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Laboratory Experiments in Consumer Research: Estimating the Effect of a Manipulation-check Variable

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ABSTRACT

In consumer research and psychological experiments, subjects' states (attitudes) are manipulated by means of stimulus treatment in order to examine the effects of the subjects' states (attitudes) on the target variable. The interest here is not the effect of the treatment (stimulus) itself, but the effect on the target variable of the difference in state produced as a result of the treatment. Therefore, a manipulation check is usually performed to establish the validity of the experimental design, i.e., whether the stimulus produced the intended difference in state.

When the manipulation-check variable (state) is directly associated with the target variable, one encounters the problem of confounding that affects both variables. To eliminate this problem, randomized controlled trials (RCTs) are used, but two weaknesses exist: first, only a discrete, binary effect of the presence or absence of treatment on the target variable can be uncovered. Second, the imperfection of the experimental design, in which the state induced by the treatment (stimulus) varies from subject to subject, resulting in different effects on the target variable, cannot be taken into account.

In this study, we propose an approach that can correctly estimate the effect, which relates the manipulation-check variable to the target variable, even when unobserved confounding factors are present. By accounting for imperfections in the experimental design, the effect of the state variable becomes statistically more efficient than the effect of the experimental approach. The simulation analysis confirms that, for the same sample size, our instrumental variable approach is more significant than the usual experimental approach.

Keywords: experiment, marketing, manipulation check, treatment, state, instrumental variable

1. Introduction

One of the greatest advantages of conducting experiments in empirical marketing research is the ability to control various factors of no interest. This is because in a randomized controlled trial (RCT), subjects are randomly assigned to an treatment group and a control group, allowing for balancing the effects of both observed factors (covariates) and unobserved factors between the two groups.

There are two types of experiments: field experiments conducted at actual sites (markets) and laboratory experiments conducted in artificial environments. A well-known example of a field experiment is IRI's research service called BehaviorScan®, which was started in the 1970s and used RCTs to verify the effects of TV commercials on consumer purchases. Field experiments are also widely used to verify the effects of sales promotion activities such as discounts, coupons, and sampling. Recent developments in information technology have made it possible to conduct RCTs, called A/B tests, at a low cost on the Internet. Laboratory experiments, such as pre-market tests like ASSESOR®, are also well known. In these marketing experiments, we are interested in the impact of the treatment itself (with or without advertising) on the target variable (sales).

Likewise, laboratory experiments are a popular approach in consumer research and psychology. In many cases, the subject's state is manipulated by means of a stimulus treatment in order to test the effect of the subject's state (attitude) on the target variable. The interest here is not the effect of the treatment (stimulus) itself, but the effect on the target variable of the difference in state produced as a result of the treatment. Therefore, a manipulation check (mostly self-reported responses) is essential to establish the validity of the experimental design, i.e., whether the treatment produced the intended difference in state.

In this study, we first consider a naive approach in which the manipulation-check variable (state) is directly related to the target variable. To avoid the effects of confounding factors, which is a problem of the naive approach, we then introduce an experimental approach. Next, we propose an instrumental variable approach that can correctly estimate the effect, which relates the manipulation-check variable directly to the target variable, if the treatment variable is incorporated as an instrumental variable, even under the influence of unobserved confounding factors.

There are two advantages of the instrumental variable approach. First, it allows estimating the effect of the manipulation-check variable itself, which is continuous, on the target variable. This is particularly useful in consumer research, where the manipulation-check variable (attitude, state), rather than the discrete (0/1) treatment variable, is of research interest. Second, it allows for imperfection of the treatment on the manipulation-check variable. As a result, the effect of the state variable will be more statistically efficient than the effect of the experimental approach.

This paper is organized as follows. Section 2 introduces the naive and experimental approaches to the analysis of laboratory experiments used in existing studies and points out their weaknesses. To overcome the weaknesses, Section 3 describes the instrumental variable approach, which can properly estimate the effect of a manipulation-check variable on the target variable. In Section 4, we show through simulation analysis that the estimation by the instrumental variables approach is superior (unbiased and efficient) to that of the naive and experimental approaches, even in the presence of confounding factors. Section 5 concludes the paper by presenting summary and extensions of the instrumental variable approach.

