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Measures of Independent Variables and Mediators Are Useful in Social Psychology Experiments: But Are They Necessary?

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A study of experiments in major social psychology journals shows that measures of independent variables have become increasingly common. The role in experiments of measures of independent variables and proposed mediating variables is examined. In the causal sequence assumed in interpreting an experimental result, the independent variable and proposed mediating variable are presumed to mediate the effect of the experimental treatment on the dependent measure. Measures of independent variables and mediators provide checks on the assumptions that the experimental treatment successfully manipulated those variables and are unquestionably useful. A separate, controversial issue is whether such measures are necessary in experiments. If no plausible alternative explanations exist, data from such measures are not needed. Plausible alternative explanations are not eliminated by data from such measures. Alternative explanations, critical for assessing construct validity (Cook & Campbell, 1979), are distinguished from different general theoretical accounts of a finding.

Measures of conceptual independent variables are often included in experiments in social psychology, and sometimes measures of proposed mediating variables are included as well. The inclusion of such measures in experiments appears to be increasing, suggesting that such measures are commonly thought to be essential in order to place confidence in conclusions from experiments. With the increase in the use of such measures, social psychologists should be very clear about how the data from measures of independent variables and mediating variables bear on the conclusions that can be drawn from experimental results.

In order to document whether the inclusion of such measures in experiments has become increasingly common, we conducted a study of experiments published in the *Journal of Personality and Social Psychology* (*JPSP*) and the *Journal of Experimental Social Psychology* (*JESP*) in 1965, 1975, 1985, and 1995. Both journals began publication in 1965 and since their inception have been major outlets for experimental research in social psychology. All articles published in *JPSP* and *JESP* in 1965, 1975, 1985, and 1995 were examined and classified into those that reported experiments (i.e., studies that involved the manipulation of some variable) and those that did not.[1] Those that reported experiments were further classified into those with between-subject manipulations and those with only within-subject manipulations, and the study was restricted to those with between-subject manipulations.[2] Those with between-subject manipulations were classified into those that included a measure of the independent variable and those that did not. Because the frequency of clear-cut instances of measures of mediators was too low to allow a meaningful assessment of trends, the study was confined to measures of independent variables.

Of the articles that appeared in the 1965 issues of *JPSP* and *JESP* that reported experiments with between-subject manipulations, 27% included a manipulation check. In 1975 that percentage was 33%, and in

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[1]The article was selected as the unit of analysis, rather than the experiment, in order to provide a fairer appraisal of the use of measures of independent variables. When a series of experiments is reported in which the same experimental manipulation is used in each experiment, the experimenter may include a measure of the independent variable to check on the manipulation in only one experiment on the reasonable assumption that the same manipulation will have the same effect on the independent variable in each of the experiments. On the basis of our examination of this literature, we are confident that the trends revealed using the article as the unit of analysis would also be found if the experiment had been used as the unit of analysis.

[2]Experiments using only within-subject manipulations include studies in which the only variation is the order in which measures are given, and manipulation checks are not relevant in such cases.

1985 it was 42%. By 1995 that percentage had risen to 63%. For *JPSP,* the percentage of experimental articles with between-subject manipulations that included a manipulation check was 25% in 1965 (n = 112), 35% in 1975 (n = 198), 39% in 1985 (n = 118), and 62% in 1995 (n = 76). For *JESP,* the percentage was 38% in 1965 (n = 24), 22% in 1975 (n = 41), 56% in 1985 (n = 25), and 68% in 1995 (n = 22). The results of the study of experimental articles in *JPSP* and *JESP* confirm that over the years the inclusion of measures of independent variables has become increasingly common in social psychology experiments.

