. (For a discussion of what *involvement* means, see Johnson & Eagly, 1989, 1990; Petty & Cacioppo, 1990; for other ways of increasing scrutiny, see Petty, Haugvedt, & Smith, 1995, and Petty, 1994.)

This research explores an alternative way of increasing message scrutiny—clinical validation of recipients' complaints about the behavior. Although it may seem counterproductive to agree with an audience's negative voice, clinical psychologists and communication researchers have long argued that people want to feel understood and that feeling understood reduces reactance, hostility, and defensiveness, and leaves people open to new ideas (Alexander & Parsons, 1982; Coates & Wortman, 1980; Coyne, 1985, p. 344; Rogers, 1951). Rogers (1951) suggested that before psychological improvement could occur, clients seeking counseling needed to feel as though the therapist understood and accepted their distress. More recently, clinical psychologists have expressed a similar theme (Hatfield, 1987; Kraus & Redman, 1986; Wikler, Wasow, & Hatfield, 1981). The basic clinical premise is that until individuals feel affirmed or validated, they respond defensively to suggestions that they need to change their way of thinking or behaving. In a particularly compelling example, Kraus and Redman (1986) suggested that denying the legitimacy of a client's postpartum depression only worsened the problem. They suggested instead, pointing out that the individual's depression was perfectly understandable under the circumstances (clinical validation), but that the depression was interfering with her other responsibilities and relationships (persuasive reason for trying to come out of the depression).

Validation is also emerging as a central factor in effective family and marital communication. Gottman's (1979) work showed that in nondistressed couples, individuals validate and sympathize with the partner's complaints; there is often a cycle of complaining by one partner followed by understanding and sympathy by the other, and ultimately better understanding on both sides. In contrast, in distressed couples, "cross-complaining" occurs. One partner's complaint is often followed by a complaint from the other that is matched by the first, with neither side listening to the other, and hostility often increasing with every exchange.

In the domain of customer complaints, anecdotal and research evidence also supports validation as a way to reduce defensiveness, maintain positive customer relations, and increase satisfaction with the problem's resolution. Managers report that customers are more satisfied with their treatment if customer service representatives spend time validating complaints before beginning to attempt remediation. In contrast, if the service representative fails to apologize or to really "hear" the customer's problem, the exchange can escalate to the point where the problem is unresolvable (Blodgett, Granbois, & Walters, 1993; Furlong, 1993; Kelley, 1993).

This array of studies suggests a persuasive recycling message may be more effective if it acknowledges recipients' reasons for not recycling than if it takes a hard-line, persuasive approach. It suggests that validation could operate by increasing motivation (viz, "willingness to listen") as well as ability (by reducing stress and hostility that may impair cognitive functioning—although our signs are not designed to produce such extreme levels of reactance). Research in littering, antipollution, and sales indicated that softer, or more polite approaches were more effective than reactance engendering "hard sells" (see Brehm & Brehm, 1981, for summary). As another example, warning recipients about an upcoming persuasive attempt can reduce message impact because it arouses reactance and elicits counterarguing (e.g., Petty & Cacioppo, 1979a). We adopted Kraus and Redman's (1986) suggestion, and hypothesized that we could reduce potential reactance while increasing acceptance through a combination of validation and persuasion—reassurance that one's perspective is valid with provision of a reason for change.

Questions about validation's impact are raised in all three of these experiments. The first experiment compares the persuasive impact of a validated message, a counterarguing message, and three levels of recycling convenience, and asks whether an effective message can overcome inconvenience. The second and third experiments use a standard ELM–HSM paradigm to ask whether validation functions like other variables known to increase message scrutiny.

EXPERIMENT 1

Theoretical Background

The first experiment evaluates two kinds of persuasive messages; one designed to increase receptivity to the message by first validating and then persuading students to ignore their complaint about inconvenience, the other to increase persuasive impact by counterarguing against the students' complaint that recycling is inconvenient. These persuasive mes-

sages were compared with manipulated levels of recycling convenience, a situational variable thought to be essential in successful recycling programs (Geller, Winett, & Everett, 1982), and the focus of the students' complaints.

Persuasion and Counterarguing

Validation is thought to enhance a message's impact by increasing the recipient's scrutiny of the message. An alternative persuasive strategy is to undermine a recipient's counterarguments. That is, there is a long history of research suggesting that effective counterarguing is one's best defense against persuasion. McGuire's (1964) work on inoculation showed that a weak attack on one's beliefs reduced the impact of a subsequent stronger attack because the weak attack stimulated people to develop or learn counterarguments. Similarly, Greenwald (1968) and Brock (1967) measured cognitions in response to persuasive messages and found that people who simply reiterated a message tended to be persuaded by it, whereas those who refuted the message or otherwise argued against it tended to resist persuasion. The ELM supports the view that counterarguing (or negative elaboration) leads to resistance, whereas positive elaboration leads to persuasion (e.g., see Petty et al., 1976). An implication is that one way to persuade people is to anticipate and refute their counterarguments. Thus, as a complement to ELM–HSM methodology, in which weak and strong arguments are prescaled and presented to participants (Petty & Cacioppo, 1986a), our counterarguing approach will identify recipients' major reason for rejecting a message and refute that reason. In essence, instead of validating participants' complaint, this strategy would reject it.

In this research, to ascertain typical counterarguments, we interviewed students in the buildings chosen as research sites. The overwhelming reason given for not recycling aluminum cans was that students did not want to go out of their way to find or use the recycling bin; specific explanations included, "I'm in a hurry" and "don't want to find a [can] crusher." Our persuasive message designed to counter this complaint was simply "It only takes 30 seconds."

Convenience and Recycling

Consistent with students' complaints, a number of researchers have demonstrated that increasing convenience is an effective way to increase recycling (e.g., Geller, Winett, & Everett, 1982 described studies of proximity and multiple containers, two ways of increasing convenience). Hormuth, Katzenstein, Bruch, and Ringenberger (1993) developed this idea more fully by grounding it in Behavior Setting Theory's concept of natural streams of behavior. Hormuth et al. (1993) suggested that recyclers and recycling managers too often remove recycling from the place where the recyclable product is used. This makes recycling more difficult because it becomes a separate behavior rather than being incorporated as a natural and habitual part of the total consumption–disposal process. In this case, students drink a soda during class and then throw the aluminum can away as they leave the room on their way to another class, lunch, work, and so on. Discarding the can becomes a natural part of consumption.

According to the behavior stream approach, the optimum strategy for ensuring that recycling replaces disposal is to provide a recycling container either next to the garbage can or on the student's route to the next class. Using this idea, we manipulated convenience by varying how close recycling bins were to students' travel routes. (Whereas providing small containers in each classroom is most convenient, it is expensive and requires that someone collect and crush the cans, a cost that our university was unwilling to absorb.)

Convenience Versus Persuasion

Research on "mindless" cooperation has shown that making a request and providing a reason is more effective than just making the request, in theory because the reason fulfills the target individual's expectations for how requests should be made (Langer, Blank, & Chanowitz, 1978). This suggests that signs with persuasive messages that justify the recycling instruction should be more effective than simple prompts (even though a prompt may be effective, and may increase recycling relative to baseline).

Summary

In summary, Experiment 1 had four hypotheses and one question: (a) Signs can effect increases in recycling; (b) increasing convenience will increase recycling; (c) a (low cost) persuasive message is more effective than (high cost) convenience for increasing recycling; (d) with similar levels of convenience, a persuasive message is more effective than a prompt for increasing recycling; and (e) is validation–persuasion more effective than counterarguing as a strategy for behavior change?

Method

Selection of Buildings

After 2 weeks of observation and data collection in 12 buildings on the university campus, we selected five buildings to serve as research sites. Criteria for inclusion were that the building had (a) a minimum of 6 classrooms in use, (b) a vending machine providing sodas and juices in aluminum cans, (c) a can crusher for recycling, (d) no other ongo-

ing recycling program, (e) evidence of large soda–juice sales, and (f) a variety of course topics being taught in the building so that our manipulated treatments would not be confounded with type of course or particular student major. This study was conducted during the spring quarter, between March and June.

Preliminary Survey

As noted previously, a brief survey provided information about students' primary reasons for not recycling so that appropriate counterarguing and validation messages could be devised to undermine those reasons. Small numbers of students in each building (total $n = 50$) were interviewed about why they did or did not recycle.

Experimental Conditions

The design was a 2 (time: baseline–treatment) × 5 (treatment) factorial; the treatments included three levels of convenience (low–moderate–high) and two different messages (counterarguing–validate persuade). The "no signs" baseline lasted 1 week, and treatment lasted 3 weeks.

