Scaling Social Impact

New Thinking

Edited by

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SCALING SOCIAL IMPACT
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First published in 2010 by
PALGRAVE MACMILLAN®
in the United States—a division of St. Martin's Press LLC,
175 Fifth Avenue, New York, NY 10010.

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ISBN: 978–0–230–10437–2

Library of Congress Cataloging-in-Publication Data

Scaling social impact : new thinking / Paul N. Bloom, Edward Skloot, editors.
p. cm.
Includes bibliographical references and index.
ISBN 978–0–230–10437–2
1. Social change. 2. Social entrepreneurship. 3. Social marketing. 4. Nonprofit organizations. I. Bloom, Paul N. II. Skloot, Edward.

HM831.S353 2010
303.48—dc22 2010003250

A catalogue record of the book is available from the British Library.

Design by Newgen Imaging Systems (P) Ltd., Chennai, India.

First edition: October 2010

D 10 9 8 7 6 5 4 3 2

Printed in the United States of America.

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Methodological Issues and Challenges in Conducting Social Impact Evaluations

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Conducting meaningful research, or "research that matters," is an important and potentially rewarding activity for many researchers. This type of research has been described as social impact research, social entrepreneurism, social marketing, altruistic marketing, and transformative consumer research (Mick, 2006), among other terms. Research topics that matter are abundant, including smoking, obesity, gambling, alcohol and drug addiction, AIDS, malnutrition, environmental damage, poverty, lack of health coverage, and illiteracy including financial and health illiteracy to name just a few.

In pursuing research that matters, researchers often conduct studies to evaluate the effectiveness of social marketing campaigns, social entrepreneurship programs, or related social initiatives. We will refer to these as social impact evaluations, and they will serve as our focus. They are often done on behalf of government entities, nonprofit groups, or firms. Successfully measuring the effectiveness of social initiatives that affect large segments of a society is an extremely important endeavor (Bloom, 2009) and focusing on behavior change is arguably the ultimate objective (Andreasen, 1994).

However, there can be numerous methodological challenges and problems inherent in such studies that can limit their generalizability and/or their ability to prove causal effects. In many instances, social impact evaluations are subject to research design trade-offs; that is, methodological decisions that increase some aspects of a study's internal or external validity also unfavorably impact other aspects (Cook and Campbell, 1979). While all research involves tradeoffs, they seem to be especially visible and problematic in these evaluations because of the high stakes involved in determining whether a social initiative worked or not, and thus whether it merits further funding.

The purpose of our chapter is to critically evaluate two key aspects of social impact evaluations: design and implementation. More specifically, we first discuss the technical aspects of designing or planning the research. Then, we identify four main design options available to researchers, give examples of each, and discuss the pros and cons of each. These include both expensive, comprehensive, multiyear designs and more limited designs that can be used when there is a shortage of money and/or time.

Under implementation, we examine the human aspects of carrying out social impact research and dealing with the various stakeholders. In particular, we explore some of the challenges in working with different constituencies with widely varying perspectives on the social initiative, in terms of goals and processes. For instance, it is often necessary to balance the conflicting recommendations and mandates of government agencies, funding agencies, advertising agencies, marketing research firms, public relations firms, legislators, government oversight groups, nonprofit groups, consumer advocacy groups, diverse consumer groups, outside evaluators, and/or academic and practitioner consultants. We then identify several major sources of conflict and discuss potential solutions by drawing on real-world examples from our own research and consulting.

Our background includes work with the Office of National Drug Control Policy on their antimarijuana ad campaign; Health Canada, Massachusetts and Wisconsin on their antismoking ad campaigns; the Risk Communication Advisory Committee of the Food and Drug Administration; the Federal Trade Commission; and the U.S. Census Bureau. In this chapter, we focus on research that we conducted or followed in evaluating the different anti-drug and antismoking campaigns. However, the designs and conflicts we discuss should be broadly applicable to other types of social impact evaluations.

Research Designs for Evaluating Social Impact Programs

The major design dimensions that we consider are whether to conduct experiments or quasi-experiments, and whether to use a lab or field research setting. We do not examine nonexperiments because they cannot assess causal impact (Cook and Campbell, 1979). By experiments, we mean that exposure to the campaign or program is manipulated; the program runs in some communities or during some time periods (the test), but not others (the control). By quasi-experiments, we mean that there is no manipulation of exposure involving test and control groups, or we mean that exposure effects are compared across demographic subgroups that precludes random assignment to subgroup (e.g., participants cannot be randomly assigned to a gender subgroup; they are born into one). This makes it more difficult to assess the causal impact of exposure or why the impact may differ by demographic subgroup, but other approaches can be taken

Table 12.1 The four main types of designs for evaluating social impact programs

Field setting		*Lab setting*	
Quasi-experiment	*Experiment*	*Quasi-experiment*	*Experiment*
Design 1: Full-scale evaluation	*Design 2:* Partial evaluation or field test	*Design 3:* Audience subgroup copy test	*Design 4:* Message or situational copy test

to allow causal inferences. The most common approach is to try to statistically equate the test and control groups and/or the demographic subgroups using covariates.

