Measuring Advertising Effectiveness
In Destination Marketing Strategies

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The use of true experiments is advocated for testing destination advertising effectiveness in causing visits. Examples of A-B ad copy split tests in other industries are described and the value and limitations of advertising conversion research studies are reviewed. A hypothetical example in tourism advertising effectiveness research of an A-B-C split is presented, C being a control group of subjects not exposed to advertising. A call for travel and tourism industry action to test advertising's influence on causing visits and revenues concludes the article.

Does advertising increase sales? Do the advertising expenditures by New York, California, Texas, Hawaii, Nova Scotia, Ontario, and other states, provinces, and countries to attract visitors actually cause visitors to come to these destinations? Although scientific research methods are available to test advertising’s impact on sales (cf., Campbell and Stanley 1963; Banks 1965; Ramond 1966) and have been applied in several industries (e.g., Banks 1965; Caples 1974; Donath 1986; Eastlack and Rao 1988), no scientific study is available in the travel research and destination marketing literature on whether advertising causes visitors.

A review of the travel and tourism literature indicated that apparently no city, state, province, or country has done accountability research on the question of whether advertising causes visitors. While tourism “advertising conversions research” studies (e.g., Woodside and Soni 1988) are helpful in comparing the performance of different ads, media vehicles, and media, such advertising conversion studies do not address the more basic questions: (1) does advertising cause sales (i.e., would the inquirers produced from the advertising who visit the destination have visited even without sending the inquiries?), (2) if advertising is a cause of visits, how much does advertising cause visits, and (3) what is the net profit on investment of the advertising expenditures for the management or government of the destination?

The purpose of this article is to advocate performing true field experiments on the effectiveness of advertising in causing visits. The key points are that (1) given that destination marketing managers for Ontario, California, Australia, and other travel/tourism destinations are responsible for allocating substantial advertising funds to attract visitors, true tests of advertising’s impact on causing visitors can be built into their plans without incurring substantial additional costs; and (2) while advertising conversion studies are valuable for evaluating alternative messages and media vehicles, they are inadequate substitutes for true field experiments to learn the effect of advertising on sales.

A brief description of designs for true experiments is presented in the second section of the article. A real-life example of applying such designs to test advertising’s influence on sales is discussed in the third section. Details on the improvements made in advertising conversion research since the early 1970s are described in the fourth section; why these improvements do not go far enough is also discussed. In the fifth section specific recommendations for destination-marketing managers are offered for measuring advertising effectiveness in causing visits; a call for industry action concludes the article.

TRUE EXPERIMENTS ON ADVERTISING’S EFFECTS ON SALES

Two basic requirements must be met for a true experiment to learn the effects of some treatment variable, such as advertising expenditures to attract visitors, on some dependent variable, such as causing tourists to visit a given destination.

First, at least two groups must be used: a treatment group and a control group. In an advertising test, the treatment group is exposed and the control is not exposed to the advertising.

Second, in true experiments subjects are randomly assigned to groups before the two or more treatment levels (or conditions) are administered. Thus, the groups have been made equivalent before the start of the experiment and the observed differences between the two groups (if any), such as the differences in proportions of visitors to the destination, following the exposure of one treatment group to the advertising are due to the advertising.

The basic issue is not which statistical procedure is used to test the significance of differing results of two experimental groups (e.g., treatment and control groups) but whether the research conditions permit the interpretation that the differences arise solely from the experimental treatments applied. If one is satisfied that the observed differences are real and do derive from the differing treatments applied to different groups, the comparisons among the different groups’ tests can be shown in percentages, in ratios, in graphs, as bar charts, or in any other way the researcher desires (Banks 1965, p. 31).

This basic true experiment is identified in the behavioral, medical, and natural sciences as a post-test only with control group (and random assignment of subjects) design. Note that a pre-measurement is not necessary to track the influence of
the treatment variable on the dependent variable; in fact, using a pre-measurement may cause an “inflammation or testing effect,” that is, an attitude or behavior change observed due to the subjects becoming sensitized to responding to a survey. In the 1950s, to prevent an instrument effect a double-blind technique was used in medical research to test the usefulness of both the Salk and Sabin polio vaccines: neither the patient nor the examining physician knew whether the patient received a drug or a placebo.

The initial use of true experiments in advertising research was the A-B copy split. In a true A-B split, every other copy of the publication would contain ad A, and every other copy would contain ad B. This advertising research technique meets the same requirements of the scientific method that Luther Burbank and Sir Ronald Fisher used in comparing the germination of two seeds under identical conditions (cf., Rapp and Collins 1987, p. 105; Fisher 1949).