Convenience. Because of fire codes that limit sizes and locations of recycling bins, we could only achieve certain levels of convenience (low & high) in two particular buildings; the remaining treatments (moderate convenience and the two persuasive messages) were randomly assigned to the remaining buildings. Low convenience was achieved by placing two recycling bins in a room away from the regular traffic flow. Moderate convenience meant leaving only one recycling bin near a major exit at one end of a building; thus it was in the "stream of behavior" for many students, but out of the way for many others. In addition, high convenience was achieved by putting recycling bins at major exits on two floors (two bins total), thereby increasing the number of students who would naturally pass by on their way to and from class. Bright gold "prompt" signs on each wastebasket in the classrooms and adjacent hallways said "no aluminum cans, please" and directed students to the recycling container. The signs themselves represented a major change in all 5 experimental buildings and could account for changes even in the low convenience condition. Compared to preexisting conditions, the actual physical changes were: low convenience, adding one bin; high convenience, adding one bin to a new floor; moderate convenience; and no physical changes.

Persuasion. In both buildings with persuasive signs, the recycling bin was located so as to achieve a moderate level of convenience; no changes in bin location were needed.

No Aluminum Cans Please!!!!!

Use the Recycler Located on the First Floor, Near the Entrance

It May Be Inconvenient

But It Is Important!!!!!!!!!!!

FIGURE 1 The validation–"important" sign.

Bright gold "persuasive" signs on all wastebaskets had the same basic information as signs in the convenience conditions, including the location of the recycling bin. Each also contained a final persuasive message; the one counterarguing against student complaints added, "it only takes 30 seconds" and the other, using a validation–persuasion approach had the message "it may be inconvenient but it is important." As an example, the validation–persuasion sign that appeared is shown in Figure 1.

Data Collection and Analysis

Two different experimenters counted the aluminum cans in different buildings at the end of each weekday. They counted the cans in wastebaskets in each classroom and in the hallways, and the cans in the can crusher(s). In most cases, aluminum cans were easily located in the trash and accurate counts were easily achieved. To further assure valid data, the importance of accuracy was stressed and data collectors expected their counts to be checked. Although no reliability data were collected in this experiment, they were collected in four other identical studies, and rs were consistently found to be above .90. To enhance the experiment's ecological validity, any discarded cans were left in wastebaskets for the custodians, who emptied the cans every morning.

For data analysis, the average daily number of aluminum cans found in each classroom was divided by the total estimated to have come from that room[2] to compute the proportion that had been discarded. Each classroom was the unit of analysis, and the condition mean was an average across these proportions. Wastebaskets in the adjacent hallways were treated as a single room. Thus, the number of classrooms plus one hallway in each building became the n

[2]The equation was MD/(MD + MR) for each classroom, where MD = mean per day in garbage and MR = mean per day in recycler estimated to be from that room. Means per day were used to avoid problems of missing data (e.g., if a class were in session or a door were locked). For MR, we assumed that the cans in the crusher came equally from each classroom. Thus, if there was a daily average of 100 cans in the crusher and 10 rooms in the building, MR for that day would be 10 for each room. Given the different classroom sizes and different distances to the can crusher, this assumption may not be entirely valid but is less subjective than attempting to devise a separate adjustment for each classroom.

for each treatment condition. This strategy was chosen instead of treating the whole building as the unit of analysis for several interrelated reasons. First, classrooms are the natural social setting, with classes meeting regularly, containing the same students, and many people regularly consuming a drink during class. Second, averaging across proportions in the classrooms is conservative, requiring that the persuasive signs have similar impacts in multiple rooms, rather than a strong impact in only part of the building (such as a large, environmentally oriented class). Third, by similar reasoning, this approach provides a check on social pressures or modeling of recycling. If these social processes contributed to recycling rates, they would need to do so in many rooms to impact the building's mean recycling rate. A potential problem is that classrooms may not be independent—some students may have more than one class in the building and use more than one aluminum can per day, or their recycling behavior may be influenced by others, resulting in a dependency between these scores.

Results

For clarity, results are presented as percentage of cans recycled rather than percentage thrown away. For convenience and clarity, the data were averaged across the 3 treatment weeks with signs in place, and these data were analyzed using a 5 (condition) × 2 (time: baseline–treatment) between and within participants analysis of variance (ANOVA). Because of significant differences between conditions at baseline, an analysis of covariance was used to test for treatment effects; the covariate, baseline recycling was homogeneous across conditions, covariate by treatment interaction, $F(4, 53) = 1.14$, $MSE = 93.53$, $p > .05$, partial $\eta^2 = .08$. Table 1 presents baseline means and the raw and adjusted treatment means (Winer, 1962, p. 604) averaged across the 3 treatment weeks. Specific hypotheses were tested using planned t tests for disparate ns (Bruning & Kintz, 1987).

Overall Impact of Interventions

The first hypothesis was that all conditions would result in increased recycling; this hypothesis was supported by an overall main effect for time, $F(1, 58) = 313.69$, $MSE = 80.95$, $p < .001$, partial $\eta^2 = .84$, as well as by the separate planned t tests for each condition (see Table 1 & Figure 2). Figure 2 presents the weekly results for each building to show the degree of consistency over time, the typical pattern in this series of studies.

Increasing Convenience

The next hypothesis was that increasing convenience would result in increased recycling (see Table 1, column 3, adjusted means). Controlling for baseline, there was a significant effect of treatment, $F(4, 57) = 7.08$, $MSE = 94.43$, $p < .001$, partial $\eta^2 = .33$, and a priori tests indicated that putting a recycler at two convenient locations resulted in significantly more recycling than either the low or moderate convenience arrangement; these latter two conditions did not differ from one another.

Persuasive Signs Versus Convenience

The third and fourth hypotheses compared persuasive messages with convenience. One hypothesis was that each persuasive sign would have more impact than any of the convenience conditions. This hypothesis was supported for the validate–persuade ("inconvenient but important") condition and was partially supported for the counterarguing ("30 seconds") condition. As shown in Table 1, column 3, when the persuasive message was "inconvenient but important," the average rate of recycling was 80% across the 3 weeks, substantially higher than the means for all of the convenience conditions. The "30 seconds" sign resulted in a high rate of recycling (68%) that was significantly higher than both the low and moderate convenience conditions, although it was

TABLE 1
Percentage of Aluminum Cans Recycled by Condition, Time, and Adjusted for Baseline Recycling, Experiment 1

	Time		
Treatment	*Baseline (1 Week)*	*Signs (3 Weeks)*	*Means Adjusted for Baseline*
Low convenience	45.3	62.0*	58.3_a ($n = 9$)
Moderate convenience	30.4	58.2*	61.1_a ($n = 16$)
High convenience	34.4	70.2*	71.3_b ($n = 11$)
"30 seconds"	39.0	69.0*	68.1_b ($n = 20$)
"Inconvenient but important"	38.8	80.9*	80.1_c ($n = 7$)

Note. ns indicate the number of classrooms in the building. In column 2, *s indicate the increase of signs over no signs (baseline) by a priori, one-tailed t-tests at $p < .05$. In column 3, percentages with different subscripts differ at $p < .05$; all but the comparison between the two persuasive signs are one-tailed.

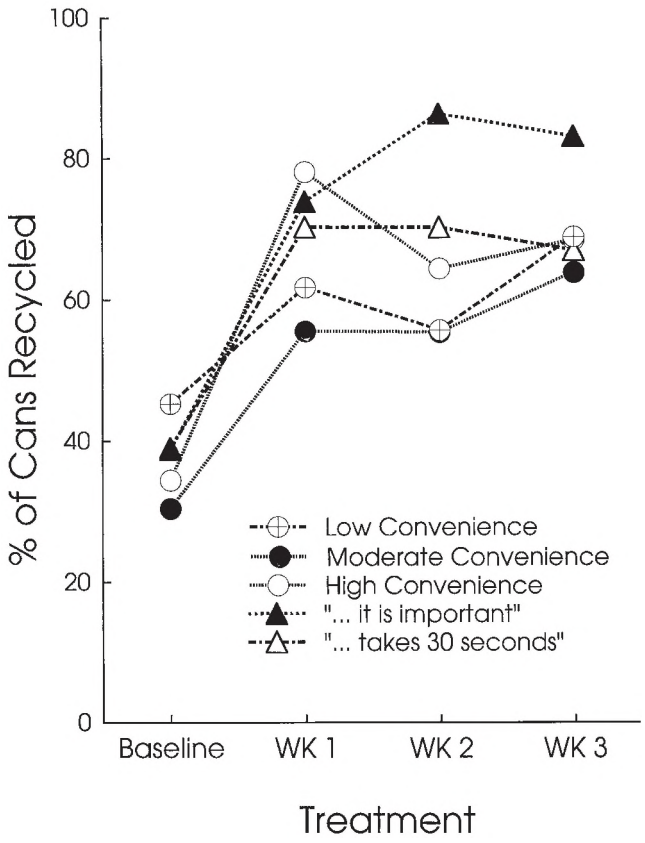

FIGURE 2 Percentage of cans recycled during baseline and treatment, Experiment 1.

not different from the condition in which recycling bins were most conveniently located.