We will also consider the research setting dimension. By lab setting, we mean an artificial testing environment, such as a mall research facility or a middle school classroom, where nuisance variables such as distraction can be manipulated or controlled. By field setting, we mean a highly realistic testing environment, such as a mass media advertising broadcast, where such nuisance variables cannot be controlled. Crossing the two dimensions discussed here leads to the four designs in table 12.1.

Design 1: The Field Quasi-Experiment for a Full-Scale Evaluation

We now examine the field quasi-experiment (design 1), which permits a full-scale evaluation of a campaign or program in its final form under realistic conditions. Many government and nonprofit groups and firms prefer or even demand this design. However, it has some weaknesses including no manipulation of exposure and little control over nuisance variables. Steps must be taken to strengthen the design, in order to increase the validity of inferences about causal effects. We will discuss various case examples that differ in the steps taken. We will focus primarily on the design decisions; the original articles contain additional details about samples, measures, and analyses.

Case 1.1: National Youth Anti-drug Ad Campaign full-scale evaluation

A costly full-scale evaluation of the National Youth Anti-Drug (Antimarijuana) Media Campaign was conducted, using a quasi-field experiment (Hornik et al., 2008). The research questions were whether the campaign, which had already commenced, was effective among adolescents in terms of creating antimarijuana beliefs or reducing intent to use, or at lessening actual marijuana use. No steps had been taken to facilitate the evaluation, which is fairly common. No pre-campaign or baseline data had been collected to provide a control group, and the campaign had aired continually in every market so there were no apparent low-exposure markets or times to create control groups this way.

The researchers surveyed roughly six thousand adolescents annually in their homes; overall, the same adolescents were surveyed four times. It was found that 94 percent of youths reported seeing the ads, and the median frequency was two–three ads per week. In effect, all adolescents had been exposed to the same ad campaign and thus, technically, were in the test condition. However, the researchers used the level of ad recall to identify adolescents with different levels of exposure, and then used the low exposure level as the control and the higher exposure levels as the test. (See figure 12.1 for a stylized illustration of the design.) Since the low and higher exposure groups likely differed in their a priori marijuana use risk propensity, the researchers measured numerous covariates (150) to try to statistically equate the groups using propensity scores to preserve degrees of freedom (Joffe and Rosenbaum, 1999). For instance, differences in risk because of sensation seeking and religiosity were controlled for or factored out in the statistical models.

The study found mixed effects and concluded that overall the campaign was ineffective or even counterproductive. There were some positive effects; as the campaign progressed, antimarijuana beliefs strengthened, but it is unclear whether the campaign caused this or other factors such as general trends, other related-product campaigns (e.g., Truth), or random variation. There were some neutral effects; ad exposure did not predict marijuana beliefs or intent when measured at the same time. There were also negative effects in that higher recall of general antimarijuana ads sometimes predicted more intent to use one year later, and higher recall of specific ads sometimes predicted more pro-marijuana normative beliefs one year later.

However, it is unclear whether these negative effects can be attributed to the campaign. Research shows that ad effects occur within days or weeks of exposure and do not take a year to appear (Koyck, 1959), and this is the case even with pro-drug ads (Pollay et al., 1996) and antidrug ads (Emery et al., 2005). Further, reverse causality may have occurred (Hornik, 2002) in which adolescents who formed intent to use marijuana on account of other risk factors may have paid more attention to marijuana ads, but nonetheless maintained their intent due to those same risk factors. This would create an association between later intent and earlier ad exposure (also between earlier intent and later ad exposure, which was not assessed), but it would be driven by adolescents' own intent rather than the ads, and covariates may not have adequately controlled for this.