2. Existing Approaches in the Analysis of Laboratory Experiments

In explaining the naive and experimental approaches, consider the following marketing problem. Corporate SDG activities require significant resources, but do they really increase the profit of the company? To address this managerial issue, the following research question (RQ) is considered.

[RQ] To what extent does a company's SDG image affect the purchase intention of the company?

The simplest approach to this RQ would be to present examples of SDG activities of various companies in a questionnaire and ask consumers to self-report their attitudes and purchase intentions. For example, for each activity, we would ask consumers to answer “SDG image of that company” (x) on a 5-point scale and “purchase intention toward that company” (y) on a 7-point scale, and estimate a model $y = a + \mathrm{b}x + e$ regressing y on x. If b is significantly positive, we conclude that there is an effect.

However, this approach is problematic because the estimate is affected by factors that are correlated with both the explanatory and target variables.

The SDG activities of a large company with more resources and better public relations skills may appear to be more impressive than those of a small company, and at the same time, consumer purchase intentions may favor the large company over the small company. In other words, the confounding factor of firm size is positively correlated with both the explanatory and target variables. In that case, a regression model ignoring the confounding factor would overestimate the true value of b.

Conversely, consumers who anticipate the strength of public relations of large firms may discount their SDG activities more than those of small firms. In other words, the confounding factor of firm size is negatively correlated with the explanatory variable and positively correlated with the target variable. In this case, a regression model that ignores the confounding factor will underestimate the true value of b.

One might think, then, that it would be better to estimate a regression model with the confounding factor added as a covariate. However, if the confounding data is not observable, or if we do not even know what kind of confounders exist, we cannot incorporate them into a model as covariates.

This is where RCTs come in. We randomly present subjects with a SDG activity in the treatment group (T=1) and a non-SDG activity of the same firm in the control group (T=0), and ask them to self-report their SDG image and purchase intention toward the firm. Because randomization results in confounding factors having equal effects across the two groups, the average treatment effect (ATE) of SDG activities (vs. non-SDG activities) on purchase intention can be estimated by comparing the values of the target variable across the two groups. That is, ATE = $E[y|T=1] - E[y|T=0]$. Here, to establish whether the experimental design is valid, we perform a manipulation check to see whether the treatment changed the value of the explanatory variable (x = SDG image). That is, we need to check that b>0 in a regression model, $x = \mathrm{b}T + e$.

3. Instrumental Variable Approach

The experimental approach eliminates the confounding factor problem, but only yields a binary-level difference in the degree to which the presence or absence of SDG activity (T = 1/0) is reflected in differences in purchase intention (y). However, even with the same stimulus (SDG activity), the degree of psychological state brought about (SDG image) would differ among subjects. Usually, the interest in consumer research is not the effect of the treatment (stimulus) itself, but the impact on the target variable of the difference in the state produced as a result of the treatment. Therefore, the "degree of influence" (continuous) from the SDG image (x) to y is more useful than the "influence" (discrete) from the treatment (T) to y. In other words, we want to relate y

with x but not T, and at the same time, we would like to avoid the influence of confounding factors that would be problematic in a naive approach.

Therefore, in this study, we propose an instrumental variable approach that allows us to correctly estimate E[y|x] by associating the manipulation-check variable (x) with the target variable (y), even in the presence of unobserved confounding factors. In other words, we take advantage of the property that RCT randomly assign different values of the manipulation-check variable to confounding factors (Angrist and Pischke 2009).

Figure 1 summarizes the relationships among the variables that have emerged so far. x is the manipulation-check variable (SDG image), y is the target variable (purchase intention), u is the confounding factor (firm size), and T is the treatment variable (=1 for SDG activity, =0 for non-SDG activity).

