A Program of Classical Conditioning Experiments Testing Variations in the Conditioned Stimulus and Context

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Twenty-one experiments regarding the strength of attitudinal conditioning for various brands of cola were performed. The conditioned stimulus, brand familiarity (various unknown, moderately known, and well-known cola brands), and the embedding context in which conditioning trials occurred (other known or unknown brands) were manipulated. Effects are strongest for unknown and moderately known brands and for colas conditioned in a context of known versus unknown brands. Evidence is also provided showing that attitudes are conditioned only when subjects are aware of the contingency between conditioned and unconditioned stimuli.

Classical (Pavlovian) conditioning has interested consumer researchers for years, but only recently has serious theoretical, empirical, and critical attention been devoted to the role and functioning of conditioning in a consumer context (see, e.g., Allen and Janiszewski 1989; Allen and Madden 1985; Bierley, McSweeney, and Vannieuwkerk 1985; Gorn 1982; Gorn, Jacobs, and Mana 1987; Kahle, Beatty, and Kennedy 1987; Kellaris and Cox 1989; Macklin 1986; McSweeney and Bierley 1984; Nord and Peter 1980; Stuart, Shimp, and Engle 1987). This growing interest among consumer researchers is in step with exciting developments throughout psychology that are sparking reevaluation and reconceptualization of the basic conditioning paradigm. A modern, fully cognitive perspective treats classical conditioning as cognitive associative learning—that is, the learning of relations among events in the environment—and supplants the historical view that conditioning is simply reflexive, simple-minded learning (cf. Dawson et al. 1982; Furedy, Riley, and Fredrikson 1983, p. 126; Holland 1984; Holyoak, Koh, and Nisbett 1989; Rescorla 1988).

This new perspective presents opportunities and imposes a challenging set of epistemological requirements for conducting meaningful conditioning research. It implores consumer researchers to go beyond merely demonstrating that an attitude toward a neutral brand can be conditioned and examining the conditions that influence the strength of the conditioned effect. It requires that consumer researchers examine factors such as the strength of the conditioned stimulus (CS) and the unconditioned stimulus (US), the similarity between them, and the context in which the CS and US appear.

This article reports on a research program designed to determine the role played by two prominent facets of conditioning experiments: (1) the character of the conditioned stimulus (in this research, different brands of cola drinks), and (2) the composition of the embedding context in which conditioning trials occur (in this research, other cola brands included as filler stimuli surrounding conditioning trials).

RESEARCH RATIONALE

In behavioral research, knowledge accumulation and generalization are complicated by inevitable variations in how experiments are designed and implemented. Perhaps in no other area is this problem greater than in classical conditioning studies, where changes in the US, CS, number of conditioning trials, experimental context, and a variety of other factors can be expected to influence research outcomes. Isolated experiments conducted by researchers in different laboratories using different methods and procedures are inevitably subject to different results. The goal of our research is to achieve some degree of generalizability through a program of experiments that differ from one another in rather slight although conceptually important ways. All experiments

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in our program employ the same conditioning procedures (described in the Methods section) but, as noted, vary the CS brand and the embedding context.

Conditioned Stimulus

Generally speaking, novel and more salient CSs promote greater amounts of conditioning and more rapid effects (Domjan and Burkhard 1986; McSweeney and Bierley 1984; Schwartz 1989). Our research varies CS novelty via brand familiarity. In each experiment, we use one of four unknown, two moderately known, or two well-known cola brands as the CS. The unknown brands are actual colas (named Cragmont, Elf, My-te-Fine, and Target colas) marketed in regions of the United States outside the study locale. These brands were almost always unfamiliar to our subjects, who were deleted from analysis in the few instances in which prior brand awareness was indicated. The moderately known colas are Royal Crown (RC) and Shasta, and the well-known brands are Coca-Cola and Pepsi. Although the distinction between moderately known and well-known brands is arbitrary, the rationale is that the moderately known colas are infrequently advertised and are rarely consumed by our subjects, whereas Coke and Pepsi are among the best-known brands in the marketplace and are virtual icons of the soft-drink category.

Congenial with this basic evidence, it is expected that greater attitudinal conditioning will result when novel, unknown brands serve as CSs compared to when moderately known and well-known brands are used. We anticipate stronger effects for RC and Shasta than for Coke or Pepsi, because attitudes toward the lesser-known brands should be more malleable than the more firmly established attitudes toward the icon-like Coke and Pepsi brands.

Filler Context

From the days of the classic attitudinal conditioning studies by the Staatses (e.g., Staats and Staats 1957, 1958), conditioning researchers have included filler items among the presentation of CS-US trials to reduce hypothesis guessing and mitigate demand artifacts as a rival account. In our research, the filler context is other brands of cola that are interspersed among the intermittent conditioning trials in which another brand, the target cola, serves as the CS.

The composition of filler items likely affects the strength of attitudinal conditioning. Indeed, basic conditioning researchers have gone so far as to claim that "context stimuli can have such large effects on resultant associative strength that they cannot satisfactorily be ignored by a nontrivial theory of classical conditioning" (Sutton and Barto 1981, p. 149).[1]

Additional justification for our interest in examining the role of filler context extends from the remonstrations of Cohen and Basu (1987), who contend that consumer researchers have artificially restricted their studies to relationships between consumers and single consumption objects and ignored the structural contexts in which the relationships actually exist. Applied to classical conditioning experiments, this is tantamount to saying that the results obtained from an experiment that conditions one brand by itself (without competition from other brands in an embedding context) may not hold up when real-world-like competition in the form of other brands competing for the consumer's attention is introduced.