With such a potential reverse causality problem, it is unclear whether ad exposure caused intent or intent caused ad exposure. The reverse causality problem arises whenever self-reported ad exposure is used, because the high and low exposure groups are likely to differ fundamentally in terms of use intent; thus, they are noncomparable. In general, consumers with higher intent pay more attention to the ads and recall them better (Pechmann and Stewart, 1990). Therefore, one solution may be an independent measure of ad exposure that is unrelated to viewer's intent (e.g., ad GRPs).[1] This should ensure that ad exposure and prior intent are relatively uncorrelated. Or, one can include a true no-exposure baseline group or a true control

1.1 National Youth Anti-drug Ad Campaign Full-Scale Evaluation (Hornik et al., 2008)

1.2 Truth Anti-smoking Ad Campaign Full-Scale Evaluation (Farrelly et al., 2005)

C + T
C + T
C + C
C + C

and so on, across numerous markets

1.3 Other Full-scale Evaluations with Limited Exposure Variability

a) Pierce et al. (1998)

C + CC
C + TT

b) Emery et al. (2005)

T
C
T
C

and so on, across numerous markets

c) Pechmann et al. (1998)

CCCCCCCCTTTTTTT

2.1 University of Vermont Anti-smoking Ad Campaign Field Test (Flynn et al., 1992)

C + T
C + C
C + T
C + C

2.2 University of Kentucky Anti-marijuana Ad Campaign Field Test (Palmgreen et al., 2001)

CCCCCCCCTTTTCCCCCCCCTTTTCCCCCCCC
CCCCCCCCCCCCCCCCCCCCTTTTCCCCCCCC

3. Copy Test of Anti-drug Ads with Different Demographic Subgroups (Foley and Pechmann, 2004)

T—subgroup 1
C—subgroup 1
T—subgroup 2
C—subgroup 2
and so on for twelve demographic subgroups

4. Copy Test of Anti-smoking Ads with Different Messages (Pechmann et al., 2003)

C
T—message 1
T—message 2
T—message 3

and so on for each of seven message types

Key: T = test, C = control; different rows indicate different markets (geographies)

Figure 12.1 Illustrations of the designs used in various social impact evaluations.

group that is not exposed to the ads in question but is matched to the test group on original intent and demographics.

Overall, the Hornik et al. (2008) antidrug ad campaign evaluation was large and comprehensive. It had several positive features including large,

representative samples; a prospective or longitudinal design that tracked the same adolescents over time; and a host of reliable measures and covariates. However, this evaluation also had design limitations that should be avoided whenever possible. Most importantly, each evaluation could include at least one control group that actually receives lower exposure to the campaign or program. The control group should not consist of people who are exposed to the exact same campaign or program but for some reason report getting less exposure; this likely means they are fundamentally different and noncomparable and that their exposure was influenced by prior intent or behavior. Proper low exposure control groups can be found by looking for true exposure variability across markets or times, as discussed later.

Case 1.2: Truth antismoking ad campaign full-scale evaluation

A full-scale evaluation was conducted of the Truth antismoking ad campaign using a quasi-field experiment that employed a completely different design that was far less costly because it relied on secondary or preexisting data (Farrelly et al., 2005). The research question was, "Did the Truth campaign reduce smoking among adolescents?" The campaign had aired in all 210 U.S. media markets, but a careful analysis revealed that there was substantial variability in exposure because some markets (e.g., rural) did not have the smaller network stations (e.g., Fox) or cable stations (e.g., UPN) that were included in the media buys. Therefore, the adolescents in these markets could only see the major network ads and served as a low exposure control group (see figure 12.1). There was about a fivefold difference between the lowest and highest exposure groups; their annual GRPs were about two thousand and ten thousand, respectively, excluding outliers.

Statistical models examined whether exposure to the Truth ads during the first two years of the campaign (2000 and 2001), as measured by cumulative media market GRPs, reduced adolescent smoking. Smoking was assessed using the independent Monitoring the Future survey, which is an annual school-based survey of a representative sample of fifty thousand students across grades 8, 10, and 12. Additional models estimated the smoking trend during the control period of 1997–1999, before the Truth campaign commenced, also using Monitoring the Future. The statistical models included numerous individual covariates (e.g., demographics) as well as media market covariates (e.g., population size, education, income, cigarette prices, tobacco control spending, and market fixed effects).

The study indicates that the Truth campaign was effective; the higher the exposure level, the lower the smoking. It appears that the effect of the campaign continued to increase through ten thousand cumulative GRPs and then began to attenuate. Based on the trend during the control period (1997–1999), the smoking rate during the test period (2001–2002) should have declined from 25.3 to 19.6 percent and it actually declined to 18.0 percent; therefore it was concluded that 22 percent of the decline was due to Truth.

The Farrelly et al. (2005) evaluation was large, comprehensive, and inexpensive; and it included two separate control groups. Baseline control group data for the pre-campaign period was obtained from an ongoing survey that was used to estimate the pre-campaign decline and to show that the decline steepened during the campaign. In addition, the low GRP markets were used as a separate control group to examine what happened when GRPs increased. The high and low GRP markets differed in TV channel availability.

