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Validation, Persuasion and Recycling: Capitalizing on the Social Ecology of Newspaper Use

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Two experiments used clinical validation to increase scrutiny of messages posted in public settings. The first experiment used a 2 (validation: no/yes) × 2 (persuasion: none/"it is important") factorial design to develop messages about newspaper recycling. The prompt (no validation/no persuasion) had little impact on newspaper recycling, but the other 3 signs all resulted in increased recycling while the signs were in place. After signs were removed, recycling remained significantly higher than baseline (marginally significant for validation/persuasion). At the end of the study, number of cognitions favoring recycling was significantly higher in the validation-only condition and was lower in the validation/persuasion condition. Experiment 2 tested whether signs would have more impact if they were sensitive to the social ecology of newspaper recycling; that is, readers' tendency to leave newspapers behind so others may read them. Messages based on a 2 (message: validation-only/validation plus persuasion) × 2 (ecology: no social ecology/social ecology) factorial design supported social ecology's importance. Furthermore, because of the increased sharing in the social ecology conditions, total numbers of newspapers used was significantly lower in those buildings. Validation only yielded sustained behavior change in both Experiments 1 and 2, supporting additional research on the question of whether validation can, by itself, lead to cognitive elaboration and self-persuasion.

Both Petty and Cacioppo's (1986a, 1986b) elaboration likelihood model (ELM) and Chaiken's (1980, 1987) heuristic-systematic model (HSM) have inspired numerous experiments showing that people use two general strategies for responding to persuasive information. One route is quick and automatic, is based on cues rather than careful thinking about a message, and rarely leads to long-term attitude or behavior change. The other route is based on a more careful examination of ideas, and results in positive (agreement) or negative (disagreement) elaboration of a message, with positive elaboration more likely to lead to long-term attitude and behavior change. Although there are several strategies for inducing message scrutiny in laboratory settings, it can be difficult to induce such close examination in other settings. We describe two experiments that borrow a technique developed by clinical psychologists to increase receptivity to new information. Validation is used in therapy settings as a way to reduce a client's defensiveness and reactance. The clinician acknowledges or "validates" pain, suffering, and unhappiness without agreeing with the client's interpretation of or explanation for the discomfort. Validation is thought to allow clients to feel heard or understood, thereby opening them to the therapist's point of view (Alexander & Parsons, 1982; Coates & Wortman, 1980; Coyne, 1985, p. 344; Rogers, 1951; see Werner, Stoll, Birch, & White, 2002, for review).

Werner et al. (2002) used this idea to increase processing of recycling messages. Although signs have been used to increase recycling in a variety of settings (Geller, Winett, & Everett, 1982; Werner, Rhodes, & Partain, 1998), this approach is unusual in applying dual pathways thinking to recycling. Commitment manipulations (Burn & Oskamp, 1986; Katzev & Wang, 1994; Wang & Katzev, 1990), and "block leaders" who organize neighbors to recycle (Burn, 1991; Nielsen & Ellington, 1983) have been very effective at increasing recycling levels, but these interventions are labor intensive and suitable primarily for groups of friends and neighbors, for whom implicit social pressures can contribute to behavioral change and maintenance. Dual pathway approaches hold promise for public settings where large groups of strangers can be exposed to written messages.

To apply validation to recycling, Werner et al. (2002) first learned that the dominant complaint about recycling was its in-

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convenience. We developed instructional signs that allowed us to manipulate whether this complaint was validated or not, and then we used a classic dual pathways experimental design (Experiment 3) to evaluate whether validation increased message scrutiny. All signs began with a simple request to recycle plus the recycling bin's location. At the bottom of the sign was the manipulation of validation (no validation vs. "It may be inconvenient, but ...") and the standard way of determining whether people had scrutinized the message, a weak ("It is the 90s") versus a strong ("It is important") message (e.g., White & Harkins, 1994). Consistent with laboratory research, when signs did not contain validation, people did not seem to pay attention to message content: Weak and strong messages yielded equal rates of recycling. However, signs that validated the inconvenience of recycling appeared to lead to scrutiny: The weak message was ineffective, but the strong message led to enhanced recycling. The experiment also demonstrated that—relative to the scrutinized weak message—the scrutinized strong message led to long-term recycling (sustained recycling 2 weeks after signs were removed), more prorecycling cognitions, a higher proportion of respondents providing any cognitions, and—at marginally significant levels—better memory for the sign and more favorable attitudes toward recycling. Thus, as suggested by clinical practice, validation appeared to provide an additional way to induce readers to scrutinize a persuasive message. The combination of sustained behavior change, pro-attitude cognitions, and initially favorable attitudes with relatively low attitude change suggests that these signs had primarily served to increase attitude accessibility (Fazio, 1986, 1990).

According to clinical observations, validation might increase receptivity even if the communicator does not make detailed arguments in support of behavior change (Kraus & Redman, 1986). This might occur in clinical settings because of the perceived authority of the clinician, the implicit expectations of change inherent in a therapeutic relationship, or the opportunity for self-reflection and self-persuasion, among other processes. These possibilities are intriguing because they suggest that validation by itself might lead to cooperation with a sign's request. The first two possibilities suggest compliance without attitude change (Cialdini, 2001), but the third suggests self-induced attitude change or increased attitude accessibility (Fazio, 1986, 1990). Thus, validation alone (validation/no persuasion) might have two possible effects. It might lead to mindless cooperation—recycling when the validated request is in place but no enhanced cognitive elaboration and no recycling once the sign has been removed. Alternatively, validation alone might invite self-reflection, which would stimulate thinking about the request to recycle. That is, participants reading a validation-only sign would think about their recycling behavior and develop their own reasons for recycling (cf. Tesser, Martin, & Mendolia, 1995, on mere thought and attitude polarization). Based on our previous research in this setting, we would expect most people reading the sign to have positive attitudes toward recycling, and therefore to elaborate in a positive way. To this end we conducted Experiment 1 in which validation alone was compared to validation coupled with a persuasive message. Experiment 2 enhanced the messages by adding support for the social ecology of recycling. In both experiments, we used a persuasive message from Werner et al. (2002), which simply says it is important to recycle and does not give any detailed explanation.

EXPERIMENT 1

Experiment 1 used a 2 (validation: no/yes) × 2 (persuasive message: no/yes) factorial design to develop content for four different signs. If validation serves only to increase scrutiny of a message, the following pattern should occur. The prompt (neither persuasion nor validation, just the instruction to recycle) should yield the least recycling immediately, no elevated prorecycling cognitions, and no recycling after the signs are removed. The nonvalidated (i.e., nonscrutinized) persuasive "it is important" message should produce mindless, short-term cooperation, with few supportive cognitions. The validation-only sign would attract interest and be scrutinized, but because there is no persuasive message, would have only short-term impact. Finally, the validation/persuasion sign used in previous research should produce both short- and long-term changes in behavior as well as enhanced cognitive elaboration and attitude change. The alternative, if validation produces scrutiny as well as self-reflection and self-persuasion, is the preceding pattern except that both the validation-only and the validation/persuasion signs should produce short- and long-term changes in behavior, attitudes, and accompanying positive cognitions.

Method

Overview

The procedures replicated those of Werner et al. (2002), except that we targeted newspapers instead of aluminum cans. In five preselected university buildings, newspaper recycling data were gathered daily for an 8-week period. After the first 3 weeks, signs encouraging recycling were placed on all wastebaskets in classrooms and the adjacent hallways. After 3 weeks with the signs in place, signs were removed and observations continued for the remaining 2 weeks. Questionnaires measuring attitudes and cognitive elaboration were gathered during Weeks 1 (during baseline) and 8 (during follow-up). A separate, no-signs control building was used to assure changes were due to our signs rather than extraneous circumstances.

Selection of Buildings

Consistent with Werner et al. (2002), we selected five buildings to serve as our research sites. Criteria for inclusion were that the building had: (a) a minimum of six classrooms in use; (b) a newspaper rack that provided free *Utah Chronicles* daily

(the university's student-produced newspaper); (c) a newspaper recycling bin; (d) no other ongoing newspaper recycling program; (e) evidence of widespread newspaper use; and (f) a variety of course topics being taught so that our manipulated treatments would not be confounded with type of course or particular student major. The study was conducted during the autumn quarter, between the months of October and December. As in previous research, experimental conditions were randomly assigned to the four buildings, recycling bins were located to achieve moderate levels of convenience (on an exit route for about half the students), and basic messages on the signs were the same. The control building fulfilled most of the criteria (and had been used in our previous research) except that there had been no newspaper recycling bin in the building. A bin was added just before the baseline period, and it was designated the control building.