A-B-C splits are also possible, with C being no ad in every third copy of the media vehicle. An A-C split or B-C split would be comparing each ad (A and B) against the control condition of no advertising (control condition C). A-C and B-C split tests are examples of basic “advertising weight” experiments; such designs provide valid answers to the question of whether the advertising caused sales (or visits). A more detailed example of this design is described in a following section of this article.

Split testing methods are being applied to all forms of media, including print, outdoor, radio, and television (via split-cable). Instead of splitting households or issues of a newspaper or magazine vehicle, in some cases markets are split. For example, 20 markets are exposed to ad A and 20 markets are exposed to ad B or to C, the no-ad control treatment. In the time period following the ad, the average sales among the 20 markets receiving ad A are then compared to the average sales among the 20 markets receiving ad B or no ad. Before treatments A, B, or C are administered, the 40 markets are divided randomly into two groups, with half the markets receiving treatment A and half receiving treatment B (or treatment C). Thus, the two groups of 20 markets each have been made as equivalent as possible before the treatments are administered. Their equivalence can be checked by testing for relevant differences in population or average household incomes between the two groups (using Sales and Marketing Management’s annual “Surveys of Buying Power”).

After running the ads (A versus B) and/or the no ad (C) treatments, the average sales levels between the two market groups are compared. Any observed difference in sales is advertising’s influence on sales. Such a true experimental research design controls for all known sources of internal and external invalidity (cf., Banks 1965)—that is, controls for possible factors that could influence sales. We know that many other factors besides advertising are likely to influence sales, but the conclusion is false that advertising’s influence on sales alone cannot be measured accurately. These other factors that influence sales would be expected to be present in all markets—not only the markets receiving ad A or ad B treatments but also in the market receiving treatment C, the no-ad treatment. During the course of the experiment, all the markets included in the study should be monitored closely to measure the presence of other factors appearing in some, but not all markets, such as unique advertising activities by competitors done to disrupt the true experiments.

APPLICATION OF TRUE EXPERIMENTS

In the mid-1980s, four true experiment field tests to measure advertising influence on sales were designed and run for four different industrial products (cf., Donath 1986). The products are described in the findings of the study as a disposable safety product, a commercial construction material, a commercial transportation component package, and highly specialized laboratory equipment.

Executives of the Advertising Research Foundation (ARF) and the Association of Business Publishers (ABP) formed a joint committee to plan and implement the tests. Three advertising levels were used during the one-year test period for each product: low-, medium-, and high-weight. For example, subjects in the low-weight treatment were exposed to eight ad pages during the year; subjects in the medium-weight treatment were exposed to 14 pages; subjects in the high-weight treatment were exposed to 28 pages.

All the trade magazines participating in the tests segmented their controlled subscription lists into three equal groups for light, medium, and heavy advertising schedules. They selectively bound inserts into copies mailed to each test group—so some readers got twice as many of the same ads as others, for example. Manufacturers of the four products paid for incremental space at half the standard page rate, and test publishers further contributed by picking up additional out-of-pocket costs.

The research company handling the project drew a sample of companies for telephone interviewing from the light-, medium-, and heavy-ad groups, handled toll-free phone inquiries from ad readers, surveyed prospective buyers, and analyzed the results.

Measures of advertising effectiveness included unaided awareness surveys via telephone interviews, inquiry response rates, sales, and profit impact. Careful track was kept of other known parts of the industrial selling process—the direct sales, the distributor support, the competitive pressure, the trade show exposure—to learn whether they remained equal in the light, medium, and heavy advertising test groups. Statistical methods such as covariance analysis are available to separate these other variables for testing advertising’s influence on sales if the levels of the variables are unequal between the three groups. In addition, ARF kept the test cell identities of potential customers a secret from manufacturers and publishers; thus, a triple-blind procedure was used: the potential customers, manufacturers, and publishers all were unaware as to what treatment level was being administered to what subjects. The potential customers were not informed that they were included in the advertising weight tests.