Another way of comparing a persuasive message with convenience is to select groups with similar convenience (in this case, moderate convenience), and hypothesize that each persuasive sign would have more impact than the "prompt," or simple instruction to recycle. As noted previously, both persuasive messages produced significantly more recycling than the prompt–moderate convenience condition.

"30 Seconds" Versus "Inconvenient But Important"

An a priori, two-tailed test indicated that the validate–persuade sign ("inconvenient but important") was significantly more influential than the sign that counterargued ("30 seconds").

Discussion

This experiment examined the impacts of convenience and persuasion and showed that both could affect recycling. The results indicated that increasing convenience is an effective way to increase recycling; however, this level of convenience was not economically viable in our setting. Recycling in response to the persuasive signs also support our view that although convenience is important, in settings where it cannot be provided, persuasive signs can be very effective at increasing recycling. Thus, although it may be important to embed recycling into the ongoing stream of behavior, that stream can include going to out-of-the-way places if a sign invokes sufficient impetus to do so. As suggested by clinical, communications, and other work, an effective sign was one that validated participants' complaints that recycling was inconvenient and then persuaded them to recycle anyhow. This sign was more effective than one that counterargued against students' concerns—although the latter sign was effective.

An alternative explanation of our signs' effectiveness is increased awareness of recycling, or a "Hawthorne" effect (positive response to any change). This is the simplest explanation for the increase over baseline in the low and moderate convenience conditions where large numbers of signs (without any substantial change in convenience) yielded significant increases in recycling relative to baseline. However, note that both persuasion conditions yielded significantly higher recycling rates than did the low and moderate convenience conditions. These findings increase our confidence that the persuasive signs provided something more than simple awareness of the recycling bins or the university's interest in recycling.

One problem with the two persuasive signs is that the type of persuasive message was confounded with validation. If "it is important" is more influential than "it only takes 30 seconds," that could explain their differential effectiveness. That is, what we have called a counterargument could also be viewed as a persuasive message. Experiment 1, then, contained a confound, with one persuasive message not validated, and the other validated. The purpose of the next experiment was to uncouple validation and message. An additional purpose was to link behaviors with cognitive responses to the signs. As noted earlier, ELM–HSM theorizing suggests that messages can influence attitudes—and by implication, behaviors—via either central or peripheral routes. Peripheral route persuasion should not be bolstered by cognitions, whereas central route influence should be. If validation increases cognitive processing and the message being processed is strong, it should be associated with central route processing and elevated cognitions.

EXPERIMENT 2

Experiment 2 used a factorial design to cross validation (no–yes) with type of persuasive message ("it only takes 30 seconds," "it is important") during 2 time periods (baseline–signs in place). We collected both behavioral and questionnaire data to estimate the relation between cognitive

elaboration and behavior change. In addition to an expected main effect for time on recycling behavior, we hypothesized main effects for validation such that validation would result in higher levels of recycling as well as higher levels of cognitive support. We used prescaling to ascertain whether the two messages were equally persuasive, and expected that validating strong messages would increase their influence (Eagly & Chaiken, 1993; Petty & Cacioppo, 1981).

Method

The basic methodology replicated that of Experiment 1: conditions were randomly assigned to building; recycling bins were located so as to achieve moderate levels of convenience; basic messages on the sign were the same; data collection was identical to that of Experiment 1; and data analysis was similar, using a combination of repeated measures, analysis of covariance, and planned *t* tests to evaluate specific hypotheses. A reliability check on the numbers of cans yielded a reliability coefficient of .99 (agreement on all but 2 containers).

The experiment was conducted during summer quarter, approximately 1 month after removal of the Experiment 1 signs. Because of low student enrollments and high maintenance and construction activity, only four suitable buildings could be located for this project. Three of the buildings had been used in Experiment 1 (see Table 2 for details), and all met the building selection criteria detailed in Experiment 1.

Experimental Design

The design was a 2 (validation: no–yes) × 2 (persuasive message: "it only takes 30 seconds", "it is important") × 2 (time: baseline–treatment) between and within participants factorial. Baseline data were gathered for 1 week, and the treatment period lasted 3 weeks.

Persuasive Messages

As a check on whether the two messages were comparable in persuasiveness, they were embedded in a list of similar signs and rated by Introductory Psychology students in a different quarter and in a classroom away from the experimental buildings (total $n = 262$, although not all students completed all items). Students were told to imagine that they had an aluminum can and were about to discard it when they saw a sign encouraging them to recycle it; all students rated 9 different signs. On a 5-point scale, ranging from 1 (*I would definitely toss*) to 5 (*I would definitely recycle*), the mean for "it only takes 30 seconds" was 3.92 and that for "it is important" was 3.79, $F(1, 256) = 5.87$, $MSE = 0.36$, $p < .016$, partial $\eta^2 = .02$; although significant, the 0.13 unit difference was too small to be considered meaningful for our purposes. On a scale ranging from 1 (*extremely negative influence [on my opinion about recycling the can]*) to 5 (*extremely positive influence*), the mean for the "30 seconds" sign was 3.69 and for the "important" sign was 3.75, $F(1, 255) = 1.02$, $MSE = 0.49$, $p > .05$, partial $\eta^2 = .00$. All means were above the neutral point, indicating participants expected the two messages to have similar positive influences on their opinions and behavior.

Questionnaires

Questionnaires were administered during the final week of the quarter, after the signs had been removed for 4 days. The rationale is similar to using a survey to test opinions about any publicized or advertised issue. If our signs have an impact on people, we should be able to detect that impact by surveying a sample drawn randomly from each building.

Two to three experimenters worked in each building. We determined the optimum time and day to administer the questionnaire for each building by reviewing the class schedule and selecting a time with the highest number and diversity of classes; for three of the buildings, this was on the same day at

TABLE 2
Percentage of Aluminum Cans Recycled by Condition, Time, and Adjusted for Baseline Recycling, Experiment 2

	Time		
Message	*Baseline (1 Week)*	*Treatment (3 Weeks)*	*Means Adjusted for Baseline*
"It is important"	58.3	74.4*	71.7 ($n = 20$)[a]
"Inconvenient but important"	66.8	75.0**	67.2 ($n = 11$)[b]
"Only 30 seconds"	49.7	65.0*	67.4 ($n = 16$)[c]
"Inconvenient but only 30 seconds"	29.2	56.1*	70.6 ($n = 7$)[d]
M	53.7	69.4*	

Note. *n*s indicated number of rooms in that building. In column 2, symbols indicate an increase of treatment over baseline by a priori, one-tailed, *t*-test: *$p < .05$. **$p < .10$. Adjusted means (column 3) do not differ from one another.

[a]Condition–recycling in E1 = 30 seconds; baseline = 39.0%, treatment = 69.0%. [b]Condition–recycling in E1 = High convenience; baseline = 34.4%, treatment = 70.2%. [c]Condition–recycling in E1 = Moderate convenience; baseline = 30.4%, treatment = 58.2%. [d]Not used in E1.

approximately the same time, and for the fourth, it was the following day at approximately the same time of day. Because the summer session provided a small number of potential participants, we attempted to draw a 100% sample in each building by asking every person we saw to "help us with a class project and fill out this questionnaire." By appealing to their support of a fellow student's project, we deemphasized the particular content of the questionnaire and were able to include people who supported and did not support recycling. Most people agreed or refused without asking the nature of the questionnaire. The most typical reason for refusing was that the student was on the way to work or to another class across campus; a small number of participants said "I just did that in building X" and were not counted as refusers.[3] Although we sampled everyone, results include only people with a class or office in the building to assure they had had opportunities to read our signs. Sixty-two percent of these participants were female.

We took great care to distance ourselves from the signs so that responses to the questionnaire would be independent. On the first and last days of treatment, we posted and removed the signs early in the morning before students arrived on campus; during the project, we gathered recycling data late in the afternoon, when most students had left campus; and although there was some overlap in duties, for the most part, different people gathered recycling data and administered the questionnaires.