Treatments and Experimental Design

The 2 (validation: no/yes) × 2 (persuasion: none/"it is important") factorial design yielded four different signs or treatment conditions. The signs contained minimal information so they could be read quickly. The top line was a request not to discard newspapers, and the second gave the location of the recycling bin in that building. Along the bottom was the message created by the 2 × 2 factorial design. In all conditions, the signs were taped to all the garbage cans in the building's classrooms and hallways (although not the office areas). In addition, we posted reminder signs at the front of the classrooms, asking people to recycle their newspapers. The complete design was a 3 (phase: baseline/signs/follow-up) × 2 (validation) × 2 (persuasion) factorial; follow-up data are from a 1-week period after a 1-week delay.

The signs were the same as in previous research except that we rephrased the validation so that the validation-only condition made sense. Instead of "It may be inconvenient," which sounds like a warning, we used "We are sorry for the inconvenience." This form validates students' complaints about inconvenience and conveys empathy without explicitly accepting responsibility for the problem. Therefore, although we changed the wording from the previous version, it was necessary, and the new form is a conceptual replication of the first.[1]

Dependent Measures

Recycling. Counts were made of the total number of university newspapers found in the classroom areas (left lying around, found in garbage cans, and in the recycling bin); nonuniversity newspapers were seen rarely and were ignored. To enhance the experiment's ecological validity, any discarded newspapers were left in wastebaskets; wastebaskets and classrooms were cleaned by custodians daily. In each building, next to the stand where people picked up the free campus newspaper, a bin was clearly marked as a newspaper recycling bin. Reliability checks on newspaper counts on randomly selected days across the study yielded a reliability coefficient, simple $r(27)$, of .91.

The newspaper data were converted to a percentage, indicating the percentage of newspapers used in that room or hallway that had been recycled (for convenience, hallways were treated as one room). To compute the percentage, we had to estimate how many of the recycled newspapers had been used in each room. We did this simply by allocating the recycled newspapers equally to each classroom. Thus, each classroom was the unit of analysis and the number of classrooms in each building was the n for each treatment condition. This strategy was chosen instead of treating the whole building as the unit of analysis for several interrelated reasons. The first is that classrooms are the natural social setting, with classes meeting regularly and containing the same students, and many people regularly reading the newspaper before or after class. Second, this approach is conservative, requiring that the persuasive signs have similar impacts in multiple settings, rather than a strong impact in only part of the building that could inflate the overall building's proportion of newspapers recycled. Third, this approach provides a check on social pressures or modeling of recycling. If these social processes contributed to recycling rates, they must have done so in every room to impact the building's mean recycling rate. Finally, this approach provides some control for problems of different class sizes, especially if a large class has majors who are particularly concerned about recycling, they are not given more weight than a small class with different concerns (and by implication, different recycling behaviors). The experiment would be better with more buildings per treatment condition, but additional buildings were not available.

Questionnaires. The purpose of the questionnaire was to provide insights about the cognitions and attitudes of newspaper readers. We drew a large random sample of participants, and then limited data analyses to those with a class or office in the building (so they would be exposed frequently to our signs) and who read the student paper at least occasionally (so they would have an opportunity to engage in recycling, the behavior of interest). This strategy is much like a random telephone survey that screens for people who watch television, and then measures their memories of and attitudes toward certain television advertisements. The sample is thought to generalize fairly well to the total population of viewers, or in this case, potential newspaper recyclers.

The baseline questionnaire was administered during the first week of baseline recycling, and the follow-up was administered during the same week as the follow-up recycling data. We chose our questionnaire samples randomly over an approximately 3-hr period on 2 or 3 days per building. Each questionnaire was randomly coded 0, 1, or 2, indicating the researcher should select the next person to pass a specified point, or wait for 1 or 2 people to pass that point before ap-

[1]We thank James F. Alexander for his help in devising this new form.

proaching a potential participant. Five different experimenters administered questionnaires; experimenters rotated among the buildings to randomize any effects due to experimenter. We used the request from earlier research, that the participant "help with a class project." By emphasizing this was a class project, administering questionnaires in the morning (whereas recycling data were gathered in the afternoon), and not mentioning the instructional signs, we aimed to separate the recycling signs from the questionnaires.

Most students agreed or refused without knowing the topic of the questionnaire, reducing self-selection bias (if students asked the topic, experimenters said "it's for an environment and behavior class"). Typical reasons for refusing were that the student was late for class or work or needed to study for an exam. Table 1 shows the participation rates for each condition, including the unsigned control building. Although there were differences among participation rates during the premeasure, $\chi^2(4, N = 1{,}077) = 14.87, p < .05$—without the control building, $\chi^2(3, N = 910) = 10.96, p < .05$—these were eliminated during the follow-up, except in the control building, $\chi^2(4, N = 972) = 13.09, p < .05$—without the control building, $\chi^2(3, N = 821) = 2.95, p > .10$. Experimenters who had accepted "no" too readily at Time 1 were retrained or replaced at Time 2. Time period is a between- rather than within-participants factor; we used random samples to estimate population attitudes and did not measure actual change.

TABLE 1
Attitudes and Cognitive Elaboration, Experiment 1 at Baseline (B) and Follow-up (F)

Persuasive Message	*None*		*"It is important"*		
Validation	*No*	*Yes*	*No*	*Yes*	*Control*
Survey participation rate					
B	45%	58%	51%	58%	55%
F	52%	57%	59%	59%	74%
Recycling (unadjusted)					
B	48%	41%	37%	54%	23%
F	53%	50%	54%	60%	26%
Difference	**+5%**	**+9%****	**+17%****	**+6%***	**+3%**
Positive attitudes					
B	6.1	6.4	6.2	6.4	6.3
F	6.2	6.0	6.2	6.2	6.2
Cognitive elaboration					
Number of reasons					
B	.83	.77	.68	.82	.90
F	$.99_a$	1.09_a	$.74_b$	$.60_b$	.98
Difference	**+.16**	**+.32***	**+.06**	**−.22***	**+.08**
Gave any reason					
B	$52\%_{ab}$	$45\%_{ab}$	$43\%_{ac}$	$56\%_{bc}$	56%
F	$66\%_a$	$71\%_a$	$45\%_b$	$44\%_b$	62%
Difference	**+14%****	**+26%****	**+2%**	**−12%***	**+6%**
Type of reason					
Specific environmental					
B	16%	16%	18%	22%	20%
F	10%	15%	16%	11%	16%
Difference	**−6%**	**−1%**	**−2%**	**−11%**	**−4%**
External					
B	30%	27%	25%	23%	30%
F	$40\%_a$	$39\%_a$	$20\%_b$	$19\%_b$	35%
Difference	**+10%***	**+12%****	**−5%**	**−4%**	**+5%**
Vague environmental					
B	30%	30%	21%	28%	38%
F	$38\%_a$	$37\%_a$	$26\%_b$	$25\%_b$	30%
Difference	**+8%**	**+7%**	**+5%**	**−3%**	**−8%**

Note. Within each row, subscripts indicate which means differ by a priori *t* test, two-tailed, at $p < .05$. Means with common subscripts do not differ. Differences (in bold) are change scores between follow-up and baseline (positive numbers indicate follow-up was higher than baseline). For these rows, an asterisk or double asterisk indicates that the change (F−B) is different by a priori two-tailed *t* test.

*$p < .10$. **$p < .05$.

The questionnaires began with an open-ended question to tap cognitive elaborations: "Why do people recycle student newspapers in this building? List or describe as many reasons as you can. Use the back side of this sheet if necessary." This question form was used so that even people who did not recycle would have an opportunity to provide cognitions (in contrast to asking "why do you recycle newspapers"). Two questions measured attitudes toward recycling and were averaged prior to analyses ("How important is it to recycle newspapers?" and "How favorable are you, overall, to newspaper recycling on campus?" measured on 7-point scales; Cronbach's α at Time 1 = .74, at Time 2 = .71). Two items measured perceived inconvenience (time to get to the newspaper recycling bin, and convenience of getting to the bin). They did not form a scale and were analyzed separately (Cronbach's α at Time 1 = .30 and Time 2 = .39); also analyzed separately was whether they knew the recycling bin's location (no/yes). One item asked how frequently respondents read the student newspaper so that we could focus on readers, and one question tapped their perception of how many people recycled newspapers to estimate whether newspaper recycling appeared to be equally socially desirable in all five buildings.[2] Except as noted, responses were made on 7-point scales with endpoints labeled *extremely* [positive/negative end-label], *always*, and similar extreme terms. Two items were worded negatively to avoid response biases, but were reverse scored for data presentation. At Time 1, reduced $n = 442$, at Time 2, reduced $n = 414$, although at both Times 1 and 2, item *n*s are variable due to missing data.

Participants' responses to the open-ended question were counted by a primary rater who counted the number of different reasons for recycling. A second rater counted a random

[2]Polystyrene recycling controversy. An additional item was included in the follow-up questionnaire to determine whether students were aware of a controversy over polystyrene recycling, and whether that controversy had led them to decrease or increase their newspaper recycling. During the term, students discovered that their carefully recycled polystyrene had been going to the landfill because the market for recycled polystyrene had dropped. Fifty-two percent had not heard about the controversy. Of those aware of the problem, many (47%) said that it made them increase their newspaper recycling, 26% said that it led them to decrease their newspaper recycling, and the rest reported that the controversy had made no difference in their recycling (28%). There were no treatments effects on this measure, indicating these attitudes were approximately equally distributed among the buildings.