A study limitation is that the markets with low channel TV availability were probably more rural, and characteristics associated with rural markets (e.g., lower education) may have been related to smoking, but covariates were used to factor out these effects. It is preferable for exposure levels to be determined randomly by conducting a full-fledged experiment, but this was not feasible. Another concern is that the smoking rates, as measured by Monitoring the Future, were affected by many variables other than the ads, but this is also true of smoking behavior, and so the aim is to determine if ads matter despite these other factors.

Additionally, it cannot be determined if the students actually saw the ads that were aired in their media markets; GRPs are an imprecise measure of actual exposure. However, trying to assess actual exposure through self-reports can lead to the reverse causality problem discussed earlier; self-reported ad exposure may be highly correlated with intent and so it not possible to ascertain definitively whether ad exposure caused intent or vice versa. Finally, this study did not track the same people over time or prospectively, but doing so can lead to carry-over or demand effects (Cook and Campbell, 1979), and the cross-sectional samples should be comparable due to the sophisticated sampling used (Farrelly et al., 2005).

Case set 1.3: Other full-scale evaluations with limited exposure variability

Ideally, a full-scale field evaluation will have substantial exposure variability across numerous markets and times including both pre- and post-campaign periods, as in the Farrelly et al. (2005) evaluation. However, this is not always possible. Fortunately, a valid field evaluation can be conducted using data from just a few markets for the pre- and post-campaign periods. An example is Pierce et al.'s evaluation (1998) of the California antismoking campaign that involved just two geographies; it compared the smoking trend in California to the United States based on various surveys. Since the surveys and the geographies were not fully comparable, weights were used to try to equate the samples on gender, age, race, and education level. Within each geography, three time periods were examined: the years prior to California's campaign, the years of California's early campaign when it had high funding, and the years of California's later campaign when it had low funding. This design improved the study's validity because trends in each geography could be compared by period, to see if there were any effects that were unique to California that were not attributable to a

general trend over time (figure 12.1). The findings indicate that the early high-funded campaign caused a 50 percent more rapid decline in smoking than had occurred previously, and that this was unique to California, but that campaign efficacy waned when funding was reduced.

A valid field evaluation can also be conducted if there is variability across markets, but no variability across times, for example, no pre-campaign data. See Emery et al. (2005) for an example of this type of evaluation, involving state-funded antismoking ads (figure 12.1). The researchers purchased monthly Nielsen data on antismoking ad GRPs (targeted at adolescents) for seventy-five top media markets for two years when the ads were running. The researchers also purchased Monitoring the Future survey data on adolescent smoking for those same years. Then, the researchers estimated a model to determine if differences in ad exposure across the markets resulted in different smoking rates, after controlling for other market differences, such as cigarette prices and tobacco control policies via covariates. Ad exposure for the four months prior to the survey ranged from zero to roughly two, so exposure levels were low. Nonetheless, the findings indicate that higher ad exposure led to more ad recall, lower smoking rates, and fewer cigarettes smoked.

A valid field evaluation might even be possible if there is variability across times but no variability across markets, although this is the weakest of such designs. Pechmann et al. (1998) used structural time-series models to estimate Canadian smoking trends by age group, and the data spanned the years prior to and during Canada's antismoking ad campaign. With sufficient data points and structural time-series models, one can estimate the trend in behavior prior to a campaign that is independent of ad spending, and then see if the trend changes, that is, if a shock occurs, when the campaign starts (figure 12.1).

The problem with simple comparisons over time (single unit time series) is that anything else correlated with the trend in ad spending is often indistinguishable from the ad effect. For example, ad spending often starts when the problem is at its worst, which means that it will improve regardless. If a structural time-series model can estimate the peak and the subsequent decline that would have occurred without the ads, it can determine if there is a residual ad effect. However, this typically requires very large datasets, with hundreds of monthly or annual observations.

Design 2: The Field Experiment for a Partial Evaluation or Field Test

Next, we will discuss the field experiment (design 2) that permits a partial evaluation or field test of a campaign or program. However, the test takes place under experimental conditions that are carefully monitored, and it occurs in only a few markets or geographies. Government agencies that fund scientific research, such as the National Institutes of Health, often prefer experiments and especially randomized controlled trials. Experiments are considered to be the gold standard for assessing causal

effects (Cook and Campbell, 1979), but they are costly because primary or original data must be collected, and sometimes they cannot be carried out due to a lack of a true control market. Virtually all clinical drug trials involve randomized controlled trials, because quasi-experiments are considered inadequate.