Respondents used 7-point scales to indicate their answers. Three questions probed the perceived convenience emphasized by the "it only takes 30 seconds" sign. These items tapped how accessible the can crusher was (time to get there, convenient to get there, and ease of use). These items did not form an adequate scale (Cronbach's $\alpha = .59$) and were analyzed separately. Two questions probed reactions to the "it is important" sign: one simply asked "how important is it to recycle aluminum cans?" ranging from 1 (*extremely unimportant*) to 7 (*extremely important*). The other was an open-ended question asking them to explain their answer to the "importance" question, listing "as many reasons as you can". Responses to the open-ended question were analyzed in three ways. First we counted the number of different reasons for the importance of recycling, regardless of their quality (consistent with the idea that people select reasons they themselves find compelling; McGuire, 1964; Greenwald, 1968). Second, to reduce the skew in these data and increase confidence in the results, we converted these scores to a simple index of whether the person had given any positive reasons. Counts of distinct reasons were provided by a single coder, with reliabilities provided by two additional coders, each counting half of the questionnaires; all condition information was removed for these counts and responses from different conditions mixed together. Reliabilities on the counts were $r = .91$ and .94 for the separate raters, and .93 overall.

Third, we used content analyses to indicate the presence or absence of strong, external, and weak reasons. Categories for the content analyses were developed and refined in a multistep process. We began with the four substantive reasons for recycling aluminum cans, and called these strong environmental reasons: extends life of landfill, reuses natural resource, uses less energy compared to mining, and reduces the need to disturb environment through mining. Some weak environmental reasons emerged as we read participants' answers: "for the environment," "they care about the environment," and "to recycle." A final cluster of reasons emphasized external motivators such as "for the money," "politically correct," "social pressure," "convenience of recycling bins," and "to keep the building clean". One person categorized all responses into these three categories, and a second categorized a randomly selected 25%. Kappa coefficients on these ratings were .95 for the strong environmental reasons (98% agreement), .82 for the weak environmental reasons (93% agreement), and .77 for the external reasons (90% agreement). Only 5 people gave negative reasons so these were not analyzed. We used category present–absent instead of actual counts to reduce skew (only a few people gave more than one reason per category, which produced considerable skew in these counts).

A final question asked respondents to estimate the percentage of people who did recycle the cans they used in this building as a check on whether students perceived differential recycling norms across the buildings.[4]

Results

Recycling Rates

Overall impact of interventions. The first hypothesis was that all conditions would result in increased recycling; this hypothesis was supported by an overall main effect for time, $F(1, 50) = 41.23$, $MSE = 154.77$, $p < .001$, partial $\eta^2 = .45$, as well as by the separate planned t tests for each condition, except for the "inconvenient but important" condition, which showed a marginally significant increase (see Table 2; for $\alpha = .05$, the critical difference was 8.9, whereas the obtained difference was 8.1). Note also that although the final rate of recycling was quite high in that group (unadjusted recycling = 75.0%), the high baseline (66.8%) made it difficult to effect a substantial increase in recycling.

[3]Experimenters recorded refusals but the original data were lost; experimenters recalled few refusals (8–15 refusals–buildings, or 14%–25%).

[4]There was little overlap among these items. Examination of the residual correlation matrix (with "number of reasons" representing the open-ended responses) indicated that rs(62) ranged from .01 to .32, with 4 of the 5 significant correlations involving "convenience."

Treatment effects. As in Experiment 1, the covariate (baseline recycling) contributed significant variance, $F(1, 49) = 51.56$, $MSE = 226.96$, $p < .001$, partial $\eta^2 = .51$, and was homogeneous across conditions, heterogeneity $F(1, 48) = 0.05$, $MSE = 231.46$, $p > .05$, partial $\eta^2 = .00$. The 2 (message) × 2 (validation) analysis of covariance on recycling rates indicated that once the covariate was removed, there were no significant main or interactive effects due to the different signs, all Fs(1, 49) ≤ .71, $MSE = 226.96$, ps > .05, partial η^2s ≤ .01 (see Table 2). Although validation did not yield significant main or interactive effects, the obtained pattern could be consistent with ELM–HSM theorizing, because both peripheral and central routes can have short-term impacts (Petty & Cacioppo, 1986a, 1986b). The more relevant question is whether validation increased elaboration, an indication of central route processing.

Questionnaire

The primary purpose of the questionnaire was to measure differential attitudinal and cognitive reactions to validation: Did validation increase scrutiny of the two messages? This question was addressed by comparing the same message with and without validation. A significant main effect for validation would indicate similar effects on both signs, and a significant validate by sign interaction would indicate differential impact. As shown in Table 3, the latter pattern was obtained for both attitudes and cognitions.

Cognitions and message scrutiny. Table 3 shows that the combination of validation with "it is important" (compared to that message without validation) resulted in significantly higher ratings of the importance of recycling and significantly more cognitions supporting recycling's importance. For these three variables (importance, number of reasons, percent of respondents providing any reason), the overall validation by message interactions were significant, and planned one-tailed t tests indicated significant differences between the validated and nonvalidated "important" message; message by validation interaction Fs were as follows: recycling important, $F(1, 136) = 3.59$, $MSE = 2.43$, $p < .06$, partial $\eta^2 = .03$; number of arguments, $F(1, 138) = 6.24$, $MSE = 0.96$, $p < .01$, partial $\eta^2 = .04$; percentage making any arguments, $F(1, 138) = 6.28$, $MSE = 0.20$, $p < .01$, partial $\eta^2 = .04$.

In contrast to the "it is important" sign, validating the "30 seconds" message did not increase its impact on indices of persuasiveness. If anything, participants generated more arguments and were more likely to generate an argument when the sign was not validated, although the differences are not significant by two-tailed tests.

Content analyses. A 2 (message) × 2 (validation) × 3 (type of reason) between and within participants ANOVA on the content analysis yielded a significant main effect for type of reason, $F(2, 276) = 9.43$, $MSE = 0.09$, $p < .001$, partial $\eta^2 = .07$. Examination of the mean percentages indicated that people used strong environmental reasons most, external motivators second, and weak environmental reasons least frequently. A significant message by validation interaction essentially replicated the previous analyses on number of arguments, $F(1, 138) = 5.23$, $MSE = 0.18$, $p < .02$, partial $\eta^2 = .04$. That is, validating "it is important" yielded more arguments, whereas validating "it only takes 30 seconds" yielded equal numbers of arguments compared to each sign's nonvalidated counterpart. Type did not interact with the other factors, indicating that the basic message by validation pattern occurred for all three kinds of reasons, Fs(2, 276) ≤ 1.62, $MSE = 0.09$, ps > .05, partial η^2s ≤ .01.

Perceived inconvenience. An additional purpose of the questionnaire was to provide checks on potential confounding factors. Using "it may be inconvenient" as validation may have had the undesired consequence of increasing perceptions that recycling was inconvenient. There was no evidence of this. Furthermore, coupling validation with the specific counterargument that recycling did not take a lot of time did not result in different ratings of the convenience of getting to or using the recycling bin. Indeed, means for the three questions measuring convenience did not differ, as indi-

TABLE 3
Impact of Validation on Attitudes and Cognitions, Experiment 2

Validation	*No*	*Yes*
Message: "It only takes 30 seconds"		
Time	2.44	1.64
Convenient	5.84	5.87
Easy use	6.00	6.25
Importance of recycling	6.04	5.68
Average number of positive arguments	0.58	0.35
Percentage making any positive arguments	35	22
Types of arguments		
Strong	27%	18%
External	12%	8%
Weak environmental	4%	5%
Message: "It is important"		
Time	2.30	2.04
Convenient	5.00	5.89
Easy use	5.32	5.91
Importance of recycling	5.39	6.05*
Average number of positive arguments	0.34	0.95*
Percentage making any positive arguments	18	45*
Types of arguments		
Strong	16%	32%
External	5%	29%
Weak environmental	8%	13%

Note. Items were measured on 7-point scales. The open-ended question followed the attitude item "How important is it to recycle?" and simply asked people to "Please explain your answer giving as many reasons as you can." Because of missing data, cell n ranges from 18 to 40. An asterisk indicates the two means in a row differ by one-tailed, a priori t tests.

*$p < .05$.

cated by the lack of any significant main or interactive effects, "time to get to," Fs(1, 89) ≤ 2.36, MSE = 2.38, ps > .05, partial η^2s ≤ .03; "convenient to get to," Fs(1, 93) ≤ 1.94, MSE = 3.37, ps > .05, partial η^2s ≤ .02; and "easy to use," F(1, 82) ≤ 2.22, MSE = 2.52, ps > .05, partial η^2s ≤ .03 (see Table 3).

Recycling ethos. Another purpose of the questionnaire was to evaluate the extent to which a "recycling ethos" was emerging in the buildings in case that spirit might account for behavior change rather than our experimental signs. Participants' average estimates of what percentage of people recycled aluminum cans ranged from 43% to 47% per building, and there were no differences due to treatments alone or in combination, all Fs(1, 111) ≤ 0.24, MSE = 468.16, ps > .05, partial η^2s ≤ .00, suggesting that the buildings did not differ in their overt recycling spirit.