10% of all questionnaires, and a reliability check on the number of reasons indicated high agreement: Time 1 $r(106) = .99$, Time 2 $r(54) = .97$. The answers were also coded into three categories: specific environmental, or detailed knowledge about the environmental benefits of recycling (e.g., reuses resources, reduces harvesting of trees, saves energy, and extends the life of the landfill); vague environmental, or general statements that recycling benefitted the environment (helps the environment, good for the environment); and external reasons (social pressure, money, to keep the building clean). For these three categories at Time 1 ($n = 113$), kappa coefficients ranged from .91 (96% agreement) to .93 (97% agreement), and for Time 2, from .90 (95% agreement, $n = 159$) to .96 (99% agreement, $n = 144$).

Data analysis. Particular analytic procedures are described as results are presented. Several of the questionnaire items were dichotomous (know bin's location; "gave any reason" for recycling; and type of reason given). For statistical power, these were analyzed with an analysis of variance (ANOVA); work by Lunney (1970) indicated that, with large sample sizes and reasonably large p and q values, the binomial distribution approaches normality and F tests are unbiased.

Results

Newspaper Recycling

Recycling changes over time. The purposes of the first analysis were to determine whether the signs increased recycling above baseline and whether any changes were maintained after the signs were removed. A 3 (phase: baseline/signs/signs removed) × 2 (validation) × 2 (persuasion) between- and within-subjects ANOVA yielded an overall main effect for phase, $F(2, 100) = 20.60$, $MSE = 65.64$, $p < .001$, partial $\eta^2 = .29$, and an interaction between phase and persuasion, $F(2, 100) = 4.54$, $MSE = 65.64$, $p < .013$, partial $\eta^2 = .083$, both of which were qualified by a significant three-way interaction among phase, persuasion, and validation, which indicated that the different treatment groups yielded different patterns over time, $F(2, 100) = 3.48$, $MSE = 65.64$, $p < .035$, partial $\eta^2 = .065$. A priori t tests are used for specific comparisons within each persuasion/validation group to test for hypothesized changes (one-tailed from baseline to signs for hypothesized increases, and two-tailed for baseline to follow-up; Bruning & Kintz, 1987).

The first question is whether the signs produced increased recycling relative to baseline. As the upper half of Table 2 indicates, recycling increased significantly in all but the prompt condition (no persuasion/no validation). Thus, as in previous research, effective signs can influence recycling behavior. The second question is whether recycling levels were higher than baseline during the follow-up period, after the signs had been removed for 1 week. As the upper half of Table 2 indicates, two groups achieved this standard: validation only and no validation/persuasion. The other two—persuasion/validation and prompt—were higher than baseline at marginally significant levels. These within-condition changes should not be confused with between-treatment comparisons, described later.

TABLE 2
Percentages of Newspapers Recycled as a Function of Phase and Condition, Experiment 1

	Baseline 3 Weeks	*Signs 3 Weeks*	*Follow-Up 1 Week After Delay*	*n*
Unadjusted means				
No persuasion				
No validation	47.9	46.6	52.8	17
Validation	40.7_a	49.9_b	49.9_b	16
Persuasion				
No validation	37.2_a	52.5_b	53.9_b	9
Validation	54.3_a	66.3_b	59.8	12
Control	22.5	27.5	25.5	8
Means adjusted for baseline				
No persuasion				
No validation		44.3_a	50.5_b	17
Validation		54.2_b	54.2	16
Persuasion				
No Validation		59.9_b	61.3_a	9
Validation		58.2_b	51.7_b	12

Note. For unadjusted means, subscripts refer to differences within a row for comparisons of baseline to signs and baseline to follow-up. One-tailed tests were used for hypothesized increases between baseline and signs, and two-tailed tests were used for comparing baseline to follow-up. Means with a common subscript (including no subscript) do not differ by a priori t tests, $p < .05$. Not shown are marginally significant differences: In both the "prompt" (no validation/no persuasion) and validation/persuasion groups, the difference between baseline and follow-up is marginally significant. For means adjusted for baseline, subscripts refer to differences within a column (time period), and indicate differences at $p < .05$. Means with a common subscript do not differ. Not shown is a marginally significant difference at follow-up between 54.2 and 61.3.

Recycling in the unsigned control building was lower than in the four experimental buildings throughout the study and did not change over time, $F(2, 14) = 1.84$, $MSE = 38.86$, $p < .20$, partial $\eta 2 = .209$. This provides some reassurance that the increased recycling in the signed buildings was not due to extraneous variables.

Treatment effects. Because of significant differences among the groups in recycling rates during baseline, analysis of covariance (ANCOVA) was used to evaluate the treatments when signs were in place and during the follow-up. The covariate was homogeneous for both time periods, Fs(3, 46) = 0.36 ($MSE = 84.18$) and 0.81 ($MSE = 94.87$), ps > .20, partial η^2s = .023 and .050, and accounted for significant variance, $F(1, 49) = 188.44$, $MSE = 141.73$, $p < .001$, partial $\eta^2 = .794$. Adjusted means and significant group differences are portrayed in the lower part of Table 2. Two-tailed a priori t tests examined treatment effects with signs in place and dur-

ing the 1-week follow-up period; subscripts indicate significant differences within a column.[3]

As expected, with the signs in place, adjusted means indicated that the prompt message yielded significantly less recycling than the other three conditions, which did not differ from one another. During follow-up, after the signs had been removed for a week, the persuasion-only group recycled the most and the validation-only group was second (the difference between them was marginally significant). The other two (prompt and validation/persuasion) recycled significantly less than the persuasion-only group.

Questionnaire Responses

Questionnaire responses were analyzed using a 2 (phase: baseline/follow-up) × 2 (persuasion) × 2 (validation) factorial ANOVA, and included only newspaper readers with a class or office in the building. Baseline scores were used to assess equivalence across the buildings before the signs were installed. Differences between baseline and follow-up scores were used to estimate cognitive reactions to the signs. Table 1 shows means for each condition and differences between baseline and follow-up for attitudes and cognitions.

Attitudes toward recycling. The different messages did not result in dramatically different levels of favorability toward newspaper recycling; main and interactive Fs(1, 842) ranged from 0.04 to 2.67, $MSE = 1.28$, ps > .10, partial η^2s = .000–.003. The one significant effect, a Validation × Phase interaction on the attitude measure, indicated simply that preexisting differences among groups were gone by the end of the study (Table 1), $F(1, 842) = 4.99$, $MSE = 1.28$, $p < .03$, partial $\eta^2 = .006$.

Cognitive elaboration. The measures of cognitive elaboration yielded a stable pattern that favored signs without persuasion (without "it is important"). For both number of arguments and percentage of respondents giving at least one argument, the pattern of means indicated that people not exposed to the persuasive message generated the most arguments. Persuasion interacted with phase such that reasons for recycling were higher at Time 2 in the groups whose signs had given no persuasive message, but stayed the same or were lower at Time 2 in groups whose signs had contained the "it is important" message, $F(1, 848) = 6.43$, $MSE = 0.85$, $p < 01$, partial $\eta^2 = .008$. This same pattern occurred in the simpler measure of whether respondents generated at least one reason for recycling, Persuasion × Phase $F(1, 848) = 13.66$, $MSE = 0.24$, $p < .001$, partial $\eta^2 = .016$.

Analysis of the particular types of reasons given (external, vague environmental, and specific environmental) indicated a significant Type × Phase interaction, $F(2, 1{,}694) = 3.49$, $MSE = 0.15$, $p < .03$, partial $\eta^2 = .004$, suggesting that type of reason changed between the baseline and follow-up questionnaires. Separate analyses of each type of reason indicated that only external reasons (money, social pressure, etc.) changed in frequency of use, for external reasons, Persuasion × Phase, $F(1, 847) = 6.86$, $MSE = 0.20$, $p < .01$, partial $\eta^2 = .008$. Specific environmental reasons did not vary with phase or condition, except for fewer arguments at Time 2, phase $F(1, 847) = 4.29$, $MSE = 0.13$, $p < .04$, partial $\eta^2 = .005$; remaining Fs involving phase (1, 847) ranged from 0.25 to 1.15, ps > .30. Nor did the vague environmental reasons differ, Fs involving phase (1, 847) ranged from 0.48 to 1.72, $MSE = 0.21$, ps > .19. Thus, considering both the simple counts and more detailed content analyses, people who had spent 3 weeks with signs that gave no reason for recycling generated more reasons for recycling, although as shown in Table 1, the reasons emphasized external pressures rather than substantive information about recycling's environmental benefits.