There are very good reasons for collecting measures of independent variables and mediating variables in social psychology experiments, and we have no wish to question the usefulness or to discourage in any way the inclusion of such measures in experiments. But despite the usefulness of measures of independent variables and mediators, occasions occur when experiments are conducted that do not include such measures. Good measures of independent variables or mediators may not be feasible for a variety of reasons. Such measures may draw attention to the purpose of the study, thereby creating suspicion that would invalidate the test of the experimental hypothesis. They may contaminate responses to the dependent measure, unless taken after the dependent measure by which time they may be less appropriate (e.g., checks on mood manipulations). Sometimes the inclusion of a measure of the independent variable can actually undermine the success of the manipulation of the independent variable by raising doubt about the veracity of the instructions designed to manipulate the independent variable ("Why, if what I was told is true, do they need my opinion?").[3]

The question we wish to address is whether the inclusion of measures of independent variables and mediators is necessary in experiments; that is, are experiments that lack such measures necessarily flawed? Our informal observation that considerable disagreement currently exists among those in the field about the necessity of using such measures in experiments led us to seek more information about the views of social psychologists on this matter. We attempted to survey the views of the members of the Society of Experimental Social Psychology (SESP) and others who attended the September 1995 SESP meeting in Washington, DC, concerning the necessity of measures of independent variables and mediators. In order to get additional responses, we e-mailed the same survey to the members of SESP. We received responses from 70 social psychologists. The way the sample of social psychologists was obtained does not permit an estimation of the exact number of social psychologists with different views on the issue. However, the divergence in the responses to the survey makes clear that considerable disagreement exists among social psychologists about the necessity of measures of independent variables and mediators in social psychology experiments.

One survey item asked if a manipulation check was "*necessary* in a well-designed social psychology lab experiment; that is, absence of the measure would constitute a flaw." Of those responding, 60% answered *yes,* and 40% answered *no.* Another item asked the same question about a *mediator check,* defined as a "measure of a hypothesized mediator variable," and to that item, 41% answered *yes* and 59% answered *no.* The patterns of responses to the survey were not influenced by whether it was distributed at the meeting or by e-mail. There is no reason to suspect that the way the responses were obtained produced a bias either for or against responses indicating acceptance by social psychologists of the view that experiments lacking manipulation checks or mediator checks are flawed.

The divergence of views concerning the necessity of including measures of independent variables and mediating variables in experiments has important consequences for experimental social psychology. Experiments that do not include such measures will sometimes be considered flawed even when they are otherwise well designed.

In the survey of social psychologists, those who indicated a manipulation check was absent in a laboratory experiment they reviewed for a social psychology journal in the past 5 years were asked if they judged the absence of a manipulation check to be a flaw, and 67% answered *yes.* Those who indicated a mediator check was absent in a laboratory experiment they reviewed for a social psychology journal in the past 5 years were asked if they judged the absence of a mediator check to be a flaw, and 68% answered *yes.* In addition, those who indicated that they had submitted a laboratory experiment to a social psychology journal in the past 5 years in which a manipulation check was absent were asked if a reviewer had judged the absence of a manipulation check to be a flaw, and 46% answered *yes.* Those who indicated that they had submitted a laboratory experi-

[3] An example of the possibility that a measure of the independent variable could undermine the effectiveness of a manipulation occurred in the experiment by Sigall and Michela (1976). That experiment attempted to manipulate feelings about one's own attractiveness by inducing participants to compare their attractiveness to very attractive others (designed to make them feel unattractive) or to very unattractive others (designed to make them feel attractive). Sigall and Michela argued that a measure asking participants to rate their own attractiveness could have diluted the effect of the context provided by the experimental conditions by leading participants to think about their attractiveness in relation to the standards they typically employ when judging their attractiveness. The authors obtained evidence that the manipulation of the participant's attractiveness was successful in that the predicted effects of the experimental conditions on the dependent measure were found and there was no way to account for those effects without assuming the manipulation was successful. In fact, a number of reviewers of that article failed to grasp the evidence that the manipulation was successful and deemed the absence of a manipulation check to be a flaw.

ment to a social psychology journal in the past 5 years in which a mediator check was absent were asked if a reviewer had judged the absence of a mediator check to be a flaw, and 69% answered *yes*.