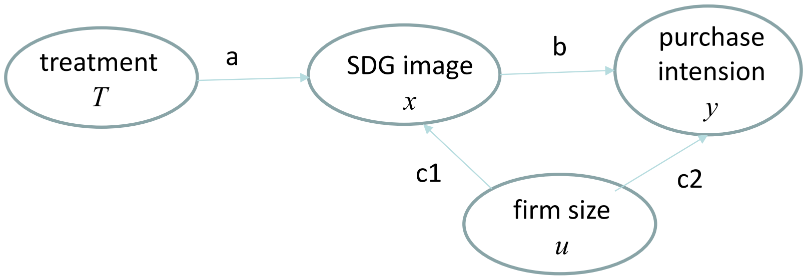

Figure 1: Relationship among relevant variables

3.1.1 Instrumental Variables

A variable z that satisfies the following two assumptions is called an instrumental variable (Wooldridge 2020).

(1) $\mathrm{cov}[z, u] = 0$

(2) $\mathrm{cov}[z, x] \neq 0$

Using this property, the instrumental variable method says that the effect b of x on y can be expressed by equation (3), even in the presence of unobserved confounding factors (Appendix 1).

(3) $\mathrm{b(IV)} = \mathrm{cov}[z, y] / \mathrm{cov}[z, x]$

Whether the treatment variable T (1/0) satisfies the instrumental variable assumption is examined in detail in section 3.2. If it does, z = T and equation (3) can be derived as in equation (4) (Appendix 2).

（4）　$b(IV) = \{ E[y|T=1] - E[y|T=0] \} / \{ E[x|T=1] - E[x|T=0] \}$

3.2 Assumptions of the Instrumental Variable Approach

In order to properly estimate the effect of x using the instrumental variable method, z must also satisfy assumptions 3 and 4, in addition to assumption 1 (equation (1)) and assumption 2 (equation (2)) in the previous section. (Wooldridge 2020).

[Assumption 1] exogeneity: presence of treatment is uncorrelated with confounding factors (firm size).

$$\mathrm{cov}[z, u] = 0$$

This assumption is satisfied because the presence or absence of treatment is random in RCTs.

[Assumption 2] relevance: the presence or absence of treatment is associated with the difference in state (image)

$$\mathrm{cov}[z, x] \neq 0$$

The association of the treatment can be confirmed by the F-test of the regression model (treatment ⇒ image) (if F>10 or more, there is an association (Staiger and Stock 1997)).

[Assumption 3] exclusion restriction: treatment affects the target (purchase intention) only through the state (image). In other words, once the value of x is determined, the impact on y is the same regardless of z. This means that z is excluded from the expression for y (y does not include z).

To see if assumption 3 is satisfied, we can check that in a mediation analysis with y as the target variable and z as the causal variable, the direct effect becomes non-significant when the indirect effect through the mediating variable x is included. In this mediation analysis, however, the effects of confounding factors must be controlled for.

[Assumption 4] monotonicity: The image increases with the presence of treatment.

$$\{E(x|T=1) - E(x|T=0)\} > 0$$

We can check if the regression (treatment => image) coefficient for the t-test is positively significant (assumption 2). This assumption is usually satisfied because the experimental manipulates x in the intended direction by the treatment (T) unless the design is flawed.

3.3 Advantages of the Instrumental Variable Approach

There are two advantages of the instrumental variable approach. First, it is possible to estimate the effect of the manipulation-check variable itself, which is continuous, on the target variable. This is particularly useful in consumer research and psychology, where the manipulation-check variable (attitude, state), rather than the treatment variable, is of research interest.

Second, it allows for imperfection of the treatment effect on the manipulation-check variable. The same treatment may significantly increase the SDG image of the company in some subjects and only slightly in others. As a result, subjects in the same treatment group will exhibit different effects on the target variable. By accounting for these differences, the effect of the state variable will be more statistically efficient than the effect of the experimental approach. Thus, for the same sample size, the instrumental variable approach tends to yield more statistically significant results than the usual experimental approach.

4. Simulation Study

In this section, we compare the results of the three approaches (naive, experimental, and instrumental variable methods) to estimate the impact of SDG image on purchase intention. As represented in Figure 1, x is the manipulation-check variable (SDG image), y is the target variable (purchase intention), u is the confounding factor (firm size), and T is the treatment variable (= 1 for SDG activity, = 0 for non-SDG activity) that corresponds to the instrumental variable.