The more important issue is to what extent cognitions corresponded to behaviors, and results favored the validation-only condition. To focus on connections between cognitions and ongoing recycling, we compare differences in cognitions to the changes in unadjusted recycling rates (actual levels of recycling, rather than recycling adjusted for baseline). Table 1 shows that by follow-up, significant recycling increases were in the validation-only (9%, column 2) and persuasion-only (17%, column 3) groups. These significant increases mapped on to a significant increase in cognitions in the validation-only group (both total number and percentage providing a reason), but not in the persuasion-only group (neither index). In the validation/persuasion group, the marginally significant increase in recycling corresponded to fewer cognitions (Table 1 shows the pre–post differences in the validation/"important" condition to be marginally significant, but both are close to the .05 level. For number of cognitions, the obtained difference is –.22 and the critical value is .23; for percentage giving any reason, the obtained difference is –.120 and the critical value is .124.) In the prompt condition (no validation/no persuasion), a small and marginally significant increase in recycling corresponded to

[3]A number of main and interactive effects were significant in the ANCOVA, but were not as interesting to us as the cell-by-cell comparisons. For the interested reader, an overall persuasion main effect indicated that signs with the "it is important" message produced more recycling than signs with no message (58.5% vs. 52.5%), $F(1, 49) = 8.69$, $MSE = 141.73$, $p < .01$, partial $\eta^2 = .15$. This effect was qualified by a Persuasion × Validation interaction, $F(1, 49) = 6.23$, $MSE = 141.73$, $p < .02$, partial $\eta^2 = .11$, and the pattern of means partially supported our hypotheses. All three signs with a message produced higher recycling than the prompt sign, but these signs did not differ among themselves. The hypothesized three-way interaction among validation, persuasion, and phase (signs/follow-up) did not emerge, $F(1, 50) = 0.05$, $MSE = 83.27$, $p > .10$, indicating that the overall Persuasion × Validation effect was obtained during both treatment and follow-up. Only the Phase × Validation interaction approached significance, $F(1, 50) = 3.77$, $MSE = 83.27$, $p < .058$, partial $\eta^2 = .070$. None of the differences between means was significant, but the pattern of means indicated a clear reversal. With signs in place, the validation groups recycled more than the nonvalidated (56.2% vs. 52.1%), but during follow-up, the nonvalidated recycled more than the validated (55.9% vs. 53.0%).

a significant increase in percentage giving at least one reason. Overall, the validation-only condition was unique in yielding the total behavior change plus elaboration pattern: increased recycling, more reasons for recycling, and more participants giving a reason.

Knowledge, convenience and recycling ethos. Consistent with the overall increase in recycling, at the follow-up more participants reported knowing the recycling bin's location, from 53% to 70%, phase main effect $F(1, 846) = 16.94$, $MSE = 0.37$, $p < .001$, partial $\eta^2 = .020$. Rated convenience of getting to and using the recycling bin did not vary with condition, especially during the follow-up period. At follow-up, convenience Fs (1, 315) ranged from 0.14 to 2.65, $MSE = 3.14$, ps > .10, partial η^2s = .000 to .008, and time to get to bin Fs (1, 328) ranged from 0.08 to 3.34, $MSE = 2.90$, ps > .07, partial η^2s = .000 to .010. Therefore, as in Werner et al. (2002), validating perceptions that recycling was inconvenient did not increase respondents' ratings of its inconvenience. Also consistent with previous research, there were no differences among the groups in perceived recycling ethos. Estimates at follow-up ranged from 34.7% to 38.3%, with an average of 37.3%, Fs(1, 345) ranged from 0.49 to 0.64, $MSE = 447.86$, ps > .20, partial η^2s = .001 to .002. Questionnaire responses in the control building were similar to those in the treatment buildings, even at follow-up, and even though that group's recycling rate was substantially lower than all four others.

Discussion

This experiment is consistent with previous research in that well-designed signs produced rapid and significant increases in recycling. When the signs were in place, validation-only, persuasion-only, and validation/persuasion messages all produced increased levels of newspaper recycling. With respect to treatments, analyses of means adjusted for baseline showed the same pattern: With signs in place, the three effective signs all differed from the prompt sign (neither validation nor persuasion).

The follow-up period tested how durable the new behaviors were. The validation-only and persuasion-only groups fared best, both being significantly higher than baseline, whereas the validation/persuasion and prompt conditions differed from baseline at marginally significant levels. Comparison of means adjusted for baseline affirmed this pattern. The persuasion-only condition was highest (significantly higher than prompt and validation/persuasion groups), and the validation-only group was almost as high, the difference between them being marginally significant. The late, marginally significant increase in the prompt condition was not expected and will be ignored until replicated. The control group (no signs) did not increase its recycling, supporting the idea that something about our signs—not something else at the university—stimulated students to recycle more.

In all groups, attitudes were favorable at pretest and did not change by posttest, even though behaviors had changed and were being maintained. This pattern—behavioral maintenance without attitude change—suggests that our project increased accessibility of existing favorable attitudes. Consistent with its sustained recycling, the validation-only group showed the strongest increases in prorecycling cognitions, increasing in both the number of cognitions and the percentage of people providing at least one cognition, although most of the reasons were in the nonsubstantive "for the environment" category. In contrast, the persuasion-only group's cognitions did not change, and the validation/persuasion group provided fewer cognitions at follow-up compared to baseline.

As described so far, this study provides mixed evidence that validation increased message scrutiny. Certainly, validation alone (validation/no persuasion) yielded an increase in recycling and prorecycling cognitions, and recycling was maintained during follow-up. Thus, given a population with very positive attitudes toward recycling and a request to recycle, simple validation—acknowledging and empathizing with a complaint—appears to have stimulated favorable thoughts and behavior. The pattern is consistent with the idea that validation leads people to think about an issue: It is not only a way to increase scrutiny of a written argument, but it may also induce people to generate their own reasons for recycling, which may in turn lead to increased attitudinal accessibility and behavioral maintenance (cf. Tesser et al., 1995). One purpose of Experiment 2 is to replicate that finding.

Another purpose of Experiment 2 is to replicate and evaluate one possible explanation for the mixed results in the validation/persuasion condition. While the signs were in place, the combination of validation and persuasion was effective (for both unadjusted and adjusted recycling). However, by follow-up, recycling and cognitions in this group had both decreased. We propose an additional interpretation of our findings that accounts for most of the results and maintains the view that validation increases message scrutiny. This explanation is based on the idea that our message did not take into account the social ecology of newspaper recycling. In contrast to aluminum cans, which could be found either in the garbage or recycling bin, but rarely lying around classrooms, newspapers were frequently left on a desk and were rarely in the garbage. It appeared to us that people wanted to share their newspapers with other students. Unlike an aluminum can, which is not immediately reusable and is clearly the responsibility of the consumer, newspapers are a shareable commodity.

From this perspective, to put a newspaper into the recycling bin before someone else has a chance to read it is actually wasteful. Indeed, a few respondents told us this in their questionnaires. Had we realized this at the outset, we could have designed a different basic message, one that encouraged sharing, but also targeted certain class periods as the time to begin recycling. Similar signs were effective at targeting particular instructors to turn off classroom lights to save energy (Winett, 1977–1978). However, we designed our signs without this in-

sight, and the signs instructed people to do something that did not make sense, given their practice of sharing rather than discarding newspapers. Under these conditions, using a technique to increase scrutiny of a persuasive message may only have induced counterarguing and reactance. Experiment 2 accommodated students' desires to share newspapers by developing signs that encouraged sharing in the morning, but asked for recycling in the afternoon and evening.

EXPERIMENT 2

Social ecology is a term commonly used in community psychology, social work, sociology, and environmental psychology to refer to the complex system of social norms, common patterns of behavior, and—among environmental psychologists—the physical environment in which behaviors unfold. People engage in behaviors that are appropriate for particular settings, and newcomers learn these norms and rules by observation or informal feedback. In the case of newspapers, people often share newspapers rather than throwing them away.

In Experiment 2, we adapted our recycling instructions to this common practice. Our new signs acknowledged sharing and encouraged people to leave their newspapers for others, but suggested a time limit of 2:00 p.m., after which there were fewer readers, making recycling a necessity. We gathered recycling data in the morning and evening, and hypothesized that newspaper readers who read the social ecology-sensitive sign would not recycle in the morning, but would recycle in the afternoon and early evening. We also hypothesized, as suggested by the findings in Experiment 1, that persuasive messages that were softened by a social ecology sensibility would be more effective.

We chose two of the persuasive signs from Experiment 1. First, we used the validation-only sign to establish whether its greater impact on recycling and supportive cognitions could be replicated. Does validation increase elaboration and self-persuasion even in the absence of a persuasive argument? Second, we used the validation plus persuasion message because we had hypothesized that such a strong message without an appreciation of readers' desire to share would create reactance. Does this message become more effective when it is accompanied by another sign that acknowledges and encourages sharing? In addition to recycling, we examined numbers of newspapers used in each condition to see if sharing resulted in a reduction in use of fresh newspapers (i.e., a reduction in total number used).