Carryover from Experiment 1. Approximately 4 weeks separated Experiment 2 from Experiment 1. Although we assumed that the new summer quarter would bring a new group of students to the buildings, the high baselines in Experiment 2 suggested there may have been carryover. Three buildings were used in both experiments. As can be seen in the notes to Table 2, the one building not included in Experiment 1 had a much lower baseline than the three previously used buildings. In these reused buildings, baseline recycling had dropped from the Experiment 1 treatment levels by only 4% to 11% over the quarter break. Also, the new building had the highest increase in recycling once the signs were put in place. This information converges on the idea that there was carryover, even though almost 1 full month separated the two experiments. These high base rates may have attenuated the impact of three of our Experiment 2 signs.

An important question is whether there was also cognitive carryover. The "validate–important" building from Experiment 1 was not used in Experiment 2, precluding concerns about cognitive carryover from that condition. None of the other signs from Experiment 1 deliberately invited scrutiny, and would not be expected to increase cognitive elaboration. Consistent with this, data from Experiment 2 show that cognitions in two of the reused buildings were similar to those in the single new building. The third reused building contained the validation or "important" sign, and as expected yielded the highest rates of cognitive elaboration and attitude change. This building had a "prompt" sign in Experiment 1. The overall pattern supports the idea that the cognitive data from Experiment 2 were due to increased processing of the validate–"important" sign, and less likely due to cognitive carryover from Experiment 1 treatments.

Discussion

This experiment replicated the finding of Experiment 1 that signs with persuasive messages increased recycling relative to baseline. We did not replicate the finding that validation coupled with "it is important" produced significantly higher levels of recycling relative to the same message without validation. We suspect that the high base rate may have dampened this sign's behavioral impact. Indeed, as noted previously, the final (unadjusted) recycling rate is quite high, almost as high as the 80% recycling achieved in the first experiment. Note also that the adjusted recycling scores did not differ between this sign and the other three. Thus, the relatively lower impact appears not to represent a decrease in this sign's effectiveness, but rather occurred because of the high base rate and because the other signs yielded comparably high levels of recycling.

With respect to cognitive elaboration, this same message (validate–"important") resulted in the highest levels of cognitive elaboration, and we remain intrigued by the utility of validating someone's concerns as a vehicle for increasing their scrutiny of and thinking about a message. Using "it may be inconvenient" as our particular form of validation did not appear to increase participants' perceptions that recycling was inconvenient.

Contrary to our expectations, validation did not appear to increase positive cognitive elaboration of the message that recycling took "only 30 seconds". If anything, the mean number of cognitions was in the opposite direction, though the differences were not significant. Compared to the validate–"important" sign, the validate–"30 seconds sign" yielded a comparable rate of recycling, but relatively few bolstering cognitions. In sum, the "30 second" message produced mixed results, and seems to be operating not as a weak message or as a strong one, but somewhere in between, much like its impact in Experiment 1. Although this counterargument was accurate (the travel time from most classrooms was actually less than 30 sec), it may be that it operated as a moderately persuasive message because it did not adequately address students' broader complaints about recycling's inconvenience (going out of their way, using a physically difficult can crusher). For soda drinkers who had personal experience with the array of inconveniences, "it only takes 30 seconds" may have seemed quite limited as a persuasive message even though it had seemed persuasive to students doing the prescaling. To further explore validation's potential as a way of increasing message scrutiny, we conducted a third experiment that used the standard ELM–HSM weak–strong paradigm for demonstrating that a manipulation can increase message scrutiny.

EXPERIMENT 3

The purposes of the experiment were to examine whether clinical validation served to increase scrutiny, cognitive

elaboration, and behavior change, and whether cognitive elaboration was linked to behavioral maintenance (continued behavior with the message removed). A typical technique in the ELM and HSM traditions for determining whether a manipulation increases scrutiny is to couple the manipulation of interest with two different messages. The messages advocate the same position, but one is composed of cogent arguments (strong) and the other is composed of specious yet plausible arguments (weak). The expected pattern is that left unscrutinized (peripheral route or heuristic processing), weak and strong messages are equally effective, but under scrutiny (central route or systematic processing), the strong message is influential but the weak message is undermined and rejected (Petty & Cacioppo, 1979b, 1984; Petty, Cacioppo, & Goldman, 1981; see Petty & Cacioppo, 1981 or Eagly & Chaiken, 1993, for reviews).

Experiment 3 had three components, each addressing the question of whether validation increases message scrutiny. If validation is effective (a) nonvalidated weak and strong messages should not differ in behavioral impact, attitude change, or positive cognitive elaboration; (b) the validated strong message should be more effective than the weak one in behavioral impact, attitude change, and positive elaboration; and (c) positive cognitive elaboration should translate into more enduring behavior change, that is, behavioral maintenance with the signs removed. Thus, changes based on a peripheral route should not be sustained, but those based on a central route should be.

Method

Experimental Design, Manipulations, and Data Collection Procedures

The design was a 2 (Validation: no–yes) × 2 (Message: weak, "it is the 90s", strong, "It is important") × 3 (Time: baseline–treatment–follow-up) between and within participants factorial. The methodology (e.g., choice of buildings, data collection, questionnaire administration) replicated that of Experiment 2. To avoid habituation to the signs and increase chances that students would read them, data were gathered more than 6 months after Experiment 2. This 6-month interval would also allay concerns about carryover effects.

The unsigned, baseline period lasted for 2 weeks, the treatment for 3 weeks, the signs were removed, and the follow-up lasted for 1 week after a 1-week delay. During treatment, bright yellow signs on each wastebasket in every classroom in these buildings said "No aluminum cans, please" and directed students to that building's recycling container. The final sentence, at the bottom of the sign, contained the validation and message manipulations. Recycling bins were located so as to produce a moderate level of convenience, except for the validation–"important" condition, which was in a building with an inconvenient location.

To verify that students perceived differences between the two messages (without validation), they were embedded in a list of similar messages and rated by 50 psychology students in a different quarter (their classroom was in one of our experimental buildings and at the time the recycling container was in place, but there were no signs about recycling). Students received extra course credit for their participation. They were told to imagine that they had an aluminum can and were about to discard it when they saw a sign encouraging them to recycle it. All students rated their reactions to 9 different messages on two different scales. On a 5-point scale, ranging from 1 (*I would definitely toss*) to 5 (*I would definitely recycle*), the mean for "it is important" was 3.36 and that for "it is the 90s" was 2.82, repeated measures $F(1, 48) = 8.61$, $MSE = 0.81$, $p < .005$, partial $\eta^2 = .15$. On a scale ranging from 1 (*extremely negative influence [on my opinion about recycling the can]*) to 5 (*extremely positive influence*), the mean for the "important" sign was 3.47 and for the "90s" sign, 2.39, $F(1, 47) = 44.93$, $MSE = 0.65$, $p < .001$, partial $\eta^2 = .49$. Note that the ratings span the midpoint, being in the positive range for the "important" sign and in the negative range for the "90s" sign. A key idea in ELM–HSM research is that the weak message should be reasonable if given a cursory glance. Ratings of the weak message are below but not too far from the neutral point on the scale, thereby satisfying this guideline.

As a further check on how the signs might be interpreted, each sign was then analyzed closely by half of the students: "What do you think the author of this sign had in mind in saying 'it is the 90s'?" ($n = 23$; $n = 27$ for the "it is important" message). Fourteen of the students focusing on the "it is the 90s" sign (61%) made a serious interpretation, saying in essence that the author used it as a reminder we are running out of time if we are to conserve adequate levels of resources; 8 students (35%) interpreted it to mean that recycling is modern or hip, and one gave no answer. The 27 students asked to review the "important" sign made a variety of comments, all sharing the general theme that its author was genuinely concerned about conservation of resources.

When asked if they would use the sign to encourage recycling, students were lukewarm about both messages. Of the students asked to focus on the "it is the 90s" sign, only 7 (30%) said maybe or yes. Eight of the students who said no explicitly said it was too flip or faddish. Of the students assigned to focus on the "it is important" sign, a slightly higher percentage, 52%, said maybe or yes. The difference between the groups is not significant, $\chi^2(2, N = 50) = 2.17$, $p > .05$. For both groups, the dominant reason for declining to use the sign was that they preferred to use a message that gave concrete reasons about the benefits of recycling.

Recycling Data Collection and Analysis

Five students counted aluminum cans in trash cans, left on or under desks, and in the recycling bins. Accuracy in counting

was stressed, and data collectors expected their counts to be checked on a random basis. Reliability data collected by different pairs of data collectors on six occasions during the study yielded reliability coefficients (simple *r*s) between .97 and .99; across the entire reliability data set, the reliability coefficient was .99, indicating high correspondence between data collectors.