However, experiments do have a substantial limitation: it is never entirely clear if the results will generalize to a full-scale campaign or program. For instance, the University of Kentucky antimarijuana ad campaign discussed subsequently produced impressive results (Palmgreen et al., 2001), and so it served as an initial template for the Office of National Drug Control Policy campaign evaluation that produced questionable results (Hornik et al., 2008). A host of decision-makers became involved in the national antimarijuana campaign, and the initial evidence-based template was modified in the hopes of improving it.

Case 2.1: University of Vermont antismoking ad campaign field test
University of Vermont researchers conducted a large field experiment of an antismoking ad campaign that complemented an antismoking school program (Flynn et al., 1992, 1994; Worden et al., 1988). The study was funded by the National Cancer Institute and it was a prospective or cohort study; the same 5,458 students were surveyed annually for five years, starting in grade 4, 5, or 6. The antismoking ads aired for five months the following year, followed by short ad surges for the next three years, providing about seven television and nine radio ad exposures per week. Also, students received an antismoking school program of four hours a year in grades 5–8, and then two hours a year in grades 9–10. The research question was: Did the ads help to deter smoking initiation?

The design was an experiment, in that ad exposure was manipulated. Two test communities received the ads and school program, while the two demographically matched control communities received just the school program (figure 12.1). Two pairs of metropolitan statistical areas were selected based on their having populations between fifty thousand and four hundred thousand, being similar on education, income, and ethnicity, and having independent media markets. Then, schools within these areas were selected that had low levels of parental education and income because these variables are associated with high smoking.

The study found that the ads were effective (Flynn et al., 1992). While smoking prevalence increased as the students aged as expected, the increase was less in the test group (ads and school program), relative to the control group (school program only), resulting in significantly lower smoking prevalence in the last two years. By the final year, 12.8 percent of the test group reported smoking in the past week versus 19.8 in the control group, a 35 percent difference. Results were similar for other measures of smoking behavior and beliefs. Two years later, smoking rates remained lower in the test versus control group (Flynn et al., 1994). The ads reduced smoking but not drinking as expected.

Strengths of this evaluation include the large samples, extensive ad exposure, and the fact that ad exposure was manipulated, which increases the likelihood that the ads at least partially caused the effects. A limitation of this evaluation, and some others like it, is that the intervention or ad manipulation was not entirely clean. In particular, some of the students who were exposed to the ad campaign participated in preliminary research to help design and pretest the ads (Worden et al., 1988), and it is unclear how much these experiences bolstered the ad effects. Also, all students received an extensive school program and so the effects of an ad-only intervention are unknown. We do not know the effects of the school-only intervention because there was no school-only group either. Another limitation is that demographic covariates were measured, but not included in the main models that estimated the ad effects. Finally, standard errors should have been adjusted by school to account for clustering (correlated errors) at the school level, since schools were the primary sampling unit not individual students (Farrelly et al., 2005; Murray and Hannan, 1990).

Case 2.2: University of Kentucky antimarijuana ad campaign field test

University of Kentucky researchers completed a large field experiment of an antimarijuana ad campaign with funding from the National Institute on Drug Abuse (Palmgreen et al., 2001). The study lasted thirty-two months, and involved monthly surveys with one hundred randomly selected adolescents in each of two counties (markets), amounting to about six thousand adolescents overall. The survey assessed marijuana use in the past thirty days, other substance use, and sensation seeking as a personality trait.

The design was an experiment as well, but ad exposure was manipulated differently. One county received the ad campaign for four months in each of two years. The second county received the campaign for four months in just the second year; in essence, it served as a control for the first county in the first year (figure 12.1). The two counties were in the same region and were demographically and culturally similar. At least 70 percent of the targeted adolescents were exposed to three or more ads per week. Surveys were conducted in each county during pre-, post-, and other non-campaign periods to provide control or baseline data; the same surveys were also conducted during the campaigns to provide intervention data.

The ads were shown to be effective in both counties (Palmgreen et al., 2001). When the ads were on the air and for about six months afterward, marijuana use declined significantly; otherwise, marijuana use increased. However, these effects were limited to high sensation seeking adolescents who overall showed a 26.7 percent decline in marijuana use from the ads. Among low sensation seekers, marijuana use was flat; there were no campaign effects and no developmental trends. The positive effects were specific to marijuana use and did not extend to other substances (e.g., alcohol, tobacco, or crack). Strengths of this evaluation include the interrupted time series design with replication, extensive manipulated ad exposure,

monthly measurement, and large samples. Yet, some experts view "two city" designs like this as risky, because there is a single treatment and a single control site, and it must be assumed that the sites are equivalent at the start (Hornik, 2002).