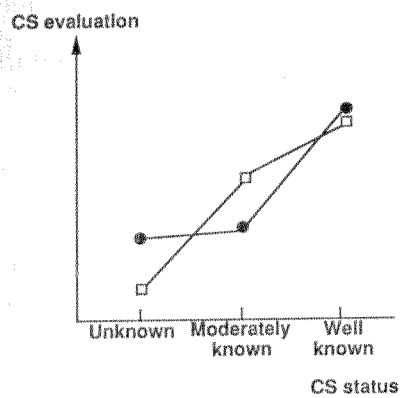
The logic of category priming and the associated notions of assimilation and contrast effects provide a theoretical perspective for making predictions about filler context's role in our experiments. To fully explicate this role, it is necessary to first overview some fundamentals of our experimental procedures. Specifically, in certain experiments the CS cola and the filler brands match in the sense that all brands are unknown or known (e.g., an unknown match would include Elf Cola as CS with Cragmont, My-te-Fine, and Target colas as filler), whereas in other experiments the CS and filler items do not match (e.g., Elf Cola as CS with Coke, Pepsi, and RC as filler). In general, then, if for the moment we disregard the distinction between moderately known and well-known brands, it can be seen that each experiment includes an unknown or known brand as the CS embedded in a context of correspondingly unknown or known filler brands. The key issue is whether the embedding context moderates the effect of CS novelty in determining how subjects evaluate cola brands serving as conditioned stimuli.

Filler brands can be thought of as providing subjects with a cognitive context or comparison standard against which to judge the CS brand, thereby priming a category exemplar or schema and influencing how the CS brand is interpreted and evaluated (see Cohen 1982; Cohen and Basu 1987; Herr 1989; Meyers-Levy 1989). The actual evaluation depends on whether the judgment is assimilated in the direction of the primed exemplar or contrasted with it. Prior research outside the conditioning domain has detected assimilation effects when the stimulus object is unknown (i.e., ambiguous) to subjects before their experimental participation, and contrast effects when the stimulus is previously known, or unambiguous (Herr, Sherman, and Fazio 1983; see also Herr 1989).

[1]It is important to note that the study of context in basic animal research involves a somewhat different issue than the treatment of context in our experiments. Whereas in our experiments the context consists of visual slides of filler cola brands interspersed among trials of a target CS cola paired with attractive visual scenes, context in basic animal research is the static background stimuli, such as features of a conditioning chamber (Balsam and Tomie 1985; Bouton and Swartzentruber 1986; Hall and Honey 1989; Reed and Reilly 1990).

FIGURE 1

MODERATING ROLE OF CONTEXT: PROPOSED RESULTS



NOTE.—●—●—● = Known context; □—□—□ = unknown context.

TABLE 1

CONDITIONED STIMULUS BRANDS AND FILLER CONTEXT FOR 21 EXPERIMENTS

	Filler context	
CS brand	Unknown brands	Known brands
Unknown brands:		
Cragmont	3	2
Elf	2	1
My-te-Fine	1	1
Target	1	1
Moderately known brands:		
RC	1	1
Shasta	1	2
Well-known brands:		
Coke	1	1
Pepsi	1	1

NOTE.—Cell entries are numbers of experiments with each brand under each context condition.

Applying these findings to our experiments, unknown CSs (Cragmont, Elf, My-te-Fine, Target) should be assimilated in the direction of the filler brands, whereas moderately known CSs (RC and Shasta) and well-known CSs (Coke and Pepsi) should be contrasted with the filler. Hence, the following results are expected: (1) unknown CS brands should be evaluated more positively when embedded in the context of known rather than unknown filler brands; (2) moderately known CS brands should be evaluated more favorably when placed among unknown rather than known brands, since, when evaluated in the context of Coke and Pepsi, RC and Shasta likely pale by comparison but are relatively attractive when they are seen with the various unknown colas; and (3) although well-known CS brands theoretically also should be contrasted with the filler context, no difference is expected because, regardless of context, the conditioning trials are not expected to have any significant impact on subjects' already firmly established attitudes toward these brands.[2] Figure 1 displays these predictions.

METHODS

The CS in each experiment is one of the four unknown, two moderately known, or two well-known cola brands previously mentioned. The US is a composite of four individually attractive water scenes used in Stuart et al.'s (1987) earlier experiments (a mountain waterfall, a sunset over water, a boat mast against the sky, and a lavender-hued island). Water scenes are appropriate stimuli for cola CSs inasmuch as both the US and the CS suggest refreshingness, soft drinks are often consumed in conjunction with water recreation, and advertisers sometimes promote soft drinks in water contexts (swimming pools, beaches, etc.). Thus, cola drinks and water scenes represent sufficient shared relevance or relatedness, we think, to facilitate a learned association between the two stimuli (see Domjan and Burkhard 1986, p. 78; Schwartz 1989, pp. 86–90).

The filler context for the conditioning trials includes three unknown or known brands. However, the specific composition of unknown and known contexts varies somewhat across experiments depending on which brand is the CS. Specifically, with an unknown brand as the CS conditioned in the context of other unknown colas, the filler items include the three remaining unknown brands; when an unknown brand is conditioned in a known context, Coke, Pepsi, and either RC or Shasta serve as fillers. When a moderately known brand (either RC or Shasta) is conditioned in the context of known brands, the filler brands include Coke, Pepsi, and the remaining moderately known brand. When a well-known brand (either Coke or Pepsi) is conditioned in the context of known brands, the filler brands include RC, Shasta, and the remaining well-known brand. When a moderately or well-known brand is conditioned in the context of unknown brands, any three of the four unknown brands constitute the filler context.