Cognitive Response Questionnaire

Questionnaires were administered the Monday after signs were removed, before the delayed follow-up period. The questionnaire opened with a general question about recycling motivations designed to apply to both the weak and strong messages: "Why do people recycle aluminum cans in this building? List as many reasons as you can." In accord with our interest in encouraging recycling, we did not ask people to rehearse reasons for not recycling; thus, we did not explicitly tap negative reasons. In addition, to keep the questionnaire separate from the signs, we did not ask for reactions to the signs or make any reference to the contents of the signs. The initial general question was followed by the same 7-point questions used in Experiment 2. A scale tapping overall convenience of using the crusher was constructed from the mean of three items: (a) time to get to crusher, reverse scored; (b) ease of use of crusher; and (c) convenience to get to crusher (Cronbach's α =.66).

We chose the questionnaire participants randomly over an approximately 2-hr period (each questionnaire was randomly coded 0, 1, or 2, indicating the researcher should select the next person to pass a point, or wait for 1 or 2 people to pass that point before approaching a potential participant). One or two experimenters administered questionnaires in each building during the same time period. We used the same "please help with a class project" appeal as in Experiment 2, and most students agreed or refused without knowing the topic of the questionnaire. Typical reasons for refusing were that the student needed to get to class or work, or needed to study for an exam.

Participation rates were fairly high except in the building where the no validation–weak message signs had been. Counting both direct and indirect refusals (people who accepted but did not return the questionnaire), participation rates in each building were 51% for no validation–"it is the 90's," 81% for validation–"it is the 90's," 71% for no validation–"it important," and 70% for validation–"it is important," $\chi^2(3, N = 328) = 19.36$, $p < .001$. Follow-up tests indicated that the no validation–"it is the 90's" group was significantly below the others (the test compared 51% with 70%, the smallest between group difference, $\chi^2[1, N = 173] = 6.89$, $p < .01$, thereby allowing the inference that this building differed from the other three). A separate analysis indicated that the others did not differ among themselves, $\chi^2(2, N = 229) = 2.22$, $p > .05$. Discussion with the research assistants (RA) suggested that the differential participation rates occurred because one of them had not been as aggressive as the others at asking for help with the project (e.g., the others made more eye contact, thrust the questionnaire at potential participants, & refused to accept no for an answer; whereas the less aggressive RA waited for students to agree to participate). As noted previously, most students refused to participate without knowing the content of the questionnaire and simply said that they were in a hurry or needed to study for an exam. Although the difference is disappointing, if the sample is biased, its impact on the results would be in the direction opposite to predictions. For example, if only students having extensive amounts of time helped in that cell, the bias would have been toward that group taking more time to write out reasons for recycling. In addition, if only students interested in recycling participated, the bias would have been that more students in that cell could describe reasons for recycling and have favorable attitudes toward recycling. That cell was expected to result in little message processing, so these hypothetical biases would hinder rather than support our predictions.

Responses to the open-ended questions were counted by a single rater, with reliability checks provided by four raters. For all coding, condition codes were removed and the conditions mixed together to eliminate possible biases. Definitions were the same as in Experiment 2. A new category was created to accommodate playful or facetious reasons. These tended to make fun of the process ("they need the exercise of walking to the crusher;" "mom said so"), or appeared irrelevant to the issue (e.g., "because of the building's architectural integrity and beauty;" "they hate the random blinking of the cosmos just before it fails"). There were no treatment effects on this measure, these analyses are not reported, and facetious answers were not included in response counts. Reliability coefficients on the number of positive reasons ranged from *r*s of .86 to .97, with an overall reliability of .94. Only one additional rater checked for playful or facetious reasons; there were no disagreements between the main and reliability rater's judgments.

The open-ended reasons for recycling were coded into the strong, external, and weak categories, as in Experiment 2. One person categorized all responses into these three categories, and a second categorized a randomly selected 25%. Kappa coefficients on these ratings were 1.00 for the "strong environmental" reasons, .91 for the "weak environmental" reasons (96% agreement), and .81 for the external reasons (91% agreement).[5]

Results (including reliabilities) are discussed for only participants having a class or office in the building (to assure adequate opportunity for exposure to our signs) and

[5]There was little overlap among these items. Examination of the residual correlation matrix (with "number of reasons" representing the open-ended responses and the 3-item "convenience" scale representing recycling convenience) indicated that *r*s(59) ranged from .00 to .18.

only those who reported drinking sodas at least occasionally (to assure we were considering the cognitions of people actually engaging in the behaviors of not recycling or recycling). Results based on all participants are similar, and are available from the first author. In the reduced sample, 40% of the participants were female.

Memory Test for Signs

One week after the final follow-up period, we revisited the experimental buildings and administered a short quiz to a random sample of students who, when asked, said they previously had a class in the building. We first assured them they would only be asked to answer "two quick questions" so that refusals were unrelated to our issue. We then informed them that we had placed signs on the garbage cans, encouraging aluminum can recycling. We showed them the first part of the sign ("No aluminum cans please!!! Use the recycler located [location given]"), and asked what the final sentence had been. When they had written an answer—including "don't know"—we turned the paper over to show the same question in multiple choice format. All four actual messages were in the list, along with four distractors ("funds support the library," "Support ASUU [student government]," "It's the thing to do,"and "We appreciate your help"). Participation rates were high, although the no validation–"important" building's rate was lower than the others (no validation–"90s" = 97%; no validation–"important" = 74%; validation–"90s" = 94%; validation–"important" = 94%, $\chi^2(3) = 11.48$, $p < .01$).

Results

Carryover From Experiment 2

Overall recycling during the baseline period was 40.4%, or slightly higher than the mean of 37.6% obtained during baseline in Experiment 1, the period before we began using signs to encourage recycling. Building information is provided in Table 4. Random assignment led to one sign (no validation–"important") being used in the same building in both Experiments 2 and 3.

Changes in Recycling Over Time

The purposes of the first analysis were to determine whether the signs increased recycling above baseline, and which changes were maintained after the signs had been removed. A 2 (Validation) × 2 (Message strength) × 3 (Time: Baseline–Signs–Signs removed) factorial ANOVA was undertaken on recycling. There was an overall main effect for time, $F(2, 102) = 31.62$, $MSE = 100.65$, $p < .001$, partial $\eta^2 = .38$. A significant three-way interaction indicated that recycling rates changed differentially over time, $F(2, 102) = 4.63$, $MSE = 100.65$, $p < .01$, partial $\eta^2 = .08$. A priori one-tailed t tests were used within each validation–message group to test for changes between the different periods. As expected, relative to baseline, when signs were in place, all but the validation–weak message ("It may be inconvenient, but it is the 90s") yielded a significant increase in recycling (see Table 4).

With respect to maintaining the change, a comparison of recycling with signs in place and after their removal revealed a pattern almost perfectly consistent with ELM–HSM theorizing. The validation–"important" (scrutinized strong) sign yielded sustained behavior change; in contrast, the no validation–"important" group (no scrutiny, strong) showed the expected significant drop (Table 4, column 2 vs. column 3). Also as expected, the validation–"90s" group (scrutinized weak) never improved. The single surprise was that the no validation–"90s" group maintained its behavior change during the follow-up period.

TABLE 4
Impact of Clinical Validation on Recycling Behavior, Experiment 3

	Baseline (2 Weeks)	*Signs (3 Weeks)*	*Signs Removed (1 week, after 1 week delay)*
No validation (no expected scrutiny)			
Weak "90's"	40%	$60\%_a$	$61\%_a$ ($n = 11$)[a]
Strong "important"	37%	$56\%_a$	$49\%_b$ ($n = 20$)[b]
Validation (expect scrutiny)			
Weak "90's"	41%	44%	44% ($n = 16$)[c]
Strong "important"	43%	$61\%_a$	$60\%_a$ ($n = 8$)[d]

Note. Data are percentage of cans recycled, unadjusted scores. Significant changes between baseline and signs, and between signs and signs removed are indicated by subscripts within each row, $p < .05$. Means with a common subscript do not differ.

[a]Condition in E2: validation–"important". [b]Condition in E2: no validation–"important." [c]Condition in E2: no validation–"30 seconds." [d]Condition in E2: validation–"30 seconds"; this was the "inconvenient" building from Experiment 1, a situation we could not change for this experiment.