The following is a summary of the findings for the advertising impact of one of the products. For the disposable safety product, an important result of the year-long test was that sales climbed to 500% in the high-weight level group versus 200% in both the low- and medium-weight level groups. Reader service (bingo card) inquiries were 5.8 per 1,000 subscribers for the high-weight group; the rates per 1,000 subscribers were 4.5 and 3.2 for the moderate- and low-weight groups, respectively. The heavy versus the medium advertising produced a marginal gain in unaided recall of the product category (46% awareness among the heavy group members versus 44% in the medium and 40% in the light groups). A substantial gain occurred in the percentage who correctly named the brand manufacturer (unaided brand awareness) in the heavy versus medium versus light ad groups: 25%, 17%, and 7%, respectively. (Unaided brand awareness [but not aided brand recall] has been found to be a useful “intermediate criterion” [Axelrod 1968; Haley and Case 1979] between advertising and sales: advertising that increases unaided brand awareness is advertising that is likely to increase sales [cf., Axelrod 1968; 1986; Haley and Case 1979]).
The ARF/ABP field advertising tests demonstrate the use of true experiments on the influence of advertising on sales. The research includes the use of test and control groups (the low advertising weight group was the control group) with random assignment of subscribers to groups, as well as use of multiple dependent measures of the impact of advertising.

**IMPROVEMENTS IN CONVERSION STUDIES**

Advertising conversion research is the most widely used method to measure the impact of advertising in the travel and tourism industry. However, Davidson and Wiethaupt (1989) correctly point out that considering accountability marketing research to be synonymous with conversion studies is a deadly sin in assessing the impact of advertising and other marketing efforts on sales.

Advertising conversion research studies include surveying a sample of inquirers generated from advertising and asking the surveyed inquirers whether they visited the destination they requested information from; the inquirers are surveyed several months after their inquiries are received from toll-free telephone calls, coupons appearing in the ads, and reader response cards.

Several substantial improvements have been reported in the travel and tourism literature on advertising conversion research (cf., Ronkainen and Woodside 1987; Woodside and Soni 1988) since the first publication of a tourism advertising conversion study in the *Journal of Travel Research* (cf., Woodside and Reid 1974). Two improvements are particularly important.

The first improvement is not identifying the destination which is sponsoring the study. Identifying the sponsor of the study to sampled inquirers is similar to telling a subject in a beverage attitude survey, “I represent Coca-Cola and would like to know if you like drinking Coca-Cola.” Identity is not revealed on the questionnaire, cover letter, or on envelopes to the inquirers selected for the study to avoid introducing unmeasured response bias into the findings; subjects who did not visit the sponsor’s destination have been found to be less likely to respond to the survey when the sponsor has been identified in the cover letter and in the questionnaire (Woodside and Ronkainen 1984). Unfortunately, the sponsors may have often identified to inquirers sampled in many tourism advertising conversion studies done in the 1980s.

For advertising conversion studies, an organization independent of the destination sponsoring the study should be identified as the sponsor of the study, such as a research firm or a university. Questions should be included in the survey instrument about travel to several destinations to avoid divulging the sponsor’s identity as well as to learn about competing destinations’ abilities to attract the sponsor’s inquirers.

The second improvement is incorporating methods to achieve response rates above 60 or 70% among the inquirers who are sent a questionnaire or telephoned. Consumer behavior studies show that nonrespondents usually differ from respondents in their buying and using behaviors, even when their demographics are similar (cf., Ferber 1977). Such differences have been noted in conversion studies (Ellerbrock 1981; Woodside and Ronkainen 1984). The several methods to increase survey response rates include making up to three and four attempts to reach subjects selected in the original sample, since multiple attempts to reach the original sample of inquirers have been found to have the largest impact on reducing the proportion of nonrespondents (cf., Heberlein and Baumgartner 1978; Kanuk and Berenson 1975).

Another improvement suggested by Ellerbrock (1981), Ronkainen and Woodside (1987), and Davidson (1989) is of dubious value for improving advertising conversion research studies. This recommended improvement is factoring out inquirers who had decided to visit the destination before the ad exposure. This recommendation assumes that respondents can accurately report the information sources used, the source’s relative influence and sequence of events in their decision process in making purchase decisions. However, this proposition is not supported by available evidence (cf., Sudman and Ferber 1974; Newman and Lockeman 1975; Weitz and Wright 1979).

Inquirers sampled in a tourism advertising conversion study might be assumed to recall and accurately report whether they visited specific destinations on vacation-related trips within a recent time period, such as visiting each of a few given U.S. states or Canadian provinces. However, they may be unlikely to recall accurately whether they made the decision before or after receiving requested destination-related literature, or how helpful the literature requested was in planning their trips. In one study, only 56% of the inquirers responding to an advertising conversion study, surveyed a few months after information had been sent to them, reported receiving travel destination information they were known to have requested (cf., Woodside and Soni 1988).