Designs 3 and 4: The Lab Quasi-experiment and Experiment for Copy Tests

Now, we will turn to the evaluations conducted in the lab, namely the lab quasi-experiment (design 3) and the lab experiment (design 4), which are typically done to conduct copy tests of campaign or program materials. By lab we mean a controlled exposure setting, such as a mall research facility or a middle school classroom. By lab quasi-experiment, we mean copy tests in a controlled exposure setting among audience subgroups, such as different ages, genders, or ethnicities. These are quasi-experiments because demographic subgroups cannot be manipulated; people were born into them. By lab experiment, we mean copy tests of materials that vary in terms of the message (e.g., theme) or situation (e.g., distraction level). These are true experiments because exposure to these materials or settings can be manipulated and often a control group is included. These are not simple focus groups in which everyone sees the ads and discusses them.

Lab evaluations cost a fraction of full-scale field evaluations (thousands of dollars versus millions), and can be done in a fraction of the time (months versus years). Researchers can conduct lab evaluations with little or no extramural funding, though some agencies will fund them (Pechmann et al., 2003). Also, lab evaluations can simulate interventions that are too controversial or costly to be rolled out, unless their efficacy is demonstrated. For example, a lab simulation was conducted on the effects of antismoking ads in movie theaters because movie theater owners were unwilling to allow the ads in the theater (Pechmann and Shih, 1999). Major movie studios eventually agreed to put antismoking ads on DVDs based on the study results.

The primary concern about lab evaluations is that it is even more uncertain that the results will generalize to a full-scale campaign. Thus, the optimal approach is to start with a lab evaluation and, if the results look promising, continue with a field evaluation. The Office of National Drug Control Policy's antimarijuana ads were copy tested in a lab setting (Foley and Pechmann, 2004) and then evaluated in the field (Hornik et al., 2008).

Case 3: Copy Test of Anti-Drug Ads with Different Demographic Subgroups

The Office of National Drug Control Policy (ONDCP) routinely copy tests its finished or near-finished anti-marijuana TV ads before airing, to ensure the ads are effective and not counterproductive among adolescents (Foley and Pechmann, 2004). An ad will air if it significantly strengthens an anti-drug belief or weakens drug use intent in the copy test, among the overall

sample or a demographic subsample such as a specific ethnic subgroup. An ad will not air if it significantly weakens an antidrug belief or strengthens drug use intent in the copy test, among the overall sample or a subsample.

This design is a lab quasi-experiment; another term for quasi-experiment is partially randomized experiment. The comparisons are between test (those exposed to the ad) and control (those not exposed to the ad) and each individual participant is randomly assigned to test or control. Nonetheless, this is a quasi-experiment or a partially randomized experiment because exposure effects are compared among ethnic subgroups or other demographic subgroups that preclude random assignment (e.g., participants cannot be randomly assigned to ethnic subgroup). Therefore, any differences in exposure effects that seem to be moderated by a demographic factor such as ethnicity cannot unequivocally be attributed to that factor. Ethnicity could be confounded with other variables; for instance, one ethnic group may have more knowledge of marijuana than another, affecting their ad comprehension and thus ad impact. If it is critical to know what is causing a differential impact on one demographic subgroup to take corrective action, researchers should try to manipulate a closely related variable (e.g., manipulate the salience of ethnicity) and do full random assignment to all conditions.

For the government's copy tests of antidrug ads, it is not critical to know what is driving ethnicity or other subgroup effects; therefore full random assignment is not needed and partial random assignment or a quasi-experimental design is adequate. However, random assignment to test versus control is needed to assess causal ad effects. Therefore, each adolescent is randomly assigned to be a test or control participant, and this is done for each of twelve demographic subgroups (figure 12.1). The twelve demographic subgroups represent three ethnicity levels (Caucasians, Hispanics, and African Americans), crossed with two grade levels (7–8 and 9–10) and also crossed with gender (male and female). For each copy test, the total sample size is six hundred, including three hundred test and three hundred control participants, with equal numbers of adolescents in each of the demographic subgroups (fifty per subgroup, including twenty-five test and twenty-five control).

Data collection is typically completed in one weekend using about forty shopping malls that have research facilities. Adolescents are approached in the malls, screened based on grade, ethnicity, and gender, offered one dollar to participate (resulting in a 75 percent participation rate), and given an informed assent form; parental consent is also obtained when possible. Next, adolescents are randomly assigned to be test participants, in which case they see the focal antimarijuana TV ad twice, or control participants in which case they do not see this ad. Afterward, all adolescents complete a ten-minute survey on their drug-related beliefs and intent, and the test participants also rate the ad on diagnostic measures. Covariates are also measured, including sensation seeking and exact age.