In the simulation analysis, we set a=b=1 and consider four cases, representing different confounding situations with different values of c1 and c2. An error term following an independent standard normal distribution (mean 0, variance 1) was added to x and y, respectively (not shown in Figure 1 for simplicity), generating 1000 data points. Let us summarize the three approaches to estimation.

In the naive approach, the effect is estimated by regressing the state (image) variable x on the target variable y, which we call AT (As Treated).

$$y = a + \mathrm{b(AT)}\, x + e, \quad \text{estimate of b(AT)}$$

The experimental approach is called ITT (Intention To Treat) because it is the difference between the means of the target variable y in the treatment and control groups and is an estimator based on the treatment intention.

$$b(\mathrm{ITT}) = \mathrm{E}[y|T{=}1] - \mathrm{E}[y|T{=}0]$$

The estimator for the instrumental variable approach is expressed in equation (4) of Section 3.1.

（4） $b(\mathrm{IV}) = \{\mathrm{E}[y|T{=}1] - \mathrm{E}[y|T{=}0]\} \,/\, \{\mathrm{E}[x|T{=}1] - \mathrm{E}[x|T{=}0]\} = b(\mathrm{ITT}) \,/\, \{\mathrm{E}[x|T{=}1] - \mathrm{E}[x|T{=}0]\}$

Let us now consider whether the treatment variable (T) satisfies the four assumptions of the instrumental variable approach.

[Assumption 1] exogeneity: $\mathrm{cov}[T, u] = 0$
The presence or absence of a treatment is randomly assigned and is thus uncorrelated with confounding factors. So the assumption is satisfied.

[Assumption 2] relevance: $\mathrm{cov}[T, x] \neq 0$
Since the purpose of the experiment is to manipulate subjects' SDG images (x) with and without treatment, T and x is expected to be correlated. Here we will confirm by statistically verifying that the F-test (or the square of the t-value of the slope coefficient) for the regression model (treatment ⇒ image) is significant.

[Assumption 3] exclusion restriction: the treatment affects the target only through the state.
In the simulation data generated according to Figure 1, this is satisfied because T is excluded from the expression for y (T is not included in the expression for y).

[Assumption 4] monotonicity: $\{\mathrm{E}[x|T{=}1] - \mathrm{E}[x|T{=}0]\} > 0$
Since the purpose of the experiment is to manipulate subjects so as to increase their SDG image with treatment, this assumption is expected to be satisfied. Here we will confirm that the t-test of the regression (treatment ⇒ image) coefficient is significantly positive.

Table 1 shows the estimation results and their standard errors by the naive approach b(AT), the experimental approach b(ITT), and the instrumental variable approach b(IV). Note that the true value of b is 1.

Table 1：Estimation Result of Unstandardized b

The effect of confounding adds bias to b(AT), but there is no bias in b(ITT) and b(IV). Also, standard errors are smaller for b(IV), which accounts for the imperfect experimental design, than for b(ITT). (standard errors in parentheses, Gelman and Hill 2007)

confounding	a	b	c1	c2	b(AT)	b(ITT)	b(IV)	F-value	t-value	b(AT) biased?
No	1	1	0	0	1.01 (0.03)	1.05 (0.09)	1.02 (0.06)	249.7	15.8	unbiased
No	1	1	0	1	1.06 (0.04)	1.09 (0.12)	1.05 (0.09)	249.7	15.8	unbiased
Positive	1	1	1	1	1.47 (0.03)	1.12 (0.16)	1.05 (0.09)	127.2	11.3	over-estimation
Negative	1	1	1	-1	0.57 (0.03)	1.05 (0.09)	0.98 (0.09)	127.2	11.3	under-estimation

When there is no effect of the confounding factor (c1=0), b(AT) has no bias and its standard error is smaller than that of b(IV) and b(ITT). However, when the confounding factor is positively correlated to the state and target variables (c1=1, c2=1), b(AT) overestimates the true value, and when the confounding factor is negatively correlated (c1=1, c2=-1), b(AT) underestimates the true value. In contrast, b(IV) and b(ITT) are estimated without bias. Standard errors tend to be smaller for b(IV), which takes into account imperfections in the experimental design, than for b(ITT).

Next, let us verify whether the four assumptions of the instrumental variables are satisfied.