For testing whether advertising causes visits to a given destination, making downward adjustments in the inquiry/visitor conversion rate by factoring out inquirers who report they had planned the trip before receiving the literature or being exposed to the ad is a poor substitute for doing a true experiment. Factoring out respondents who do not recall receiving the literature requested may also be used in assessing the value of inquirers by mode of response or in making media performance comparisons. For example, in a study of modes of inquiries Woodside and Soni (1988) found that 71% of toll-free inquirers compared to 49% of reader-service card inquirers reported making information requests they were known to have made.

The legitimate purpose of advertising conversion studies is to compare the relative performance of different advertising campaigns (cf., Woodside and Motes 1981), media vehicles (cf., Woodside and Ronkainen 1982), or modes of inquiries. Tourism advertising conversion studies cannot estimate whether advertising causes destination visits because such studies are not true experiments.

Given (1) the continuing requests by legislators and governors (and other executives and administrators who approve advertising budgets) for evidence that advertising causes visits and (2) that true experiments can be designed to be run as part of the advertising plan in a given advertising budget (so that additional expenditures cannot be used as a reason for prohibiting such testing), A-B-C splits are recommended to state, province and other destination-marketing managers to gain evidence of advertising’s influence on visits.

**SPECIFIC RECOMMENDATIONS TO DESTINATION-MARKETING MANAGERS**

The following five specific recommendations are made for destination-marketing managers who want accurate estimates of whether advertising causes visits to their destinations. First, request the cooperation of one or more media vehicles to use A-B-C advertising splits of a sample of magazine or newspaper subscribers or cable television viewers. Such cooperation is possible (cf., Donath 1986; Rapp and
Collins 1987; Bogart 1984) and serves the long-run interests of both advertisers and media representatives.

Second, use large sample sizes—1,000 print-media subscribers per A-B-C group—because the size of effect of advertising on sales is small compared to other sources of influence, such as demographic variables (cf., Ramond 1966). "Effect size" describes the magnitude or strength of the relationship among two or more variables in the population. The effect size of advertising’s influence on causing sales is likely to range from zero to 5%, that is, zero to 5% of the variation in sales may be found to be caused by sales. However, an increase of even 1% in sales due to advertising is likely to have a substantial positive impact on profitability (cf., Bogart, Tolley, and Orenstein 1970). When the true effect size is known to be small between two variables (for instance, less than 5%), then large sample sizes are necessary to detect such effects (cf., Cohen 1977; Sawyer and Ball 1981).

Third, begin by doing an A-B-C split experiment to provide information on the impact of two different advertising message-executions (ad A versus ad B) against a control group (group C). Such a test includes the most basic advertising weight test: advertising versus no advertising treatments. The research hypotheses being tested are (1) that advertising causes more visits to the destination than no advertising, that is, the proportions of visitors from groups A and B are higher than the proportion of visitors from group C, and (2) that the two ads may not be equal in performance in causing visits. Substantial evidence exists that supports the second hypothesis which, combined with testing several advertising weight levels, has lead to the conclusion that creative copy is likely to have more impact than added media weight (cf., Eastlack and Rao 1989; Dodson 1989; Gross 1967).

Fourth, carefully track inquiries received from the subjects in each of the three groups (A, B, and C): toll-free telephone inquiries, ad coupons, reader service cards, personal letters, and phone calls. Monitor the possibility that one group is exposed to other ads for your destination or competing destinations more or less than the other groups. Such a possibility is unlikely to occur if random or systematic assignment (every third name) of subjects to groups is made across several metropolitan markets; thus, the subjects in all three groups are equally likely to be exposed to non-controlled variables during the test. Analysis of covariance may be used to separate the treatment variable effect (ad A versus ad B versus no advertising, C) from other factors likely to influence the dependent variables, such as visits (cf., Banks 1965, pp. 126-28).