Method

Overview

A preliminary study established that newspaper sharing was perceived to be a common practice at the university. Experiment 2 was conducted during the autumn semester more than 3 years after Experiment 1, thereby reducing the possibility of carryover effects interfering with our treatments. For the most part, the methodology replicated that of Experiment 1: Criteria for selecting buildings was the same; the persuasive messages contained the same basic information as the persuasive message from Experiment 1; data gathering and reliability checks were the same for both the recycling and questionnaire data; and so on. Three additions were a final mnemonic tag line to the persuasive signs, evening recycling data collection, and the addition of social ecology signs on the walls.

Preliminary Study

Approximately 2 years after Experiment 1 was completed, we conducted a brief study in the five buildings from Experiment 1. We drew random samples in each building, contacting most students as they sat in their classroom, waiting for class to begin. We asked for their help with a class project that involved completing a questionnaire entitled "Opinion Survey." Three questions tapped how frequently they read the student newspaper and their attitudes toward recycling it. Then they read a short story about a student who read the campus newspaper and left it behind after class, and finally they answered the following open-ended question: "Why did Pat leave the newspaper on the desk instead of throwing it away or taking it to the recycling bin? Give all the reasons that occur to you." The questionnaire continued on the back side of the page, with five reasons "other students had given" (forgot to take it; no time to discard or recycle; left it for someone else to read; didn't know where to recycle; didn't feel like recycling or discarding). Respondents were asked to indicate on 11-point scales how likely each reason was as an explanation for Pat's behavior. Scales ranged from 1 (*extremely unlikely*) to 11 (*extremely likely*).

Ten students indicated they never read the student paper and were omitted so we could focus on people who might have insights about recycling or not recycling (group *n*s ranged from 18 to 26, average $n = 23$). Three answers emerged in the open-ended responses, and leaving the paper for someone else to read was high among them: 64% spontaneously said Pat had left the paper for others to read; 61% said that Pat was too lazy to recycle or discard the paper; and 40% guessed that Pat had forgotten about the paper (other answers:"too inconvenient to dispose of," 11%; "Pat just didn't care," 19%). Our scaled items paralleled these, with mean scores of 7.1 for sharing with others, 5.7 for forgot, and 7.8 for didn't feel like it or too lazy. Two 5 (building) × 3 (reason) between- and within-participants ANOVAs resulted in similar patterns. Both yielded significant main effects for reason, with contrasts indicating that sharing and being lazy were equally likely, and both of these reasons were higher than forgot, for the open-ended responses, repeated measures $F(2, 224) = 6.78$, $MSE = 0.22$, $p < .001$, partial $\eta^2 = .057$; for the scaled items, repeated measures $F(2, 218) = 16.88$, $MSE =$

8.03, $p < .001$, partial $\eta^2 = .134$. Reason did not interact with building, indicating these patterns obtained in all five buildings; for the open-ended responses, interaction $F(8, 224) = 0.90$, $MSE = 0.22$, $p > .20$, partial $\eta^2 = .031$; for the scaled items, interaction $F(8, 218) = 1.39$, $MSE = 8.03$, $p > .20$, partial $\eta^2 = .048$. Thus, both open-ended and scaled responses support the idea that students consider sharing to be a reason for leaving a newspaper behind, providing support for proceeding with Experiment 2.

Experimental Conditions

The experimental design was a 3 (phase: baseline/treatment/follow-up) × 2 (time: morning/afternoon) × 2 (message: validation-only/validation plus "it is important") × 2 (social ecology: no/yes) between- and within-subjects factorial. The study was conducted over a period of 11 weeks. Baseline lasted 4 weeks, treatment lasted 4 weeks, and follow-up lasted 2 weeks after a 1-week delay. To be consistent with Experiment 1, unless indicated, we present follow-up data from only the second week (1 week after a 1-week delay). The persuasive signs were taped to garbage cans in classrooms and adjacent hallways. The mnemonic tag line in the no social ecology conditions was "It's up to you," and the mnemonic tag line in the social ecology conditions was "After 2PM, it's up to you." One copy of the no social ecology or social ecology sign was taped to the wall at eye level, near the exit in each classroom. Both kinds of signs were removed during the follow-up period.

The social ecology sign acknowledged the practice of sharing but asked people to "dispose properly of their newspaper" after 2:00 p.m. (note we did not ask them to recycle). To equate the social ecology and no social ecology conditions in their cognitive effort, we developed a control sign about the energy crisis, asking people to turn off the lights as they left the room (cf. Winett, 1977–1978). This sign differed in appearance (different font and paper color) from the recycling notice on the garbage cans. We selected the energy crisis because it has been associated with environmental activism, conservation, and energy costs. We wanted this sign to activate concerns about resources and the natural environment so that if there are differences between the social ecology and no social ecology contexts, the differences are more likely due to the social ecology endorsement, not simple activation of environmental, social, or financial concerns. To reduce chances that people would see both the energy conservation and recycling ecology signs, we separated the signs physically. We chose two pairs of buildings; the buildings in each pair were adjacent, but the pairs were a 5-min walk apart. We first randomly determined which pair would receive the no social ecology or social ecology sign, and then randomly assigned the two persuasive recycling signs within each pair.

For three of the buildings, the recycling bin was in a moderately convenient location (on an exit route for about half the students), and for the fourth, the validation plus persuasion/no social ecology condition, the recycling bin was highly convenient.

Dependent Measures

Recycling data. Three different experimenters rotated responsibility for counting the newspapers in each building twice daily, Monday through Thursday (there were too few afternoon and evening classes on Fridays). Newspapers were counted before noon and then again after all classes were finished for the day in each building (usually at 7:00 p.m.). One additional experimenter collected the reliability data on seven randomly selected occasions unknown to the experimenters. The average interrater correlation was .98, with a range from $r(16) = .70$ to $r(14) = .99$.

To maintain ecological validity and allow sharing of newspapers all day, no newspapers were removed from the rooms or wastebaskets (custodians removed them after 7:00 p.m.). Experimenters recorded three newspaper counts: in the wastebasket, lying around the room, and in the recycling bin. The morning recycled, trashed, and left in the room data are reported as collected, converted to percentage recycled [(recycled)/(trashed + left in room + recycled]. The afternoon recycling data represent the evening counts of papers left lying around the rooms combined with any changes between morning and evening in trashed and recycled newspapers (evening minus morning data). Included in the afternoon percentage are all of the papers left lying in the rooms (rather than computing a difference score) because the papers were available for afternoon and evening students to use, and were therefore their responsibility to recycle.

In addition to computing recycling, we summed the numbers of newspapers used (trashed, left lying around, and recycled) to see if our signs reduced demand for fresh newspapers by increasing sharing (i.e., the "reuse" in "reduce, reuse, recycle"). The evening fresh usage data represented changes between morning and evening for all three measures (recycled, trashed, and lying around the room).

Attitudes and cognitive responses. Questionnaire data were gathered twice during the study. The first administration was the second Monday of baseline, Week 2 of the study, and the second was the second Monday of follow-up, Week 10 of the study. Questionnaires were collected during the morning and then again the evening of the same day, spanning approximately 8 hr. Random sampling and the request to "help with a class project" were the same as in Experiment 1. Participation rates were quite high, for the morning during baseline, ranging from 68% to 80%, $\chi^2(3, N = 940) = 8.89$, $p < .05$; for the morning during follow-up, ranging from 75% to 91%, $\chi^2(3, N = 872) = 18.90$, $p < .05$; for the evening during baseline, ranging from 73% to 88%, $\chi^2(3, N = 325) = 6.39$, $p < .05$; and for the evening during follow-up, ranging from 60% to 82%, $\chi^2(3, N = 872) = 12.18$, $p < .05$. The high participation rates provide

fairly representative samples in each building, despite the significant differences among the buildings. Fifty-four percent of the participants were females.

Questionnaires were identical at Time 1 and Time 2. Most of the questions were the same as items used in Experiment 1. Because of research showing that affect can predict recycling, especially for people with weak (i.e., not accessible) attitudes (Smith, Haugtvedt, & Petty, 1994), we added one new item to tap affect about recycling ("If I did not recycle, I would feel terrible/great"). In addition, we used a different way of gathering respondents' cognitions, a question that had been used successfully in previous research (Werner et al., 2002). After a few background questions, we asked "How important is it to recycle *The Chronicle*?" with a 7-point scale ranging from 1 (*extremely unimportant*) to 7 (*extremely important*). After the response scale, the question continued "Please explain your answer to #1: List as many reasons as you can. Use the back side of page if necessary."