Twenty-one experiments were performed (see Table 1). Some brands were used multiple times in a particular CS-type/filler-type combination, whereas other brands were used only once. The rationale is straightforward: whenever an experiment yielded an unexpected finding or an exceptionally strong effect, a replication was conducted to assess whether the previous finding was generalizable or anomalous. For example, in the first experiment with Cragmont, we unexpectedly found that the control group had a slightly more positive attitude toward this brand than did the conditioning group (see Cragmont1 in Table 2). Accordingly, an identical sec-

[2]As a matter of reporting integrity, it should be noted that the foregoing account evolved during the course of the review process rather than having been formulated a priori.

ond experiment (Cragmont2) produced a finding in accord with our original expectation. Considering the extreme reversal of these two results, a third Cragmont experiment (Cragmont3) was deemed necessary. Similarly, the first Elf and Shasta experiments (see Elf1 and Shasta1 in Table 2) yielded exceptionally strong results, thereby justifying replications.

Experimental Procedures

All 21 experiments adhered to the forward-conditioning procedures employed in Stuart et al.'s (1987) fourth experiment. Specifically, we used a short-delayed procedure in which a slide of the CS always preceded a slide of a positively valenced US. Each experiment involved a conditioning group and a random control group.

The conditioning group was exposed to 20 conditioning trials in which the CS cola was always followed by a US scene, and to 60 nonconditioning "trials" in which non-CS brands of cola were paired with neutral scenes (i.e., 12 pictures pretested as being evaluatively neutral; e.g., a license plate, weeds growing in a pond, and unpainted boards). All conditioning and nonconditioning trials involved projecting a cola brand on a screen for 7.5 seconds followed immediately by a 7.5-second slide of either a positively valenced slide (in the case of the conditioning trials) or a neutral slide in the nonconditioning pairings. All 80 15-second pairings ended with a 2-second pause during which the screen was dark, thereby encouraging subjects to react to the subsequent pairing. Each of the four US water scenes followed the CS a total of five times, with the various water scenes randomly distributed among the 20 conditioning trials. To minimize any possible temporal conditioning, the 20 conditioning trials were interspersed among the 60 nonconditioning pairings. The time between the end of a conditioning trial and the onset of the next trial (i.e., the intertrial interval) ranged from 2 to 102 seconds with an average of 54 seconds.

The random control group received the same number of presentations of the CS and the US and the same number of presentations of the filler brands and scenes, but all were assigned randomly with respect to each other. We imposed constraints on the structuring of the random control presentation to prevent the CS and the US from occurring contiguously more than three times during the total presentation.

All experiments employed identical procedures, except that two experiments (labeled RC1 and Shasta1 in Table 2) were conducted by a different experimenter and in a different research setting than the other 19 experiments. Experimental sessions had two to 10 subjects, with most sessions having four to seven. The experimenters, who were not blind to the research hypothesis, told subjects that they were participating in an advertising research study but did not offer any further details. Subjects then viewed a slide presentation that included either the conditioning or random control treatment. All slide presentations were shown using three Kodak Ektagraphic III slide projectors with the timing of the slides preprogrammed and recorded on audiotape using an Audio Visual Laboratories Coyote three-projector dissolve unit with memory programmer and a Sharp sync-pulse recorder. Timing accuracy was to within 0.1 second, thereby assuring consistency with repeated presentations.

The 23-minute slide presentation was interrupted following the first of three approximately equal portions; a questionnaire booklet was distributed; and subjects were instructed to complete the first page, which included scale items measuring attitudes toward one of the filler brands. The slide presentation resumed; subjects were exposed to the middle portion of slides; the presentation again was halted; and subjects rated a second filler brand. (These two interludes served to break up the monotony of a long, continuous presentation and to deflect hypothesis guessing by not restricting attitude measurement to the CS brand.) The slide presentation resumed once again, and attitude toward the CS was measured after the final portion, after subjects had been exposed to all 20 CS-US conditioning trials. Demographic characteristics and a measure of CS-US contingency awareness were included at the end of the questionnaire. We concluded each experimental session by informing subjects that they would receive details about the study in the mail and urging them not to discuss the study with anyone.

Subjects

Subjects for the first 19 experiments were students from a psychology subject pool, who participated during a period of four regular-session semesters (fall 1987 to spring 1989). The two remaining experiments were conducted with business students during summer 1990. The 21 experiments ranged in size from 51 to 83 subjects, with most experiments including roughly 30 subjects each in the conditioning and random control groups. The majority of subjects were female (69 percent on average) and white (81 percent on average).

Dependent Variable

Four measures operationalized the conditioned response, or attitude toward the brand serving as the CS. They were (1) a summated score of seven seven-point semantic differential items (good-bad, high quality–poor quality, like very much–dislike very much, superior-inferior, attractive-unattractive, pleasant-unpleasant, and interesting-boring); (2) a seven-point global evaluative item ("Overall my feeling about [the CS cola brand] is favorable-unfavorable"); (3) an 11-point measure of purchase intentions ("All things considered, if you were to purchase soft drinks on one of your next several trips to the supermarket, what are the