Assumption 1 (exogeneity) is satisfied because the treatment is uncorrelated with confounders in a randomized controlled trial.

Assumption 2 (relevance) is satisfied because the F-value for the regression model from treatment (T) to image (x) in Table 1 is significant (and >10).

Assumption 4 (monotonicity) is met because the t-value of the regression coefficient from treatment (T) to image (x) in Table 1 is significantly positive.

Assumption 3 (exclusion restriction) is satisfied because, in the two mediation analyses (Figure 2) where confounding do not exist (c1 = 0), the significant direct effect from treatment (T) to target (y) becomes insignificant when mediated by image (state x).

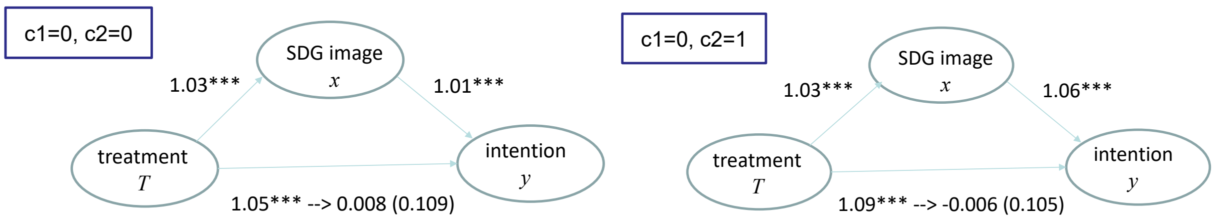

Figure 2: Results of the Mediation Analysis

The significant direct effect from intervention to purchase intention become insignificant when mediated by image. (Standard errors in parentheses)

Table 2 shows the standardized estimates. As with the unstandardized estimates, when there is no confounding (c1=0), b(AT) has no bias. When a confounding factor is present, b(AT) is subject to bias in the direction of over- or underestimation, so the estimate is not informative. On the other hand, for b(ITT) and b(IV), where the problem of confounding does not occur, we find a larger effect for b(IV) than for b(ITT), which accounts for imperfect experimental design.

Table 2： Standardized Estimation Result of b

The effect of confounding adds bias to b(AT), but there is no bias in b(ITT) and b(IV). The effect size is larger for b(IV) than for b(ITT), which accounts for the imperfection of the experimental design. (standard errors in parentheses, Gelman and Hill 2007)

confounding?	a	b	c1	c2	b(AT)	b(ITT)	b(IV)	b(AT) biased?
No	1	1	0	0	0.75 (0.02)	0.34 (0.03)	0.75 (0.05)	unbiased
No	1	1	0	1	0.64 (0.02)	0.28 (0.03)	0.63 (0.06)	unbiased
Positive	1	1	1	1	0.88 (0.02)	0.21 (0.03)	0.63 (0.05)	over-estimation
Negative	1	1	1	-1	0.58 (0.03)	0.34 (0.03)	1.00 (0.09)	under-estimation

5. Conclusions

In laboratory experiments in consumer research and psychology, subjects' state (attitude) is usually manipulated through stimulus intervention in order to test the effect of the subjects' state (attitude) on the target variable. A manipulation check is then conducted to establish the validity of the experimental design, i.e., whether the stimulus produced the intended difference in state.

Usually, the interest of the study is not the effect of the treatment (stimulus) itself, but rather the effect of the difference in state produced as a result of the treatment on the target variable. When the manipulation-check variable (state) is directly associated with the target variable, one encounters the problem of confounding that affect both variables. To eliminate this problem, randomized controlled trials (RCTs) are used, but two weaknesses exist: first, only a discrete, binary effect of the presence or absence of a treatment on the target variable can be uncovered. Second, the imperfection of the experimental design, in which the state induced by the treatment (stimulus) varies from subject to subject, resulting in different effects on the target variable, cannot be taken into account.

In this study, we proposed an instrumental approach that can correctly estimate E[y|x], even when unobserved confounding factors are present, if the treatment variable is incorporated as an instrumental variable. This approach exploits the idea of the instrumental variable method that the RCT intervention randomly assigns different values of the manipulation-check variable to confounding factors. In the simulation analysis, we compared the estimation of causal effects in the naive, experimental, and instrumental variable approaches in the presence and absence of a confounding factor.