Responses to this open-ended question were coded into positive and negative statements and counted by a single individual. A negative statement was an antirecycling statement, such as "Recycling is stupid" (at Time 1, 20 people [4%] and at Time 2, 12 people [2%] made a negative statement). One additional rater provided reliability checks on 100% of the questionnaires; errors were corrected but inconsistencies based on different interpretations were not changed. Reliability between the two raters at Time 1 was $r(1,003) = .996$ for the positive comments and $r(1,003) = .984$ for the negative comments; at Time 2 the coefficients were $r(970) = .994$ for the positive comments and $r(970) = 1.00$ for the negative comments. As an overall index of how positive respondents' cognitions were, we subtracted the negative from the positive comments and analyzed the difference scores. For the content analysis, we used the same categories as in Experiment 1 (external, vague environmental, and specific environmental), with one primary and two reliability raters. For the primary and second rater at Time 1, kappa coefficients ranged from .95 (99% agreement) to 1.00 ($n = 331$), and for Time 2, they ranged from .96 (99% agreement) to 1.00 ($n = 200$). For the primary and third rater at Time 1, kappa coefficients ranged from .97 (99% agreement) to 1.00 ($n = 252$); for Time 2, they ranged from .92 (98% agreement) to 1.00 ($n = 256$). No answer and irrelevant comments were not content analyzed.

The three attitude items (importance, favorability, bad feeling) formed a scale (Cronbach's α at Time 1 = .66, and at Time 2 = .67) and were averaged prior to analyses. The two convenience items did not form a scale (Cronbach's α at Time 1 = .26, and at Time 2 = .38) and were analyzed separately in a multivariate analysis of variance (MANOVA).

Memory test for signs. In Werner et al. (2002), students had done poorly on a memory test of their building's sign, but the test was administered 2 weeks after the signs had been removed. In this experiment, to more accurately gauge knowledge of the sign without influencing recycling, we drew small samples in each building the Tuesday after the signs had been removed and stayed in the building only a short period of time (approximately 15–20 min). Fifty participants in each building were asked to answer "two quick questions" so the refusal rates were low and unrelated to our issue. Participants were informed that we had placed signs on the wastebaskets to encourage newspaper recycling. We showed them the first part of the sign ("Please, No Newspapers in the Trash!! Use the recycler located [location given for that building]"), and asked them to write out the persuasive message that had been at the bottom of the sign. Once they had written an answer (including "don't know"), the experimenter turned the page over to reveal the same question in multiple-choice format. Both of the validation messages were listed, along with four distractors ("Thank you for your help," "Support [undergraduate student government]," "Funds support the library,", and "We appreciate your help"). Because of low accuracy, responses to both questions were combined into a single score, with a 0 meaning both answers incorrect and a 1 meaning at least one was partly or completely correct.

Results

Replication of Experiment 1

The data in Tables 3 and 4 are difficult to compare to those in Table 2 (different times of day and different computational procedures), so before examining whether the social ecology conditions in Experiment 2 had been successful at shifting recycling to the afternoon, we examined day-long recycling in the no social ecology conditions to see whether these data replicated the results of Experiment 1. The 2 (message: validation-only/validation + persuasion) × 3 (phase: baseline/signs/follow-up) analysis yielded a significant main effect for phase, $F(1.21, 25.34) = 8.34$, $MSE = 351.93$, $p < .005$, partial $\eta^2 = .284$; and no Phase × message interaction, $F(1.21, 25.34) = 1.05$, $MSE = 351.93$, $p > .20$, partial $\eta^2 = .04$ (when sphericity is not achieved, we present Hyunh–Feldt degrees of freedom and significance levels). Follow-up t tests indicated that recycling was significantly higher than baseline during both the signs period and the follow-up period (baseline: 14.9%; signs: 24.7%; follow-up: 31.2%).

The ANCOVA indicated covariate homogeneity, $F(1, 20) = 2.06$, $MSE = 280.61$, $p > .16$, partial $\eta^2 = .093$; a significant effect for the covariate, baseline, $F(1, 20) = 37.44$, $MSE = 345.49$, $p < .001$, partial $\eta^2 = .652$; but no differences between the two validation messages either overall, $F(1, 20) = 0.01$, $p > .20$, $MSE = 345.49$, partial $\eta^2 = .000$, or between signs and follow-up, interaction $F(1, 20) = 0.21$, $MSE = 280.61$, $p > .20$ partial $\eta^2 = .010$. This replicates the impacts of the validation-only sign (adjusted means, signs: 23.4%; follow-up: 32.1%), and replicates as well as reaffirms the impact of the validation/persuasion sign (adjusted means, signs: 27.1%; follow-up: 29.6%). That is, not only was valida-

tion/persuasion effective when signs were in place, recycling did not decline after signs were removed as it had in Experiment 1.[4] We turn next to the separate morning and afternoon data shown in Table 3.

TABLE 3
Percentages of Newspapers Recycled as a Function of Phase, Time of Day, and Condition: Experiment 2

	Baseline 4 Weeks	*Signs 4 Weeks*	*Follow-up (1 Week After Delay)*	*n*
Morning				
No social ecology				
Validation only	6.47	10.45	13.75	15[a]
Validation and persuasion	30.09_a	39.25_a	55.15_b	8[b]
Social ecology				
Validation only	9.77_a	13.55_a	23.26_b	17[c]
Validation and persuasion	12.15	7.96	4.22	10[d]
Afternoon				
No social ecology				
Validation-only	1.86	3.92	0.00	
Validation and persuasion	13.33_a	19.37_a	47.29_b	
Social ecology				
Validation only	4.24_a	21.28_b	19.29_b	
Validation and persuasion	4.06_a	22.39_b	6.90_a	

Note. Subscripts refer to differences relative to baseline within each row, by a priori *t* tests, one-tailed between baseline and signs, and two-tailed between baseline and follow-up. All *p*s < .05. Building *n*s differ because not all classrooms were available for both experiments.

[a]Building's condition in Experiment 1 SBS Validation/No Persuasion. [b]SW Control. [c]BUC No Validation/No Persuasion. [d]MBH Validation/Persuasion.

Recycling Data

Changes in response to signs. The first analysis asked whether the signs increased recycling above baseline and whether these increases were maintained after the signs had been removed. A 3 (phase) × 2 (time of day: AM/PM) × 2 (message) × 2 (social ecology) between- and within-subjects ANOVA yielded three significant interactions involving experimental phase, with the highest order interaction that among phase, time of day, and social ecology, $F(1.26, 55.58) = 4.04$, $MSE = 237.56$, $p < .04$, partial $\eta^2 = .084$. Cell-by-cell comparisons shown in Table 3 indicate that, for signs in place, only the two afternoon social ecology signs yielded a significant increase relative to baseline (by one-tailed, a priori *t* tests).

With respect to sustained recycling, two-tailed tests were used to compare the follow-up period with baseline. As indicated in Table 3, for two conditions, morning recycling was higher during follow-up than it had been during baseline (no social ecology/validation plus persuasion, and social ecology/validation only). For these same two conditions, afternoon recycling was also higher during follow-up than it had been at baseline. Further consideration of treatment effects is presented within ANCOVAs.

Treatment effects. A 2 (phase: signs/signs removed) × 2 (time of day: AM/PM) × 2 (message) × 2 (social ecology) ANCOVA, covarying both morning and afternoon baseline, indicated considerable covariate heterogeneity; for morning baseline, covariate interaction *F*s(1, 42) ranged from 1.66, $MSE = 225.44$, $p > .20$, partial $\eta^2 = .038$, to 4.84, $MSE = 293.86$, $p < .03$, partial $\eta^2 = .103$; for afternoon baseline, interaction *F*s(1, 42) ranged from 1.55, $MSE = 225.44$, $p > .20$, partial $\eta^2 = .030$, to 5.00, $MSE = 194.18$, $p < .03$, partial $\eta^2 = .106$. Covariate homogeneity was achieved by analyzing the morning and afternoon data in two separate phase (signs/follow-up) by social ecology by message ANCOVAs, covarying either the morning or afternoon baseline period. The covariates were homogeneous, and each accounted for significant variance: Morning Covariate × Phase interaction, $F(1, 45) = 0.01$, $MSE = 291.81$, $p > .20$, partial $\eta^2 = .000$; covariate main effect $F(1, 45) = 25.71$, $MSE = 613.24$, $p < .000$, partial $\eta^2 = .364$; Afternoon Covariate × Phase interaction, $F(1, 43) = 1.62$, $MSE = 272.38$, $p > .20$, partial $\eta^2 = .036$; covariate main effect $F(1, 43) = 27.99$, $MSE = 522.31$, $p < .000$, partial $\eta^2 = .394$. Means adjusted for their associated covariate are presented in Table 4; subscripts refer to significant differences within a column, separately for morning and afternoon.