TABLE 2

SUMMARY OF 21 CONDITIONING EXPERIMENTS

		Conditioning group			Random control group				
CS	Context	*n*	M[a]	SD	*n*	M[a]	SD	*t*-value	Effect size[b]
Unknown:									
Cragmont1	Unknown	30	−.13	3.82	26	.15	3.89	−.26	−.08
Cragmont2	Unknown	42	.83	3.67	41	−.85	3.72	2.07*	.46
Cragmont3	Unknown	31	.71	3.70	28	−.79	3.91	1.51+	.40
Elf1	Unknown	25	1.58	3.91	26	−1.52	3.12	3.14**	.88
Elf2	Unknown	36	.11	4.09	29	−.34	3.23	.49	.12
My-te-Fine1	Unknown	27	1.34	4.11	32	−1.13	3.31	2.57**	.67
Target1	Unknown	29	.93	3.95	31	−.87	3.59	1.85*	.48
Cragmont4	Known	41	.87	4.24	40	−.89	2.48	2.30*	.51
Cragmont5	Known	37	.77	4.67	39	−.73	2.46	1.73*	.41
Elf3	Known	30	.53	4.54	33	−.59	2.37	1.20	.31
My-te-Fine2	Known	35	.45	4.09	32	−.45	3.18	.99	.24
Target2	Known	33	.94	4.38	30	−1.04	2.53	2.22*	.55
Moderately known:									
RC1	Unknown	31	.26	3.03	31	−.27	4.17	.58	.15
Shasta1	Unknown	30	.39	3.33	29	−.41	3.84	.85	.22
RC2	Known	30	.91	3.87	29	−.94	3.45	1.93*	.50
Shasta2	Known	26	1.57	3.41	29	−1.61	2.98	3.62**	1.00
Shasta3	Known	31	1.08	4.02	33	−.99	3.11	2.31*	.58
Well-known:									
Coke1	Unknown	28	−.04	3.88	26	.03	3.59	−.06	−.02
Pepsi1	Unknown	34	.75	2.68	32	−.81	4.13	1.82*	.45
Coke2	Known	32	.50	3.18	30	−.55	4.07	1.14	.29
Pepsi2	Known	28	.63	4.05	25	−.58	3.25	1.19	.33

NOTE.—All probabilities are based on one-tailed *t*-values in line with the expectation that the conditioning group in each experiment would have a more positive attitude toward the CS than would the random control group.

[a]Mean Att_{CS} scores.

[b]The effect size is based on the Glass *d*-statistic (Hunter et al. 1982).

+$p < .10$.

*$p < .05$.

**$p < .01$.

chances in 10 that you would purchase [the CS cola brand] if it were available?"); and (4) a graphic rating scale consisting of a 130-millimeter line on which subjects placed an "X" to indicate their feelings toward the CS cola brand, from very negative to very positive.

Each of the individual measures was standardized, and then all four were summed to form a cumulative attitude variable, hereafter referred to as Att_{CS}. In Fazio et al.'s (1986) terms, this attitude represents a relatively cold, cognitively based judgment of the subject's feelings of favorability or unfavorability toward the conditioned cola brand. The amount of conditioning is assessed in each experiment by comparing for statistical significance the difference between conditioning and control group's mean Att_{CS} scores and then computing Glass *d*-statistics that reflect each experiment's effect size in terms of the difference in the number of standard deviation units between the conditioning and control groups (Hunter, Schmidt, and Jackson 1982).

RESULTS

Measurement Considerations

Combining the four separate measures into a composite dependent variable (Att_{CS}) assumes that Att_{CS} is indeed unidimensional. This was tested by running confirmatory factor analyses. Although unidimensionality was rejected in five of the 21 experiments, even in these cases the diagnostic statistics were favorable. The average goodness-of-fit index across the 21 studies was .97, and the average root mean square residual was .015; only rarely were standardized lambdas (linking the individual measures to the latent attitude construct) below .80. Also, the average reliability (coefficient alpha) across the 21 studies was .94. We are thus comfortable in treating Att_{CS} as a meaningful and psychometrically sound index of subjects' attitudinal responses.

Overview of Results

Table 2 summarizes the findings for all 21 experiments, including sample sizes, mean Att_{CS} scores, standard deviations, and *t*-values. When an experiment behaves as expected, the Att_{CS} means should be positive in the conditioning group and negative in the random control group. And, when an unknown or moderately known brand—but not a well-known brand—serves as the CS, the difference between the conditioning and random control groups should be statistically signifi-

cant. Also presented are Glass d-statistics; for example, the effect size, d, of .46 in the Cragmont2 experiment means that the attitude of the conditioning group toward that brand was nearly one-half a standard unit more favorable than that of the random control group.

Eleven of the 21 experiments yielded statistically significant ($p < .05$) evidence of positive attitudinal conditioning. The conditioning groups in these experiments hold significantly more positive attitudes toward the CS brands than do the random control groups. In instances of statistical significance, the effect sizes ranged from .41 SD between the two groups to a full standard deviation, with an average effect size of .59. Table 2 reveals that, of the 11 instances of statistical significance, seven of the 12 experiments employing unknown brands as CSs achieved statistical significance, as did three of five experiments with moderately known brands and only one of four with well-known brands as CSs. The following section provides an explanation for these results.

Meta-analysis of Effect Sizes

Table 3 cumulates the results by the two study characteristics: CS (unknown, moderately known, and well-known brands) and filler context (unknown or known brands). The average frequency-weighted effect size, $\bar{d}$, across the 21 experiments is .40. Because the variance of $\bar{d}$ is zero when corrected for sampling error (Hunter et al. 1982, p. 102), $\bar{d}$ in this case is a precise estimate of the unknown population parameter, D. Also presented in Table 3 are 95-percent confidence intervals (see margin totals). Because none of the intervals contain zero, it can be inferred that conditioning experiments of the type performed in this research always yield positive results regardless of study conditions. It also can be concluded that the size of the conditioned effect is generally consistent across experiments, except when unknown brands provide the filler context ($.15 < D < .53$) and when moderately known brands are used as CSs ($.17 < D < .79$). More will be said later about moderately known brands.