Treatment Effects: Recycling, Message Scrutiny, and Cognitive Elaboration

Treatments and recycling with signs in place. Because of significant differences in recycling during baseline, a covariance analysis was used to compare the different messages. The covariate contributed significant variance, $F(1, 50) = 61.34$, $MSE = 253.68$, $p < .001$, partial $\eta^2 = .55$, and was homogeneous across the different treatment conditions, both time 2 and time 3 covariate by treatment interaction Fs $(3, 47) \leq .97$, MSEs ≤ 242.70, ps $> .05$, partial η^2s $\leq .06$. None of the effects involving time (signs in place vs. signs removed) was significant. Instead, a significant overall validation by message interaction emerged, $F(1, 50) = 10.65$, $MSE = 253.68$, $p < .002$, partial $\eta^2 = .18$. However, to test for hypothesized effects separately for signs and follow-up (treatment differences with signs in place, and the differential follow-through), a priori t tests for disparate ns were used within each time period.

When signs were in place, recycling rates adjusted for the covariate (baseline) were consistent with ELM and HSM research on peripheral versus central route influence (Petty & Cacioppo, 1979b, 1984). Absent validation, the weak (59.7%) and strong (57.9%) message groups did not differ in recycling levels, $p > .05$. In contrast and as expected under central route processing, the two validation groups differed, with the strong message condition yielding significantly higher recycling (58%) than the weak message group (43%), $p < .05$.

Questionnaire

The questionnaire measuring attitudes and cognitive responses was administered in between the treatment and follow-up periods, early during the 1-week delay. This placed it close to the previous week, and it is probably a better indicator of thoughts during the final treatment week than of the subsequent follow-up period. As another caveat, it is more accurate to compare cognitions with the raw recycling rates because those are the behaviors students would explain. In this experiment, the adjustments for the covariate are minimal and do not change the patterns of results, so for convenience we discuss cognitions in the context of the adjusted scores.

Cognitive elaboration. The first two hypotheses addressed cognitive responses and proposed that behavioral change induced via a peripheral route would be less bolstered by supportive cognitions; whereas behaviors resulting via a central route with thoughtful analyses of the message would have more bolstering cognitions. These hypotheses received considerable support, number of reasons, interaction, $F(1, 81) = 7.01$, $MSE = 1.49$, $p < .01$, partial $\eta^2 = .08$, and percentage giving any reason, interaction $F(1, 81) = 4.12$, $MSE = 0.22$, $p < .05$, partial $\eta^2 = .05$. Consistent with the recycling data, students who had seen the validation–"important" message for several weeks provided more positive reasons ($M = 2.00$, $n = 15$) and had a higher percentage giving at least one positive reason for recycling (87%) than its validation–"90s" counterpart ($M = 1.09$, $n = 23$; 61%, ps $< .05$). The two no validation groups differed in the number of reasons provided (weak, $M = 1.53$, $n = 19$; strong, $M = 1.00$, $n = 28$, $p < .05$) but not in the proportion providing any reasons (weak, 74%; strong, 57%, $p > .05$).

Although we thought the validation–"important" sign might stimulate more serious thinking about recycling, analysis of the contents of students' reasons for recycling yielded no interaction among type, validation, and message strength, $F(2, 162) = 2.02$, $MSE = 0.20$, $p > .05$, partial $\eta^2 = .02$. Instead, a single significant main effect for type indicated that in all four sign conditions, only a small percentage of participants provided strong reasons (15%), slightly more than one-third provided external reasons (37%), and almost half provided simple environmental reasons, such as "for the environment" (45%), $F(2, 162) = 8.25$, $MSE = 0.20$, $p < .001$, partial $\eta^2 = .09$.

Attitudes. The attitude measure should parallel the cognitive responses. Analyses revealed a marginally significant interaction between validation and message strength, interaction $F(1, 79) = 3.19$, $MSE = 2.56$, $p < .08$, partial $\eta^2 = .04$. An a priori, one-tailed, protected t test with α set at .10 indicated the validation–strong group was more favorable toward recycling ($M = 5.93$, $n = 15$, on a 7-point scale) than the validation–weak group ($M = 5.23$, $n = 22$). Attitudes of the two no validation groups did not differ (weak, $M = 5.95$, $n = 19$; strong, $M = 5.37$, $n = 27$, $p > .10$).

Emergent recycling ethos. Participants' estimates of what percentage of people in their building recycled aluminum cans ranged from 29% to 46%. A main effect for message indicated that people in the buildings with the "it is the 90's" signs believed there was more recycling in their buildings (44% of the people were thought to recycle, $n = 39$) compared to participants in buildings with the "it is important" sign (36%, $n = 34$), $F(1, 69) = 3.88$, $MSE = 388.15$, $p < .05$, partial $\eta^2 = .05$. These perceptions were not consistent with actual recycling rates or with respondents' other attitudes and cognitions toward recycling. There were no other significant effects, validation and message by validation Fs $(1, 69) \leq 1.91$, $MSE = 388.15$, ps $> .10$, partial η^2s $\leq .03$.

Perceived inconvenience. There was no evidence that validating students' complaints about inconvenience increased their perceptions of inconvenience. There were no significant effects on the scale constructed from items tap-

ping how long it took to get to the can crusher, how convenient it was to get there, or how easy it was to use the crusher, the main and interactive Fs(1, 74) ≤ 0.77, MSE = 2.73, ps > .05, partial η^2s ≤ .01.

Recycling: Treatment Effects After Signs had Been Removed

During follow-up, after the signs had been removed for 1 week, treatments yielded both expected and unexpected results. The analyses of adjusted means essentially echo the repeated measures analysis reported previously. Consistent with ELM- and HSM-based theorizing, the group with the strongest cognitions maintained a high recycling level. That is, the validation–"important" sign was hypothesized to elicit the most positive scrutiny and indeed provided more cognitions on the questionnaire. During follow-up, that group recycled more than its counterpart, the validation–"it is the 90s" group (57% vs. 42%, $p < .05$).

The no validation–"it is the 90s" group also recycled at a high rate after the signs had been removed (61%). They recycled significantly more than their counterpart, the no validation–"important" message group (51%, $p < .05$) and at the same rate as the validation–"it is important" group (57%). Thus, although that sign was expected to produce only short-term recycling, it appears to have produced sustained recycling, an intriguing puzzle we address in the discussion.

Memory for Signs

The memory test administered 1 week after the follow-up recycling period indicated that people had little memory for details of our signs. In the recall test of 114 interviewees, only 1 stated that building's sign correctly, 4 wrote partially correct answers, and the rest wrote that they did not know (although many said spontaneously they remembered that there had been signs). These data were not analyzed.

Even in the easier multiple choice format, only 11% selected the correct sign for their building, and most selected one of the distractor answers ("We appreciate your help" received most endorsements, 31, or 27%). There was an interesting pattern, in that the signs with the highest recycling rates during follow-up produced the highest percentages of correct answers: no validation–"90s"had 14% correct; validation–"important" had 17% correct; and the two remaining groups each had 7% correct; interaction $F(1, 110) = 2.12$, $MSE = 0.10$, $p < .15$, partial $\eta^2 = .02$. Although it makes sense that the more highly scrutinized sign would yield better recognition memory, cognitive response researchers do not require such an outcome. They propose instead that people have better memories for their own thoughts about a message than about the message itself (Greenwald, 1968; Petty & Cacioppo, 1984). Thus, the low rates of accuracy in the memory test are acceptable within ELM theory. The differential accuracy is intriguing because it suggests that different levels of scrutiny did occur.

Discussion

The total pattern of results provides support for ELM–HSM and validation-derived hypotheses, whether considering unadjusted recycling rates over time, treatment effects adjusted for baseline, or behaviors after signs had been removed. The general pattern is clear, and supports hypotheses that validation would increase message scrutiny that would stimulate behavior when coupled with a strong message ("it is important"), but yield rejection when coupled with a weak message ("it is the 90s"). Indeed, with respect to the validation–weak message, recycling (unadjusted) never exceeded baseline levels. Consistent with hypotheses, compared to the validation–weak message group, the validation–strong group provided more cognitions in support of its behavior, had a higher proportion of respondents providing a reason, and maintained a high level of recycling after signs were removed—even though the recycler was in an inconvenient location. They also had stronger attitudes and better recognition memory for the sign, although these effects were marginally significant. The results support theorizing that cognitive elaboration of a message makes the attitude and its bolstering cognitions more accessible, and this accessibility increases the correspondence between attitudes and behavior even after the message has been removed (Petty et al., 1995; Rennier, 1988).

An intriguing pattern emerged in the nonvalidated–weak message condition. As expected under peripheral route influence, this group increased its recycling when the signs were in place. However, consistent with central route processing, they provided more reasons for recycling, maintained recycling during the follow-up, and had relatively good memory for the posted signs. This group also had the most positive attitudes, although this measure was not significantly higher than the comparison group (the no validation–strong message group).