For morning recycling, a significant main effect for social ecology, $F(1, 45) = 4.52$, $MSE = 613.24$, $p < .04$, partial $\eta^2 = .091$, was qualified by a significant Social Ecology × Message interaction, $F(1, 45) = 4.52$, $MSE = 613.24$, $p < .04$, partial $\eta^2 = .091$. This interaction was due to the relatively low recycling in the social ecology/validation plus persuasion condition, where signs had instructed students to share newspapers in the morning but recycle in the afternoon (means across signs and 1 week of follow-up = 7.2% vs. 21.8%, 30.5%, and 21.9%; Table 4, rows 4 and 1–3, respectively). There were no effects involving experimental phase, *F*s(1, 45) ranged from 0.07 to 1.80, $MSE = 291.81$, *p*s > .18, partial η^2s < .038, indicating that the overall interactive effect obtained when signs were in place and after their removal.

[4]For readers interested in day-long recycling in the social ecology conditions, we conducted a phase by message by social ecology ANOVA. It yielded no interactions involving message, *F*s(1.59, 73.20) = 0.03 and 2.23, $MSE = 244.06$, *p*s > .125, supporting the finding that the validation-only and validation plus persuasion messages were equally effective. A significant Phase × Social Ecology interaction, $F(1.59, 73.20) = 6.20$, $MSE = 244.06$, $p < .006$, partial $\eta^2 = .119$, and follow-up *t* tests indicated that all groups increased in response to the signs, but during follow-up, no social ecology groups maintained their recycling (as described in the text), whereas the social ecology groups dropped back to baseline (baseline: 11.0%; signs: 27.0%; follow-up: 14.6%), a finding that must be considered in the context of their reduced use relative to morning (Table 5) and our different computational procedures.

TABLE 4
Percentages of Newspapers Recycled Daily Per Room, Adjusted for Baseline Recycling: Experiment 2

	Signs 4 Weeks	Follow-Up (After 1 Week Delay) 1st Week	2nd Week
Morning			
No social ecology			
Validation only	17.14	26.42	[2.72]
Validation and persuasion	22.37	38.58	[17.75]
Social ecology			
Validation only	17.06	26.70	[9.38]
Validation and persuasion	9.08	5.32	[7.99]
Afternoon			
No social ecology			
Validation only	10.51_a	9.36_a	[0.68_a]
Validation and persuasion	2.44_a	23.23_b	[1.97_a]
Social ecology			
Validation only	23.00_b	21.72_b	[27.15_b]
Validation and persuasion	24.46_b	9.85_a	[18.56_b]

Note. Subscripts refer to significant difference within each morning or afternoon column. The third column, in brackets, was analyzed in separate analyses of covariance. For morning recycling, phase did not interact with treatment, so individual means were not compared.

For afternoon recycling, a significant Phase × Social Ecology interaction, $F(1, 43) = 5.78$, $MSE = 272.38$, $p < .02$, partial $\eta^2 = .119$, was qualified by a significant three-way interaction among phase, social ecology, and message, $F(1, 43) = 4.81$, $MSE = 272.38$, $p < .034$, partial $\eta^2 = .101$. Examination of the adjusted evening recycling data in Table 4 shows that the two bases of this effect are that (a) with signs in place, as expected, the two social ecology conditions recycled at a higher rate than the two no social ecology conditions; and (b) with signs removed, there is a shift, and the highest recycling occurs in one no social ecology condition (validation plus persuasion) and one social ecology condition (validation only).

Second week of follow-up. To further examine long-term impacts of the treatments, we examined the second week of follow-up (i.e., the third week after signs had been removed), using separate ANCOVAs because of considerable covariate heterogeneity. As can be seen in brackets in Table 4, in the morning, the no social ecology, validation plus persuasion group recycled more than the others, Message × Social Ecology interaction, $F(1, 46) = 3.98$, $MSE = 15{,}698.61$, $p < .05$, partial $\eta^2 = .08$. In the afternoon, a single main effect for social ecology indicated that both of the social ecology groups recycled more than the no social ecology groups, $F(1, 46) = 6.83$, $MSE = 71{,}672.12$, $p < .012$, partial $\eta^2 = .129$.

Sharing and Newspaper Use

Although the social ecology message shifted recycling from morning to afternoon, it did not yield an overall substantial increase in recycling rates. Indeed, on average with signs in place and during follow-up, there was slightly more recycling in the no social ecology condition (adjusted mean = 18.8%) than in the social ecology conditions (17.2%). However, it is possible that the social ecology message increased the amount of sharing, thereby conserving paper by reducing the numbers of fresh newspapers needed. An overall ANCOVA indicated that the covariates were heterogeneous: morning baseline Fs(1, 44) ranged from 2.25 ($MSE = 0.42$) to 169.37 ($MSE = 1.00$), ps < .15, partial η^2s = .049 to .794; afternoon baseline Fs(1, 44) ranged from 0.00 ($MSE = 0.42$) to 4.02 ($MSE = 0.77$), ps > .20 and < .05, partial η^2s = .000 to .084. Covariate homogeneity was achieved in the morning usage data through two separate Message × Social Ecology ANCOVAs, one for signs in place and the other for the 1-week follow-up period. For the afternoon usage data, a single phase (signs/1 week of follow-up) by message by social ecology ANCOVA achieved homogeneity.

Table 5 shows the adjusted usage data (the Appendix provides unadjusted usage data). As expected, in the morning with signs in place, usage was significantly lower in the social ecology (3.99) than in the no social ecology condition (7.83), social ecology $F(1, 45) = 146.67$, $MSE = 1.08$, $p < .001$, partial $\eta^2 = .765$. Usage was also significantly lower in the validation/persuasion condition (5.56) than in the validation-only condition (6.25), message $F(1, 45) = 4.96$, $MSE = 1.08$, $p < .03$, partial $\eta^2 = .099$. Both of these effects were qualified by a marginally significant Message × Social Ecology interaction, indicating that without the social ecology message, the validation/persuasion group used fewer newspapers than the validation-only group, but that with the social ecology message, the two validation groups did not differ and were lower than the no social ecology conditions, interaction $F(1, 45) = 3.32$, $MSE = 1.08$, $p < .075$, partial $\eta^2 = .069$. The same pattern occurred during the first week of follow-up, as shown in Table 5, Social Ecology × Message interaction $F(1, 45) = 45.30$, $MSE = 0.87$, $p < .001$, partial $\eta^2 = .502$.

In the afternoon, the numbers of fresh newspapers used was quite small in general, and even significant differences translate into little practical impact. The Phase × Message × Social Ecology interaction was significant, $F(1, 45) = 64.66$, $MSE = 0.70$, $p < .000$, partial $\eta^2 = 590$, and significant differences are shown in Table 5.

To further understand these effects, we analyzed the fresh usage data during the second week of follow-up using two separate ANCOVAs (Table 5, column 3 in brackets). In the morning, the two no social ecology groups differed, with the validation plus persuasion group using more papers, whereas the two social ecology groups did not differ, interaction $F(1, 45) = 3.74$, $MSE = 1.08$, $p < .06$, partial $\eta^2 = .077$. In the afternoon, fresh newspaper use was extremely low overall, and the social ecology validation-only condition used significantly fewer than any of the other groups, social ecology $F(1, 45) = 4.61$, $MSE = 4.31$ $p < .04$, partial $\eta^2 = .093$; Message × Social Ecology $F(1, 45) = 2.82$, $MSE = 4.31$, $p < .10$, partial $\eta^2 = .059$. The steady low use by the social ecology validation-only group parallels its stable and relatively high fol-

TABLE 5
Average Number of Fresh Newspapers Used Daily per Room: Adjusted for Baseline Usage, Experiment 2

	Signs 4 Week	Follow-Up (After 1 Week Delay) 1st Week	2nd Week
Morning			
No social ecology			
Validation only	8.5_a	8.7_a	$[4.2_a]$
Validation and persuasion	7.2_b	4.6_b	$[5.5_b]$
Social Ecology			
Validation only	4.1_c	3.6_c	[4.6]
Validation and persuasion	3.9_c	3.3_c	[4.7]
Afternoon			
No social ecology			
Validation only	2.0_{ab}	0.6_b	$[2.9_b]$
Validation and persuasion	1.5_{bd}	5.0_a	$[2.1_b]$
Social ecology			
Validation only	1.1_{cd}	0.2_b	$[0.3_a]$
Validation and persuasion	2.4_a	0.5_b	[1.7]

Note. Subscripts refer to significant differences within each morning or afternoon column, by a priori two-tailed t tests, $p < .05$ (in the third column, the difference between 1.7 and 0.3 is marginally significant). Means with a common subscript do not differ. The third column, in brackets, was analyzed in separate analyses of covariance.

low-up recycling (Table 4), further supporting the impact of this sign.

Questionnaires

Memory test. Overall, 31% of the respondents gave a partly correct or correct answer to the memory test of the sign's message. Percentages ranged from 28.8% to 33.8% in the four conditions, and did not differ; main and interactive Fs(1, 257) ranged from 0.04 to 0.45, $MSE = .22$, ps > .20, partial η^2s = .000 to .002.

Numbers reading the paper. The small numbers of newspapers used in each classroom suggested that perhaps few students read the paper. Examination of that item in the questionnaire indicated that 72.5% of the students read the paper at least occasionally, and the percentages reading the paper did not vary by building (percentages ranged from 69.0%–74.6%).