Role of Conditioned Stimulus. The row margins in Table 3 reveal that the average effect sizes are .41 for unknown CS brands, .48 for moderately known brands, and .27 for well-known brands. Although these average effect sizes are not significantly different (Kruskal-Wallace $\chi^2 = 1.68$, NS) because of an underpowered test of only 21 data points, it is evident that the least amount of conditioning is realized when attempts are made to condition attitudes toward well-known brands. Two factors account for this. First, in line with conditioning-theory predictions (see, e.g., McSweeney and Bierley 1984), highly familiar brands, such as Coke and Pepsi in the present experiments, are so well-known that little opportunity remains for additional learning and attitude enhancement. Second, the operation of ceiling effects made it unlikely that the conditioning groups' attitudes could be enhanced enough by the CS-US pairings to become significantly more favorable than the control group's already-favorable attitudes (e.g., on the seven-item seven-point semantic differential scale, random control group subjects had summated scores exceeding 40 out of a maximum score of 49). Indeed, it is surprising that even one of the experiments with well-known brands reached significance (see Table 2, Pepsi1).

TABLE 3

META-ANALYSIS OF 21 CONDITIONING EXPERIMENTS

CS	Filler context: Unknown brands	Filler context: Known brands	Totals
Unknown brands:			
d[a]	.41	.41	.41
Number of experiments	7	5	
Confidence interval[b]			$.41 < D < .41$
Moderately known brands:			
d[a]	.18	.68	.48
Number of experiments	2	3	
Confidence interval[b]			$.17 < D < .79$
Well-known brands:			
d[a]	.24	.31	.27
Number of experiments	2	2	
Confidence interval[b]			$.27 < D < .27$
Totals:			
d[a]	.34	.47	.40
Confidence interval[b]	$.15 < D < .53$	$.47 < D < .47$	$.40 < D < .40$

[a]Average, frequency-weighted effect size based on Glass d-statistics for individual experiments.

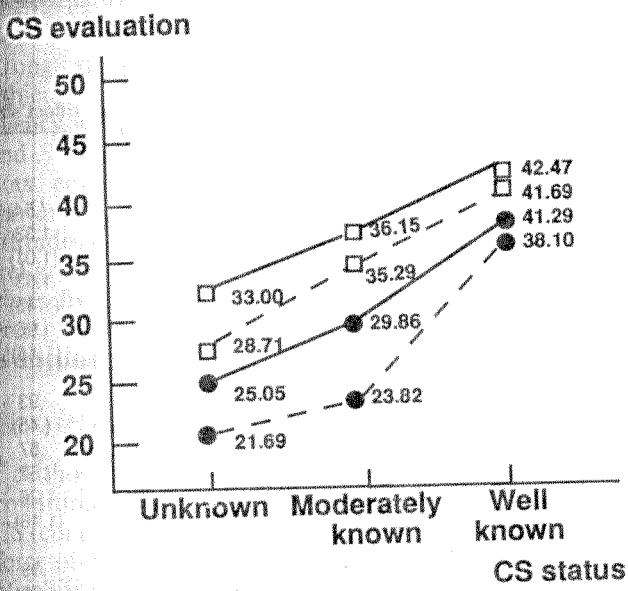
[b]Ninety-five-percent confidence intervals: the intervals are invariant in those instances where the variance of effect size controlled for sampling error is zero (see Hunter et al. 1982, pp. 102–103).

Role of Filler Context. The average effect sizes for unknown and known context brands are .34 and .47, respectively (Table 3), which is not statistically significant (Kruskal-Wallace $\chi^2 = 1.61$, NS). The 95-percent confidence interval for unknown filler brands is $.15 < D < .53$, whereas for known brands the estimate is invariant at .47 because of a zero variance when corrected for sampling error. Overall, then, it appears that conditioning effects were somewhat stronger (although not to a statistically significant extent) and less variable when known brands provided the filler context; however, the following section qualifies this general statement and reveals the precise manner in which the CS and filler-context factors interacted.

Moderating Effect of Filler Context. It was argued earlier in the case of unknown and moderately known

FIGURE 2

MODERATING ROLE OF CONTEXT: OBTAINED RESULTS



NOTE.—Results summarize the raw, unstandardized scores for all 21 experiments on the basis of the seven-item, seven-point semantic differential scale (range: 7–49), which is the most psychometrically sound of the four measures that constitute the dependent variables (Att_{CS}). ●—●—● = Known context, classical conditioning group; ● - - - ● - - - ● = known context, random control group; □—□—□ = unknown context, classical conditioning group; □ - - - □ - - - □ = unknown context, random control group.

CS brands that the filler context should moderate the CS-novelty factor's influence on Att_{CS} (see Fig. 1). However, the results are decidedly mixed regarding this prediction. If we turn first to the unknown CS brands, evaluations of these colas were expected to be assimilated in the direction of the context brands, which would lead to more favorable evaluations when in the context of known as opposed to unknown filler brands. Contrary to expectation, filler context played absolutely no role when unknown brands were CSs; the average effect size was .41 in both unknown- and known-brand contexts. The data, in fact, show that both the conditioning and random control groups evaluated the various unknown brands less favorably when embedded in the context of known as opposed to unknown cola brands (see Fig. 2). The unknown CS brands apparently were contrasted with the known filler brands but assimilated in the direction of the other unknown filler brands. In any event, considerable opportunity remained in both filler contexts for the conditioning treatment to elevate the conditioning groups' evaluations of the novel CS brands to levels significantly above the control groups' ratings.