The effects of seeing the ad (test) versus not seeing it (control) are examined for the overall sample and for the subsamples. Covariates are included

in the analyses to help rule out rival explanations for any observed differences between test and control groups or demographic subgroups (e.g., sensation seeking, exact age). Some of the ads may have null effects, or even adverse effects among a subgroup or the total sample, but these ads are not aired until they are revised, retested, and shown to have only desirable effects. The diagnostic measures help to reveal an ad's specific weaknesses. For instance, it may be found that the youngest adolescents miscomprehended certain ad content.

The strengths of this type of evaluation include relatively low cost, speed, manipulation of ad exposure, random assignment to test or control to ensure the groups are comparable except on ad exposure, and the assessment of effects by demographic subgroup. The main concern about this evaluation is whether the results generalize to the real world. For instance, the lab setting ensures that the ads are attended to; but in the real world, there are many distractions and other influences (e.g., word-of-mouth, peers), and the ads may not be attended to and may fail.

After the Office of National Drug Control Policy's copy test system was fully operational, all of the ads that it aired had been shown to reduce marijuana beliefs and/or intent in a copy test. Nevertheless, the Hornik et al. (2008) full-scale field evaluation found that the campaign was ineffective. In part, this may be because the Hornik et al. evaluation commenced before the copy test system was fully operational. Still there are troubling discrepancies in the research findings that call for follow-up studies.

Case 4: Copy Test of Anti-Smoking Ads with Different Messages

Researchers at the University of California, Irvine, obtained state funding to copy test antismoking ads with different messages among adolescents (Pechmann et al., 2003; also Zhao and Pechmann, 2007). There was considerable controversy about which messages to use, because different states had adopted radically different approaches. Research was needed to identify evidence-based approaches, and it would have been costly and time consuming to test the common types in a large field study. Therefore, a lab experiment was conducted, and the seven most prevalent message types were copy tested including long-term health consequences, cosmetic effects, second-hand smoke risks, and tobacco industry manipulation.

The design was a pure lab experiment or a randomized controlled trial involving 1667 adolescents from public schools. The adolescents who assented to participate and had parental consent were released from class, and then each individual was randomly assigned to a different classroom. In each classroom, several ads representing a single message type were shown on a large-screen TV. [Zhao and Pechmann (2007) embedded the ads in a TV show.] Overall, eight different classrooms were set up, in order to randomly assign participants to view one of the seven message types or the control message type (nonsmoking-related public service announcements) (see table 12.1).

After seeing the ads, adolescents completed a short survey on their smoking-related beliefs and intent. It was not feasible to measure the direct impact of the ads on smoking behavior by offering the adolescents cigarettes, for obvious ethical reasons, and one short ad exposure was not expected to affect long-term behavior. Instead, a validated measure of behavioral intent was used to assess how a well-funded ad campaign featuring each message type might impact long-term behavior (Pierce et al., 1995). The analyses compared the smoking beliefs and intent of the adolescents who saw the control ads, versus the adolescents who saw each type of test ad.

The study (Pechmann et al., 2003) found that the messages about the social aspects of smoking (e.g., second-hand smoke) significantly lowered adolescents' smoking intent, relative to the control condition, while the other messages had weaker and nonsignificant effects. The strengths of this evaluation (low cost, control over ad exposure, random assignment) as well as the weaknesses (artificiality) are similar to those discussed earlier.

Methodological Challenges

Choosing a research design to evaluate a social impact intervention almost invariably involves difficult tradeoffs. After-only designs that lack true control groups suffer from possible reverse causality (e.g., higher prior intentions to use drugs leading to greater awareness of antidrug ads). Simple pre-post designs can be problematic with respect to alternative explanations and confounds (e.g., general trends), as well as insufficient time between observations and inadequate resources for the treatment (Pechmann and Reibling, 2000). Cohort or longitudinal designs may be subject to history and maturation effects of participants. If a no-exposure control group is used, one should try to ensure that treatment and control groups (e.g., cities) are comparable a priori and that the treatment does not spill over to the control group. All of these designs can be challenged, yet it should be recognized that no social impact study is flawless.