In operation, there are two advantages of the instrumental variable approach. First, it allows estimating the effect of the manipulation-check variable itself, which is continuous, on the target variable. Second, it allows for imperfection of the treatment on the manipulation-check variable. As a result, the effect of the state variable will be more statistically efficient than the effect of the experimental approach.
Thus, for the same sample size, the instrumental variable approach produces more significant result than the usual experimental approach.

One limitation of this approach is that, in the presence of unknown confounding factors, one must rely on theory to determine whether Assumption 3 is satisfied, because a proper mediation analysis cannot be performed. A situation in which assumption 3 is not satisfied is when a treatment changes a state other than that monitored by the manipulation-check variable and that state affects the target variable. Therefore, it is important to adequately capture the state that is being changed by the treatment, such as by making the manipulation-check variable a multiple-item sum.

In this paper, we use the simplest one-factor (instrumental variable) two-level (with or without treatment) ANOVA framework to estimate the effect of a manipulation-check variable in a laboratory experiment. There are several possible directions for extending this method. The first is to extend it to a two-factor analysis of variance. This extension would be very useful since interaction analysis is often the core of research in consumer research and psychology. Second, we can extend it to an analysis of covariance framework by adding covariates, which are exogenous variables. Third, it can be applied to models that include moderating variables.

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APPENDIX 1

Let $x2$ be the unobserved confounding factor and define y as in equation (1).

(1) $y = b0 + b1 \cdot x1 + b2 \cdot x2 + e = b0 + b1 \cdot x1 + u$ where $u = b2 \cdot x2 + e$

Thus, the effect of $x2$ is included in the error term u.
From the definition of confounding factors, $\mathrm{cov}[y, u] \neq 0$ and $\mathrm{cov}[x1, x2] \neq 0$

From endogeneity (explanatory variable $x1$ and error term u are correlated), there exists bias ($du/dx1$) in the effect estimation of $x1$ by OLS.

$$dy/dx1 = b1 + du/dx1$$

Now consider an instrumental variable z that satisfies the following assumptions.

$$\mathrm{cov}[z, u] = 0$$

cov[z, $x1$] ≠ 0

Then,

cov[z, y] = cov[z, b0 + b1 ･$x1$ + u] = b1･cov[z, $x1$] + cov[z, u] = b1･cov[z, $x1$]

This leads to

b1 = cov[z, y] / cov[z, $x1$]

Multiplying the denominator and numerator by cov[z, z],

b1 = (cov[z, y]/cov[z, z]) / (cov[z, $x1$] / cov[z, z]) = b / a

b1 can be interpreted as the ratio of the coefficient b (y regressed on z) to the coefficient a ($x1$ regressed on z).

Replacing the instrumental variable z with the treatment variable T, estimator b1 is expressed as follows (Appendix 2).

b1 = { E[y|T=1] – E[y|T=0] } / { E[$x1$|T=1] – E[$x1$|T=0] }

2SLS can also be used for estimation (Wooldridge 2020).

APPENDIX 2

First,

$$\begin{aligned}
\mathrm{cov}[z,y] & \\
&= E[zy] - E[z]E[y] \\
&= p_1 E[zy|z=1] + p_0 E[zy|z=0] - p_1\{\, p_0 E[y|z=0] + p_1 E[y|z=1]\,\} \\
&= p_1\{E[y|z=1] - p_1 E[y|z=1]\} - p_1 p_0 E[y|z=0] \\
&= p_1 p_0 E[y|z=1] - p_1 p_0 E[y|z=0] \\
&= p_1 p_0\{\, E[y|z=1] - E[y|z=0]\,\}
\end{aligned}$$

Similarly,

$$\mathrm{cov}[z,x] = p_1 p_0\{\, E[x|z=1] - E[x|z=0]\,\}$$

Therefore,

$$\mathrm{b(IV)} = \frac{\mathrm{cov}[z,y]}{\mathrm{cov}[z,x]} = \frac{E[y|z=1] - E[y|z=0]}{E[x|z=1] - E[x|z=0]}$$