Directing attention now to the moderately known CS brands, it can be seen (Table 3 and Fig. 2) that context played an important role: the average effect varies greatly from .18 for unknown filler brands to .68 for known brands—a full one-half standard-unit difference

TABLE 4

MEAN SCORES ON UNSTANDARDIZED VARIABLES FOR EXPERIMENTS WITH MODERATELY KNOWN CSs

Dependent variables[a]	Known filler			Unknown filler	
	RC2	Shasta2	Shasta3	RC1	Shasta1
SDSUM:					
Conditioning	34.23	27.19	28.16	37.90	34.40
Control	29.76	19.93	21.76	36.65	33.93
OVERALL:					
Conditioning	5.10	3.92	3.88	5.77	5.17
Control	4.21	2.46	3.33	5.48	4.97
INTENT:					
Conditioning	4.97	2.81	3.25	6.32	5.17
Control	3.86	1.55	1.67	5.94	4.07
GRAPH:					
Conditioning	84.20	53.52	63.81	97.50	91.10
Control	64.66	29.86	48.59	95.81	86.72

NOTE.—Experiments are keyed to the labels in Table 2.

[a]SDSUM = seven-item seven-point semantic differential scale; OVERALL = single-item seven-point evaluative rating scale; INTENT = single-item 11-point purchase probability scale; GRAPH = 130-millimeter graphic rating scale.

(Kruskal-Wallace $\chi^2 = 3.00$, $p = .08$). To fully appreciate this result, it is necessary to examine the raw data that formed the standardized dependent variable (i.e., Att_{CS}) used in calculating effect sizes for the five RC and Shasta experiments (see Table 4). A very telling picture emerges: when RC and Shasta were embedded in known-brand contexts (i.e., experiments RC2, Shasta2, and Shasta3), both the conditioning and control groups' average ratings on all four measures are substantially lower than when these brands were embedded among unknown brands (i.e., experiments RC1 and Shasta1). These results reflect the previously discussed priming effects expected to materialize when a moderately known CS is contrasted with a category exemplar. Specifically, when embedded in the context of Coke and Pepsi, RC and Shasta paled by comparison, as evidenced especially by the control groups' low evaluations in experiments RC2, Shasta2, and Shasta3. However, when the context consisted of unknown brands, RC and Shasta were relatively attractive and received elevated ratings. Hence, in the case of moderately known CS brands, we see that, when the context contains known cola brands, the conditioning group's mean scores, although relatively low themselves in comparison with comparable means in the unknown-filler experiments, are sufficiently greater than the control group's to reach statistical significance. Comparatively, when the filler context contains unknown brands, the control group's elevated scores restrict detecting a significant effect.

A Final Consideration: The Role of Contingency Awareness

A particularly provocative and troubling issue throughout the history of conditioning experiments with

TABLE 5

ROLE OF CONTINGENCY AWARENESS

	Contingency-aware group			Contingency-unaware group (random-control group)[a]				
CS	n[b]	M[c]	SD	n	M[c]	SD	t-value	Effect size
Unknown:								
Cragmont3	25 (81)	1.24	3.57	6	−1.51	3.68	1.68*	.77
				(28)	(−.79)	(3.91)	(1.97*)	(.54)
Elf2	12 (33)	3.21	4.11	24	−1.44	3.14	3.78**	1.33
				(29)	(−.34)	(3.23)	(2.95**)	(1.01)
Cragmont5	19 (51)	3.65	3.89	18	−2.28	3.33	4.97**	1.63
				(39)	(−.73)	(2.46)	(5.23**)	(1.46)
Target2	7 (21)	4.05	6.41	26	.11	3.35	2.25*	.96
				(30)	(−1.04)	(2.53)	(3.45**)	(1.45)
Moderately known:								
RC1	17 (55)	1.43	2.83	14	−1.16	2.71	2.59**	.93
				(31)	(−.27)	(4.17)	(1.51)†	(.45)
Shasta1	17 (57)	1.32	2.17	13	−.82	4.21	1.67*	.67
				(29)	(−.41)	(3.84)	(1.94*)	(.52)
Shasta3	18 (58)	2.89	3.75	13	−1.44	2.95	3.46**	1.26
				(33)	(−.99)	(3.11)	(3.96**)	(1.16)
Well-known:								
Coke1	17 (61)	−1.40	3.83	11	2.06	3.96	−3.55	−.89
				(26)	(.03)	(3.59)	(−1.22)	(−.39)
Pepsi1	17 (50)	.68	2.48	17	.79	3.02	−.11	−.04
				(32)	(−.81)	(4.13)	(1.36)	(.41)

NOTE.—All probabilities are based on one-tailed t-values given the expectation that the contingency-aware group in each experiment would have a more positive attitude toward the CS than would the contingency-unaware and random control groups.

[a]Results for the random control group are given in parentheses.

[b]Values in parentheses in this column indicate the percentage of subjects in the conditioning group who were contingency aware.

[c]Mean Att_{cs} scores.

†$p < .10$.

*$p < .05$.

**$p < .01$.

human subjects has been the matter of subject awareness of the CS-US contingency (cf. Brewer 1974; Shanks and Dickinson 1990). Some scholars argue, on the one hand, that subject awareness is in violation of classical conditioning's ontological premises (see, e.g., Kahle et al. 1987), whereas neo-Pavlovians contend that the presence of contingency awareness is indeed a necessary condition for classical conditioning: "The acquisition of autonomic CRs [conditioned responses] is not an automatic process, but rather requires conscious cognitive processing of the stimulus contingency" (Dawson and Schell 1987, p. 33). Although the philosophical issue is provocative, our interest is primarily empirical. What impact in our experiments did contingency awareness have, if any, on the strength of conditioned response? Only Allen and Janiszewski (1989) have heretofore offered empirical evidence on this issue in a consumer context.