Fifth, collect destination expenditure information from subjects reporting visits to the destination from each of the three groups. Such information is useful for estimated revenue associated with each test group (A, B, and C). Note that not everyone agrees with this recommendation; for example, Davidson (1989) makes the following observation: "I must question the use of ranges as a valid method of measuring trip expenditures. Again, our work shows a great deal of memory failure when recalling travel expenditures." A destination visitor may be unable to report expenditures with an accuracy of a few dollars. However, with prompts about accommodations used, food and restaurant purchases, shopping purchases, and admission fees the visitor is likely to know if she/he spent bracketed amounts at the destination such as zero dollars, $1 to $50, $51 to $100, $101 to $200, $201 to $400, $401 to $800, $801 to $1,600, $1,601 to $3,200, or more than $3,200. The benefits resulting from including such information are likely to be greater for estimating and comparing destination expenditures among the three groups than the benefits from deciding not to include expenditure estimates in the study.

For an example, assume that in an A-B-C split test a married couple reports a vacation visit to Chicago for a four-night stay at the Marriott Hotel on North Michigan Avenue at $135 per night. During their visit the husband buys a suit for $400 and the wife goes to a Cubs baseball game ($8) with friends; they have two dinners at restaurants ($80 for the two of them for each dinner) and eat fast food meals ($75) at other times during their visit. The couple went by train from and back to the airport ($1 per trip per person) and walked to the restaurants from the hotel. For the Chicago trip, this couple reports their total destination expenditures to be between $801 and $1,600. While their actual destination expenditures are likely to have been higher than the $1,200 midpoint in this range, such expenditure reports provide information useful for estimating total expenditures associated with each group (A, B, and C) and for comparing total destination expenditure estimates between groups. Their reported air travel expenditures might be split between the origin and destination city or not included in estimating destination expenditures. An example of a basic advertising weight experiment with hypothetical results is summarized in Exhibit 1 and Table 1. The design described in the exhibit calls for random assignment of subjects into groups A, B, and C; this design could be modified by using systematic assignment. After either random or systematic assignment of subjects to groups, the geographic distributions of the three groups could be checked to insure that they were similar; demographic and general media exposure information collected from each group will serve as a further check that the three groups are similar.

For measuring travel behavior, telephone surveys or mail surveys could be used. Again, the sponsor of the study should not be revealed to the subjects and, using follow-up questionnaire mailings, high response rates to the survey should be

<table>
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<tr>
<th>EXHIBIT 1</th>
<th>EXAMPLE OF AN ADVERTISING WEIGHT TEST</th>
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<tbody>
<tr>
<td><strong>Design:</strong></td>
<td>30,000 magazine Y subscribers are selected; each subscriber selected is assigned randomly to one of three treatment groups: A, B, or C.</td>
</tr>
<tr>
<td><strong>Treatments:</strong></td>
<td>Group A: exposed to advertising message A (e.g., full page with ad copy of “FUN IN THE SUN AT DESTINATION X”). Group B: exposed to advertising message B (e.g., full page with ad copy of “RELAX WITH YOUR FAMILY AT DESTINATION X”). Group C: exposed to no advertising message.</td>
</tr>
<tr>
<td><strong>Measurements:</strong></td>
<td>(1) Total coded inquiries received from each group from all sources (toll-free telephone inquiries, ad coupons, reader service, personal letters, and department telephone calls) are tabulated. (2) “Unaided” telephone survey to 1,000 subscribers in each group (A, B, and C). (3) Estimated expenditure levels of visitors to destination X from each group are measured via telephone interviews.</td>
</tr>
<tr>
<td><strong>Results:</strong></td>
<td>(1) One advertisement performs better than the other in increasing awareness, inquiries, visitors and expenditures (e.g., 10 visitors per 1,000 subscribers in group A versus 6 visitors per 1,000 subscribers in group B). (2) the three treatment groups perform better than the control group in causing awareness, inquiries, visits and expenditures.</td>
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TABLE 1
HYPOTHETICAL RESULTS FROM TESTING ADVERTISING INFLUENCE ON SEVERAL DEPENDENT VARIABLES

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>Treatment Group</th>
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<tbody>
<tr>
<td>1. Total inquiries per 1,000 subscribers</td>
<td>A</td>
</tr>
<tr>
<td>2. Unaided awareness of destination X as a vacation destination (A) Top-of-Mind Share</td>
<td>30</td>
</tr>
<tr>
<td>(B) Consideration</td>
<td>12%</td>
</tr>
<tr>
<td>20%</td>
<td>15%</td>
</tr>
<tr>
<td>3. Travel party visits to destination X per 1,000 subscribers</td>
<td>9.0</td>
</tr>
<tr>
<td>4. Estimated travel party's expenditures at destination X</td>
<td>$400</td>
</tr>
<tr>
<td>5. Total estimated expenditures at destination X per 1,000 subscribers</td>
<td>$3,600</td>
</tr>
<tr>
<td>6. Net tax revenue per 1,000 subscribers (10% of revenues)</td>
<td>$360</td>
</tr>
<tr>
<td>7. Total advertising cost per 1,000 subscribers</td>
<td>$40</td>
</tr>
<tr>
<td>8. Net tax revenue per advertising cost</td>
<td>$9.0</td>
</tr>
<tr>
<td>9. Net tax revenue per 1,000 subscribers</td>
<td>$320</td>
</tr>
</tbody>
</table>