In some cases, researchers may not be able to utilize a control group in field research. For example, it might not be politically feasible to hold out a portion of the country in a national antidrug campaign; or in an evaluation of a state antitobacco campaign, a matched control state might refuse to participate in the survey measurement. In such instances, insights may still be derived from a study of key relationships affecting intent or behavior, and of possible moderating and mediating effects. In Andrews et al.'s evaluation (2004) of a statewide, adolescent antismoking campaign, ad campaign attitudes, prior trial behavior and social influence (i.e., family and friends who smoked) were all found to significantly affect antismoking beliefs. Ad campaign attitudes also interacted with prior trial behavior to strengthen antismoking beliefs. In addition, the effect of ad campaign attitudes in lessening social influence and prior trial effects on smoking intent persisted even when accounting for antismoking beliefs.

The temptation may arise to rely on focus groups for evaluations, but these should only be used for idea generation and not as a final evaluation. Pressure may also be exerted to test non-comparable stimuli (e.g., ads in near final form rather than final form), use nonstandard or practitioner measures that look good but have no known reliability or validity (e.g., single item scales), ignore power analyses, focus on selective, data-driven (versus hypothesized) results, and the like. However, it is important to try to adhere to generally accepted research principles and not succumb to such pressures.

Dealing with Stakeholders

Researchers who are evaluating social impact studies often face conflicting recommendations and mandates of a wide variety of stakeholders, including government agencies, funding agencies, ad agencies, marketing research firms, public relations firms, legislators, government oversight groups, nonprofit groups, consumer advocacy groups, diverse consumer groups, outside evaluators, and academic and practitioner consultants. A common problem with having too many stakeholders involved in an evaluation is the pressure to want to test everything, whether or not the issues are related to the key policy questions and/or decisions. Helping the stakeholders to differentiate between what is "important" versus "interesting" can lead to a more focused study.

A second stakeholder problem is related to the often controversial nature of social impact research. Well-intentioned advocates may rush to implement the intervention before pre-exposure control or baseline data can be collected. Also, objectively worded and standard measures must be used, rather than biased measures that may lead participants to answer the questions in the desired ways. Finally, research partners have been known at times to make ill-advised suggestions not to pretest treatment stimuli to save on costs because "they know how consumers will react" (i.e., the "sample of one" problem) or because the stimuli have been tested elsewhere (e.g., in a different state than the current one).

Outstanding Practices of Note

We would like to make special note of two design procedures used to copy test ads for social impact research. First, the National Youth Anti-Drug Media Campaign's testing of ads is a thorough procedure that links focus group data with copy test data and finally with the tracking of campaign effects in the field. These data collection efforts are synergistic because the same measures are used and the results are directly compared for additional learning.

The Federal Trade Commission's procedures for testing deceptive advertising are somewhat different, but they are based on generally accepted

principles too. Federal Trade Commission researchers normally include test and control ad groups, as well as control questions, and they funnel respondents from open-ended questions to successively narrower questions and ending with closed-ended ones (Andrews and Maronick, 1995). For greater detail on these copy testing methods, readers are encouraged to review the National Youth Anti-Drug Media Campaign and Federal Trade Commission procedures as discussed in Pechmann and Andrews (forthcoming).

Final Recommendations and Conclusions

We now offer some final thoughts and recommendations. First, we feel that it is important that social impact researchers get in on the "ground floor" of the evaluation planning and design process. It is vitally important to engage in planning and design with the right people who understand program evaluation techniques, the importance of a guiding consumer behavior theory, design options, and reliable and valid measures. As such, people are needed that adequately represent the three-legged stool of theory, research, and social issue (Brinberg and McGrath, 1985). Moreover, the use of multiple methods and triangulation certainly aids in offering greater credence to campaign outcomes. As noted by Shimp (1994), there are many different routes to knowledge.

Perhaps the most important consideration in conducting and evaluating social impact research is the selection of a control group to help account for alternative explanations. However, as noted earlier, there are some situations in which a single control group may not be feasible and creative solutions might be considered (e.g., comparing areas with different ad exposure levels; see also Hornik, 2002). In addition, given the many stakeholders involved in most social impact research decisions, we feel that it is critical to truly understand the many different disciplines that are involved. For example, some may focus on maximizing the amount of information provided in the campaign, rather than the effects on consumers who may have limited processing capabilities (Mazis, 1980). Finally, although there is no such thing as a perfect social impact study, researchers should try not to compromise on generally accepted, research principles.

Note

1. A GRP (gross rating point) is a measure of exposure opportunity, and it is calculated from the media buy reach x frequency. For example, an annual GRP of two thousand might mean that 100 percent of the target consumers received 20 weekly exposure opportunities or about .5 a week; while an annual GRP of ten thousand might mean that 100 percent of the target consumers received 100 weekly exposure opportunities or about 2 a week.

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