To address this issue, we initially measured contingency awareness by posing an open-ended question to subjects at the end of each experiment; however, only after performing 12 experiments did it occur to us that this measure was too crude to ascertain whether subjects were truly aware of the systematic relationship between CS and US. Thereafter, we used a more precise, two-part close-ended measure. We asked subjects to select from four brands (the CS brand and three fillers) the one brand that always preceded attractive visual scenes and to indicate how confident they were that their choice was indeed the correct brand. Responses were classified as "contingency aware" when subjects selected the CS brand and were absolutely or somewhat certain of their choice; all other responses were considered "contingency unaware."

Table 5 presents means and standard deviations for the conditioning group subjects (subclassified as either contingency aware or contingency unaware) and for the random control subjects and provides t-test and effect-size results. Note that the range of contingency-aware subjects is considerable (from 21 to 81 percent). Awareness levels are basically stable in the five experiments with moderately known and well-known brands, ranging only from 50 percent (Pepsi1) to 61 percent (Coke1), but the percentage of contingency awareness ranges greatly in the case of unknown brands, from 21 percent (Target2) to 81 percent (Cragmont3).

Turning to the substantive results in Table 5, it can be seen that contingency-aware subjects had signifi-

cantly more positive attitudes than both the unaware subjects and the random control subjects in all experiments except those with the two well-known brands, Coke and Pepsi. Moreover, the effect sizes were very strong, showing on average that contingency-aware subjects' attitudes toward the CS brands were .78 frequency-weighted SDs greater than the unaware subjects' and .74 frequency-weighted SDs greater than the random control subjects'. Comparatively, though not shown in Table 5, the contingency-unaware subjects' attitudes were not more positive than the random control groups' except in the case of Cragmont5. It is obvious from these results that contingency awareness greatly enhanced positive attitude conditioning, which is discussed next.

DISCUSSION

Although our research program is based on a single conditioning paradigm, a single product category as the CS (cola) and only one kind of US (water scenes), the force of 21 experiments affords some degree of generalizability to the following conclusions. First, this type of conditioning experiment has an average effect in the magnitude of a .40-SD difference between conditioning and random control groups. This amount of effect is equivalent to saying that the conditioning experience (vis-à-vis the control-group experience) explains 4 percent of the variance in subjects' attitudes toward the brands serving as conditioned stimuli.[3] This small amount of explained variance may seem a pittance but actually is in line with much of consumer research and behavioral research in general (cf. Peterson, Albaum, and Beltramini 1985). It must further be recognized that the unconditioned stimuli in our experiments are relatively pallid and not particularly salient in comparison to the types of associative stimuli in the actual marketplace. That is, the water scenes serving as unconditioned stimuli are attractive and perhaps even enchanting, but they cannot begin to compare with the "approach power" of enticing fragrances, beautiful people, or the alluring qualities of audiovisualizations prominent in many television commercials.

A second generalization from our experimental program relates to the roles performed by the CS brand and the embedding context in which conditioning trials take place. We expected to obtain significant conditioning effects for unknown and moderately known brands of cola but not for well-known brands. Our results indicate that filler context had minimal impact except when moderately known brands (RC or Shasta Cola) served as the CS. It appears that these known, unambiguous colas were contrasted with the exemplar brands, which thus made it relatively easy in the known context but difficult in the unknown context for the conditioning treatment to enhance the conditioning groups' evaluations to a statistically significant level above the control groups' evaluations.

Turning to the effect that type of CS brand had on the amount of conditioning, our results support basic conditioning theory in showing that conditioning effects for the well-known, familiar brands, Coke and Pepsi, were considerably less than for the moderately known or unknown brands. This finding represents a human conditioning analogue to the conclusion from basic animal conditioning research that "familiarity breeds not contempt, but indifference" (Schwartz 1989, p. 72). However, it is important to recognize that demonstrating strong conditioning effects for familiar (vs. unfamiliar) brands is more difficult because of the potential operation of ceiling-effect bias. Because consumers already have highly positive attitudes toward familiar brands—Coke and Pepsi in our case—it is difficult to increase the conditioning group's mean attitude significantly above the already-positive attitudes that control-group subjects bring to the experiment.

Mere Contingency Awareness or Demand Artifact?

Merely being exposed to the CS-US contingency did not assure that subjects would form favorable attitudes toward the CS. Instead, subjects had to pay enough attention to the sequencing structure to learn the CS-US contingency and hence to form a favorable attitude toward the CS brand. Subjects in the conditioning groups who were unaware of the contingent relation held no more positive attitudes toward the CS brands than did subjects in the control groups in which no contingent relationship existed. Our results may thus be looked on as supporting either of two explanations, one being that contingency awareness is a necessary condition for conditioning and the other that the results are due to demand artifacts. It is difficult to ever discount entirely a demand-artifact explanation, especially in view of the fact that efforts to measure the presence of demand artifacts are themselves subject to demand artifacts (Gorn et al. 1987). Yet, a demand-artifact explanation for our results seems untenable inasmuch as such an explanation would not account for why significant conditioning effects were detected in some experiments but not in others. Perhaps the best proof of this is the set of five experiments with moderately known brands. No demand-artifact account could possibly explain why strong conditioning effects were obtained when RC and Shasta were embedded among well-known filler brands but no evidence of conditioning occurred when efforts

[3]The value of 4 percent is obtained by transforming the unreliability-corrected average *d*-statistic of .40 to a Pearson correlation, which when squared equals the amount of explained variance between the independent variable (conditioning or control group) and the dependent variable (Wolf 1986, p. 35). Parenthetically, it is noteworthy that the explained variance in the 11 statistically significant experiments is 8 percent ($\bar{d}$ = .59), and, when contingency-unaware subjects are excluded from analysis, explained variance increases to 12 percent ($\bar{d}$ = .74).

were made to condition these same brands in the context of unknown filler brands.