Attitudes and cognitions. The primary issues tapped by the questionnaires were respondents' attitudes toward newspaper recycling and their related positive and negative cognitions. Items also measured the perceived convenience of recycling and perceptions of the recycling milieu. There were no differences due to treatments.

A 2 (phase: pre-/postsigns) × 2 (a.m./p.m.) × 2 (message) × 2 (social ecology) between-subjects MANOVA on the attitude scale, numbers of cognitions (positive minus negative), and percentages giving at least one reason for recycling, indicated that participants were moderately favorable toward and fairly knowledgeable about newspaper recycling. There was no strong evidence that any of these indexes had changed between the first and second questionnaires—that is before and after our signs had been in place for 4 weeks. On the 7-point attitude scale, the overall means before and after the signs were the same (5.4); before and after the signs, people gave the same number of reasons for recycling (.98); and before and after the signs, the same percentages of people gave at least one reason for recycling (73%), all eight multivariate Fs involving phase (3, 1,113) were nonsignificant, ranging from 0.09 to 2.39, ps > .07, and partial η^2s = .000 to .006; five of these Fs were less than 1.0.

Content analyses of the cognitions revealed that a small percentage of the participants gave external reasons for recycling (10%), one third used vague environmental reasons (35%), and one third provided specific environmental reasons (36%), with the remainder giving no reasons, type of reason, $F(1.64, 1833.12) = 88.53$, $p < .001$, $MSE = 0.25$, partial $\eta^2 = .073$. None of the reason by baseline versus follow-up effects was significant, Fs(1.64, 1833.12) ranged from 0.15 to 2.10, ps > .12, partial η^2s = .000 to .002, indicating that reasons for recycling did not become more sophisticated after our signs had been in place.

Convenience. A 2 (phase: baseline/follow-up) × 2(a.m./p.m.) × 2 (message) × 2 (social ecology) MANOVA on the two convenience items (quick to get to bin; convenient to get to bin) yielded a significant phase (pre/post) by a.m./p.m. by message by social ecology interaction, multivariate $F(2, 538) = 3.37$, $p < .035$, partial $\eta^2 = .012$. Examination of the means indicated that no group differed pre and post in its perception of convenience. This is consistent with previous work in indicating that using a message that validates complaints about inconvenience does not itself increase perceived inconvenience.

Emergent recycling ethos. Participants' estimates of what percentage of people in their building recycled newspapers at Time 1 ranged from 32% to 40%, with an overall mean of 35%, and at Time 2 ranged from 34% to 43%, with an overall mean of 38%, phase $F(1, 1,027) = 3.88$, $MSE = 436.75$, $p < .049$, partial $\eta^2 = .004$. The 2 (phase) × 2(a.m./p.m.) × 2 (message) × 2 (social ecology) ANOVA yielded no other significant effects involving phase, Fs(1, 1,027) ranged from 0.01 to 1.40, ps > .20, partial η^2s = .000 to .001. The perceived popularity of recycling is unlikely to have differentially influenced recycling in any of the buildings.

Discussion

One clear result to emerge from this experiment is that adding a message supportive of a behavior's social ecology can be an effective component of a behavior change program. The social ecology groups cooperated immediately and increased their afternoon recycling, and one of them—the social ecology/validation plus persuasion group—also reduced

morning recycling. When the signs were in place, afternoon recycling by the two social ecology groups was twice that of the two no social ecology groups. Although they did not recycle at a higher rate overall, they actually used fewer newspapers than the no social ecology groups, an alternative and equally effective way to conserve resources. Indeed, for this commodity, recycling data should not be used without also gathering sharing and usage data.

When the signs were in place, the social ecology message tended to overwhelm differences between messages (validation only vs. validation/persuasion). Both social ecology groups recycled more than the two no social ecology groups. However, after the signs' removal, recycling differences emerged that favored the social ecology/validation-only group (lower portions of Tables 4 and 5); this group was most consistent in its recycling, and recycled more than 20% of its newspapers. With respect to use, evening use of fresh newspapers was fairly low, but was particularly low in the social ecology/validation-only condition when signs were in place as well as afterward, further supporting the greater impact of that sign.

There were no differences in attitudes or cognitions between the pre- and posttest, nor did the treatment groups differ on these measures. Although it is hard to gauge, it is possible that the events of September 11, 2001—which immediately preceded our first week of baseline—were so preoccupying throughout the semester that our signs were not given the attention and deep cognitive processing they otherwise might have received.

As anticipated by the preliminary study, it was relatively easy to induce people to share newspapers more frequently, but it appeared to be more difficult to persuade people to recycle (recycling rates were low). Perhaps the message to recycle would have more impact if the papers were larger (had more pages) and were therefore worth more as a recycled good, or if this were a setting where people felt ownership over and responsibility for the space, such as in a secondary territory (Brown, 1987; Worchel & Lollis, 1982).

The responsiveness of students to the social ecology message suggests we should look for other situations in which the ecology may hinder or support ongoing behaviors and behavioral compliance. Because social ecology is so often "out of awareness," it may be easier to see this phenomenon when problems exist. For example, anecdotal information at the Salt Lake City airport indicated that confusion about whether or not to pass others in cafeteria lines led to frequent delays at some restaurants. When new signs indicated people should move ahead of someone who had ordered but was waiting for food, the lines moved more efficiently.

GENERAL DISCUSSION

Two purposes of these experiments were to enhance message effects by acknowledging the social ecology of newspaper use and to evaluate the impact of validation alone and in combination with persuasion. In Experiment 1, the validation-only sign was quite successful, yielding one of the highest recycling rates with signs in place and after their removal, and sponsoring significant increases in cognitions. In Experiment 1, validation/persuasion was effective initially, although not as effective as in previous research: the effectiveness waned slightly after signs were removed, and cognitions did not increase over time. The first analysis of Experiment 2 used total daily recycling in the no social ecology conditions as the best (although not perfect) comparison to Experiment 1. This analysis replicated the finding that validation alone was an effective message, and showed that validation/persuasion was also more effective than suggested by Experiment 1.

The remaining Experiment 2 analyses used slightly different computations of recycling (both morning and afternoon students were responsible for recycling newspapers left behind by morning students). These analyses indicated that, when signs were put in place, the social ecology message boosted the effectiveness of both the validation-only and the validate/persuade signs. Indeed, as instructed by this message, significant increases over baseline recycling occurred only in the two afternoon social ecology conditions. Results from the follow-up analyses tend to favor the social ecology/validation-only sign as a way to encourage behavioral maintenance. Recycling rates were consistently high in this condition, even in the morning. In contrast, in the validation/persuasion conditions, recycling was variable, especially in the afternoon, with or without the social ecology message.

In summary, across the two experiments, validation only yielded the most steady recycling, supporting additional research on the question of whether validation can, by itself, lead to cognitive elaboration and self-persuasion. Validation with persuasion yielded unpredictable results—sometimes high, sometimes low. Although adding the social ecology message was an important improvement, it did not unequivocally improve the validation/persuasion message. Additional research is needed to address the variability of the validation/persuasion sign.

We have emphasized social ecology, but other similar concepts have guided recycling research. For example, drawing on behavior setting theory, Hormuth, Katzenstein, Bruch, and Ringenberger (1993) suggested that recycling could be increased by teaching people to make recycling part of the total consumption–recycle–disposal process; that is, by weaving the desired environmental behavior into the ongoing "stream of behavior." Our ideas about social ecology and Hormuth et al.'s about behavior streams include the idea of ongoing patterns of behavior that involve people and their physical environment. As Wicker (1987) noted, both of these are similar to *scripts,* or sequences of events appropriate for certain settings and circumstances. The concepts are subtly different, and comparisons among them may help to identify

their similarities and differences and clarify the meaning of each (see Wicker, 1987). We chose the term social ecology for a number of reasons: It does not carry the connotation of behavior setting theory that the setting overwhelms and carries along the individual; it emphasizes the social and environmental contexts of behavior, not just the social milieu; it does not link behavior to a particular setting (people share newspapers in coffee shops, airports, and train stations, as well as university buildings); and the normative component appears to us to be stronger than is typically acknowledged in the work on scripted behavior. Further comparison and clarification of these concepts and their implications for instructional signs is needed.

These experiments and those that preceded them (Werner et al., 2002) aim to capitalize on dual pathway thinking to improve signage in public settings. Simple prompts were not as effective as signs that contained more—whether validation or validation and persuasion—supporting the idea that carefully designed signs can encourage people to process messages instead of just responding automatically to them. Much work remains to be done, however, especially with respect to understanding how signs influence behavior, and whether it is realistic to think that public signs can effect long-term attitude and behavior change. This project added social ecology to dual pathway thinking; future research may also benefit from combining the very robust ELM and HSM models with other setting- or behavior-specific variables.

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