achieved from all three groups. The predicted results from such a study are summarized in Exhibit 1; detailed hypothetical results appear in Table 1.

Notice in the table that a few inquiries are made from each group: 3.0% from Group A, 1.0% from Group B, and 0.5% from Group C. Having some inquiries from Group C reflects the likelihood that destinations sometime receive substantial numbers of inquiries that are not associated with ads for the destination or some inquiries are impossible to track back to a particular ad. The 3% inquiry rate among subscribers shown for Group A is very high compared to typical inquiry rates of less than 0.5% for ads containing coupon offers for additional information. Given that high versus low inquiry rates are likely to be associated positively with high awareness and purchase rates (i.e., visits), ad A is likely to be more successful than ad B or the no-ad C treatment in causing visitors. This proportion is confirmed in the additional hypothetical findings reported in the table.

If a telephone interview method is used, unaided destination awareness information could be collected to compare the influence of the ads in increasing top-of-mind awareness among the subjects, as well as the ability of the ads in influencing whether the advertised destination is included in the subjects' consideration set of destinations for vacation-leisure visits (see the data for the second dependent variable in the table). The hypothetical results indicate that ads A and B do cause an increase in awareness and consideration for the destination compared to the control group.

For the third dependent variable, an estimated nine visits occurred for the 1,000 subscribers in Group A, which is a visitation rate substantially higher than for Groups B and C. The visitation rate for Group B is substantially higher than for Group C (two versus one-half per 1,000 subscribers).

For the fourth dependent variable, the estimated travel party expenditures at the destination are equal for Groups A and B but higher than for Group C. These hypothetical results are shown because the literature sent to the inquirers who did visit probably informed these visitors about the many activities at the destination, activities that visitors from Group C may be unaware of doing.

Multiplying travel party visits by estimated travel party expenditures produces total estimated expenditures at the destination (for Group A: 9.0 by $400 = $3,600) as shown in the table. Given that the destination being advertised is a city, province, or country, then net tax revenues from visits caused by the advertising should be estimated; note that a 10% tax revenue on expenditures is used in the table.

In the table, the cost for advertising to reach Groups A and B is $40 and the net revenue per advertising cost indicates that both ad A and ad B are estimated to have produced higher net revenues per advertising expenditures. Ad A performs at a substantially higher level than ad B in estimated net tax revenues per advertising expenditure, $9 versus $2.

These hypothetical findings illustrate the conclusions that advertising is beneficial for the destination and that ad A execution performs better than ad B execution in increasing awareness, visits, and net revenues.

CALL FOR INDUSTRY ACTION

The purpose of this article has been to advocate tests to answer the following question: Does destination advertising increase visits? Research design are available—that is, true experiments in the form of A-B-C splits—to provide valid answers to this question. These designs have been used in other industries. Such research designs can be incorporated into the advertising campaigns for destination marketing programs. The research costs for such true experiments of measuring advertising's effect on sales are estimated to range from 3 to 7% of media vehicle expenditures: 3% for larger advertising budgets (more than $1 million) and 7% for small advertising budgets (less than $500,000).

The formation of an industry task force on advertising and marketing accountability research by the U.S. Department of Commerce (cf., Wynegar 1989) indicates growing recognition that the destination advertiser must assume responsibility for measuring advertising's impact on increasing visits. Tourism advertising budgets are very visible, and each year are often vulnerable line items in many city, state, and provincial governments. While advertising conversion studies are useful for comparing media vehicle and ad message performances, such studies do not address the more basic question. The methods are available and the time has come to estimate advertising's impact on causing visits and producing revenues.

Advertising agencies do not often lead the way in encouraging field tests of advertising performance in increasing sales. However, advertising agencies are likely to cooperate fully in such tests to insure that measurements are done correctly and, given that advertisers insist on doing such research.

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JOURNAL OF TRAVEL RESEARCH 7


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