We are confident that our results are something other than artifactual. We are equally convinced that, with our experimental paradigm, contingency awareness is essential for conditioned learning. Furthermore, we concur with Allen and Janiszewski (1989), Petty and Cacioppo (1981), and the long line of scholars before them (for citations, see Allen and Janiszewski 1989, p. 37) who have argued that Pavlovian conditioning is cognitively mediated learning. Assignment of subjects to a conditioning group does not assure all will be conditioned, because some will not devote the level of attention necessary to become aware of the CS-US contingency (Staats 1969). The only subjects who were conditioned in our experiments were those who payed enough attention during the course of the experiment to discern the CS-US contingency.

Signal Learning or Evaluative Conditioning?

We have shown that conditioned attitudes are significantly more favorable in subjects who are aware (vs. unaware) of the CS-US contingency and have argued that contingency awareness is necessary within our paradigm for conditioned learning to occur. It should be noted, however, that our results (along with Allen and Janiszewski's [1989]) are in conflict with some influential conditioning research from Europe. Martin and Levey in Great Britain (1985, 1987; Levey and Martin 1983) and Baeyens and his colleagues in Belgium (Baeyens et al. 1988; Baeyens et al. 1989; Baeyens, Eelen, and Van den Bergh 1990) have conducted various experiments claiming evidence of attitudinal conditioning in the absence of subject awareness. These European researchers have argued that their results evidence evaluative conditioning (i.e., a hedonic shift from US to CS) and not merely serial learning (i.e., where the subject acquires knowledge that a CS systematically precedes the occurrence of a US). This conclusion, although not undisputed (see Shanks and Dickinson 1990), is important in its suggestion that affect for an initially neutral object can be formed automatically without conscious awareness of the contingent relation between conditioned and unconditioned stimuli.

The disagreement between our findings and those from Europe on the role of contingency awareness is initially troubling. It is difficult not to ask who is wrong. However, comparison of the differences in the experimental conditioning paradigms may suggest that neither research group is in error. Whereas the paradigm used by the European researchers makes it difficult for subjects to learn that a particular CS predicts a particular US, our paradigm makes it difficult for any attentive subject not to learn the contingency between CS and US. Notable points of distinction are that the European paradigm (1) uses pictures of human faces as both conditioned and unconditioned stimuli; (2) presents multiple combinations of neutrally valenced human faces (the CSs) paired with highly liked, disliked, and other neutral faces (the USs); (3) conducts sessions with one subject at a time; (4) displays each slide on the screen for just 1 second; (5) uses a longer interval between CS and US presentations (4 seconds vs. our interstimulus interval of virtually zero); (6) uses fewer trials than we do (10 vs. our 20); and (7) employs a different procedure for assessing contingency awareness.

Baeyens et al.'s (1988, 1989, 1990) paradigm, on the one hand, restricts the likelihood that subjects will correctly discern the multiple CS-US contingencies contained in their slide presentation. (In fact, the level of contingency awareness is considerably lower in their experiments, ranging from 18 to 36 percent, than in ours; see Baeyens et al. 1988, p. 189.) Yet, on the other hand, their paradigm allows for the possibility that arguably attentive subjects—who, it will be recalled, are run individually rather than in groups—will process CS-US contingencies in an automatic (vs. controlled) fashion and thereby be evaluatively conditioned. In other words, whether or not subjects in Baeyens et al.'s (1988, 1989, 1990) experiments are consciously aware of the CS-US contingency, the possibility nonetheless remains that they will be conditioned since they are attentive and hence potentially conditionable.

Comparatively, because of the fact that we ran subjects in groups and many of the slides are inherently uninteresting, not all of our subjects pay close attention during the course of an experimental session. Consequently, only those who have allocated sufficient attentional capacity stand a chance of being conditioned, and it so happens that these subjects while being attentive are likely to discern the critical CS-US contingency. Thus, contingency awareness in our experiments appears to operate as an index of attentiveness and hence conditionability, whereas in Baeyens et al.'s (1988, 1989, 1990) experiments attention is present with or without contingency awareness.

Conclusion

A. sociative learning is prevalent in the marketplace. Evaluatively positive (and sometimes, unintentionally, negative) stimuli are continuously juxtaposed against brands and other consumption objects. Consumers learn these associations either mindlessly or consciously. In either event, much of what consumers feel and think about consumption objects is inevitably due to associative learning. Yet as a discipline, consumer researchers have focused greater emphasis on studying more analytical forms of learning. There are enough demonstrations in our literature to accept *the fact* of classical conditioning of consumers' attitudes toward consumption objects. More effort now needs to be focused on better understanding the process and boundaries of conditioning (cf. Allen and Madden 1985). Just as our discipline began during the 1980s to recognize

the importance of studying other nonmainstream topics such as affect (Zajonc 1980; Zajonc and Markus 1982) and experiential aspects of consumption (Holbrook and Hirschman 1982), we believe the time is ripe to accelerate the study of classical conditioning and associative learning more generally.

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