One explanation for the behavioral maintenance in this group is that some students spontaneously scrutinized the nonvalidated–"90s" message and were able to generate a serious interpretation. Indeed, recall that 61% of the prescaling students who focused on the "90s" sign had generated a serious interpretation, especially that "we are running out of time to preserve resources." In contrast to the validated–weak message, which invited scrutiny and led to rejection, this gradual, spontaneous scrutiny may have led to serious interpretations, especially if students already favored recycling. The relatively high number of reasons for recycling and better memory for the sign 3 weeks after

signs had been removed supports our speculation that some students had spontaneously scrutinized the weak message. If they figured out a serious interpretation, it could have led to its more enduring impact. Possibly, had we measured cognitions late in the follow-up period, we would have detected this effect in the content analysis. The intriguing pattern of results has led us to consider using "provocative" signs as a strategy for increasing elaboration in future studies, much as rhetorical questions can increase elaboration (Petty, Cacioppo, & Heesacker, 1981). As a final point, this group had the lowest survey participation rate, a potential source of bias. However, the correspondence between recycling rates and cognitive measures on the questionnaire mitigates concerns about a biased sample in this building. Their participation rate in the follow-up memory test was high and similar to the others, reducing concerns about bias for that measure.

GENERAL DISCUSSION

Across the 3 experiments, one effect is quite robust: Effective signs increased recycling over baseline. The single exception was in Experiment 2, when the increase was marginally significant, possibly because the high base rate made a significant increase difficult to achieve. The first experiment also suggests that effective signs can encourage recycling despite some inconvenience, thereby potentially reducing the costs of recycling programs.

Another pattern that emerged is that validation appeared to have the desired impact of increasing message scrutiny. In Experiment 2, the validation–"important" message group generated more reasons for recycling than the other groups. In Experiment 3, that same sign again yielded a higher level of cognitive elaboration than its validated–weak counterpart. This strategy may be an important complement to other strategies known to increase cognitive elaboration, especially in field research, where more traditional methods cannot be implemented or would be impractical (although a sign highlighting the reader's vested interest might suffice [Crano, 1995] as might a sign using a rhetorical questions [Petty, Cacioppo, & Heesacker, 1981]). Along these lines, it is useful that validating complaints of inconvenience did not raise perceptions of inconvenience, nor did it lead people to downplay the levels of inconvenience. Perceived lack of convenience may have been a barrier that people were willing to get around once they elaborated the prorecycling message, and made salient and accessible their own preexisting prorecycling attitudes. We imagine a minibattle played out in students' heads, in which they eventually convince themselves that it's more important to recycle than to worry about inconvenience. This minibattle was stimulated by a sign that acknowledged their complaint but encouraged them to think about it.

Future research is needed to articulate exactly why validation might have this effect. For example, does it reduce reactance and restore the recipient's sense of control (Brehm & Brehm, 1981), set up a demand for reciprocal listening (cf. Cialdini, Green, & Rush, 1992), increase liking for the communicator, or have some other impact? If it can have such effects, validation is akin to Cialdini's (1993) jujitsu maneuvers. Rather than confronting people with stronger and stronger persuasive messages, validation may "open the door" and increase their exposure to and thoughts about the message. If the message is inherently persuasive and appropriate for the situation, it should have more influence on attitudes and possibly increase the connection between attitudes and behaviors over the long-term. In addition, as we stated at the outset, these signs may be most effective in cases where attitudes are already generally positive. In this milieu, simple signs can strengthen attitudes, make them a little more positive, and increase their accessibility in ways that may not occur if recipients are actively hostile toward the message. In theory, the validation technique should be effective with a hostile audience, but additional effort may be required for effectiveness with such a group (e.g., a gradual increase in persuasiveness, more comprehensive validation, changing the physical appearance of the signs to renew attention, etc.).

We have argued that saying "it may be inconvenient" is essentially the equivalent of clinical validation, and this validation is what produced the increased scrutiny and receptivity to the signs. ELM and HSM researchers state that persons need both the motivation and ability to scrutinize messages. However, we have not specified whether validation influences motivation, ability, or both, and future research is needed to address this. Future research may also address an alternative reason for validation's effectiveness in this research: The validating message may have appeared to be more polite. So it is not clinical validation but rather greater social appropriateness that elicited more attention and thought, and greater adherence to the sign's request. Although we agree this is an intriguing possibility, we expect that the pattern of results would be different if students are simply more receptive to polite signs. If polite signs make people more helpful or put them in more friendly and receptive moods, there should be no interaction (no decrement in the validation–weak message group), but instead simple main effects for the validated conditions. It would be interesting to compare truly polite signs (varying such words as "please," "thank you," and "we appreciate your help") with validating signs (acknowledgment of primary counterarguments) to see if politeness can also increase message scrutiny.

Validating someone's initial concerns is similar to presenting a two-sided argument, and as such raises the question of how much negativity one can validate before undermining one's own persuasive message. For example, in our particular message, we pointed out the inconvenience but never attempted to undermine generally accepted reasons for recycling. Had we validated a common

urban legend about recycling that "this material may not actually be reused, but it's important to try anyhow," we might have undermined our persuasive intent. Thus, any negative arguments acknowledged with a validation would need to be offset by more positive or more heavily weighted arguments generated by the recipient or provided by the communicator. Indeed, this is exactly the strategy used by clinicians in their validation therapy. For example, family therapists only legitimate their clients' affective reactions, not their cognitive interpretations (Alexander & Parsons, 1982). Similarly, Kraus and Redman (1986) implicitly suggest to clients that although their reasons for being depressed are legitimate, those reasons are less important and less serious than the negative consequences of their depression. Similarly, no matter how nice and validating a customer service representative is, if a serious problem cannot be remediated, the customer will probably not be fully satisfied. Furthermore, it may be important to use validation early on, but over time, begin to minimize the significance of the initial complaint in concert with its reduced relevance to those in the setting. Future research could define the parameters of effective validation and demonstrate the underlying attitudinal processes involved.

There are several caveats involving this line of research. First, as noted earlier, treatment is confounded with building in every experiment, and there may be dependencies among the classrooms (students with multiple classes in the same building; observations of others' behaviors). Although we used several methodological techniques for reducing the severity of this problem (random assignment to building, use of baseline data, evidence in every building of a wide variety of course topics and by implication types of students, mostly similar refusal rates in the questionnaire studies), we would have preferred to use several buildings per treatment, but suitable buildings were not available. Another problem is that aluminum cans were left in the wastebaskets, and could potentially have contributed to treatment effects (visible cans could have induced more people to discard rather than recycle; lack of visible cans could have induced more to recycle). We have no record of how visible the cans were in the trash container (we know they were sometimes visible & sometimes hidden by other garbage), but this would be useful data to collect in future research. In addition, of course, we have no measures of the extent to which seeing cans or no cans influenced others' behavior, although that would also be a useful experiment. Another problem of field research is that we have no control over exposure to the sign, whereas in a laboratory, one knows that a participant has read a message. This is simply not the case in field research, and we must hope that initial reading rates were similar across the buildings (although of course, we expected differential message scrutiny). Although we used strategies for drawing representative samples and focused on participants who spent time in the building (Experiments 2 & 3) and used aluminum cans (Experiment 3), we still needed to infer a connection between questionnaire responses and behavior. On the other hand, there is a long and distinguished literature on the need to use both laboratory and field research to gain true understandings of behavior. So we are not apologizing for our choice of methodology, but simply acknowledging the need for future research in both field and laboratory that clarifies and extends our findings.

In conclusion, this research has contributed to theoretical perspectives on persuasion and added to the growing body of literature on recycling. The experiments show that simple, well-designed signs can influence behavior while the signs are in place as well as after their removal, as long as people are generally favorable toward the behavior. It also shows the utility of combining ideas about validation, scrutiny, and accessibility to studying attitude and behavior processes. It contributes to the body of research aimed at understanding recycling and other positive environmental behaviors (Burn & Oskamp, 1986; DeYoung, 1993; Guagano, Stern, & Dietz, 1995; Stern & Oskamp, 1987), being unusual in using persuasive messages rather than prompts or simple instructional signs. It supports the idea that there is no simple trick for changing behaviors, but that messages from multiple sources are needed (Werner, 1999; Werner & Adams, 2001; Werner, Rhodes, & Partain, 1998). That is, the high attitude scores suggest that these students came to the situation with prorecycling attitudes, learned in another setting, such as in high school, at home, or in the media. Our signs served to make these attitudes more salient and—after elaboration—more accessible. The relative convenience of the bins contributed an additional source of motivation to recycle. We suspect that, to be optimally effective, behavior change interventions need to address these multiple levels.

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