Experimenters may be constrained to include measures of independent variables or mediators in experiments even if they have reason to believe the inclusion of such measures may compromise the procedure of the experiment. We believe that social psychologists need to examine carefully the issue of whether measures of conceptual independent variables and mediators are necessary in experiments.

In a social psychology experiment, what is of interest is evidence of a causal relation between a conceptual independent variable and a conceptual dependent variable.[4] Sometimes interest also exists in the operation of a mediating variable that links the independent and dependent variables, that is, a conceptual variable that is influenced by the conceptual independent variable and that, in turn, influences the conceptual dependent variable.

In an experiment, the conceptual independent variable, an abstract concept, is operationalized in a concrete way by the experimental treatment. The conceptual dependent variable, also an abstract concept, is operationalized in a concrete way by the dependent measure. The experimental treatment and dependent measure are chosen because the researcher believes that an effect of the experimental treatment on the dependent measure will provide evidence of a causal relation between the conceptual independent variable and the conceptual dependent variable. The evidence of a causal relation depends on what Cook and Campbell (1979) refer to as the construct validity of causes and effects.

Researchers assume that the experimental treatment has an effect on the dependent measure as the result of the following causal sequence: The experimental treatment is perceived by the experimental participants, differences in the perceived treatment create differences on the conceptual independent variable, differences on the conceptual independent variable create differences on the conceptual dependent variable, and differences on the conceptual dependent variable create differences on the dependent measure. This sequence is depicted in Figure 1.

In some cases the conceptual independent variable and the perceived treatment are the same. For example, if the conceptual independent variable is the perception of whether one is alone or another person is present, then if the participants perceive the treatment (i.e., that they are alone or another person is present), the conceptual independent variable was successfully manipulated. In social psychology experiments, the perceived treatment is more typically not the same as the conceptual independent variable, but rather the perceived treatment creates differences in the independent variable.

If a mediating variable is proposed, the sequence involved in the conceptual interpretation of the experimental result includes the additional assumptions that the conceptual independent variable creates differences on the mediating variable, and the mediating variable creates differences on the conceptual dependent variable. This is also depicted in Figure 1. In some cases the distinction between an independent variable and a mediator may be blurred. What is considered the conceptual independent variable by one researcher may be considered a mediating variable by another. Note that in the conceptual interpretation of the experimental result, both the conceptual independent variable and the mediating variable (if proposed) mediate the effect of the experimental treatment on the dependent measure.

In order to examine the sequence involved in the conceptual interpretation of an experimental result, and the issues involved in the use of measures of independent and mediating variables, we will consider a specific example. Suppose that an experiment is conducted to test the hypothesis that the greater a communicator's perceived expertise, the greater the attitude change toward the communicator's conclusion. In one experimental condition (labeled *high expertise*) the communicator is described as a mutual fund manager, and in another condition (labeled *low expertise*) the communicator is described as a video rental store clerk. Following the communication, a measure is taken of agreement with the communicator's conclusion (the dependent measure). The test of the experimental hypothesis assumes that the different descriptions of the communicator's occupation are noticed (perceived treatment), the perception of the communicator's occupation influences the communicator's perceived expertise (the independent variable), perceived expertise influences attitude change (the dependent variable), and attitude change is reflected in scores on the dependent measure.

In such an experiment, it might be further hypothesized that the effect of perceived expertise on attitude change is mediated by the audience's thoughts favorable to the communicator's conclusion. In this case, the test of the experimental hypothesis also assumes that perceived expertise influences favorable thoughts and that favorable thoughts influence attitude change.

In addition to the dependent measure assessing the conceptual dependent variable of attitude change, additional measures might be taken to check on the various assumptions involved in the conceptual interpretation of the effect of the experimental treatment on the dependent measure. A measure of the independent variable, perceived expertise of the communicator, might be taken to check on whether the different ways the

[4] Of course, more than one independent variable may exist in the experiment, and the hypothesis may specify an interaction between the independent variables.

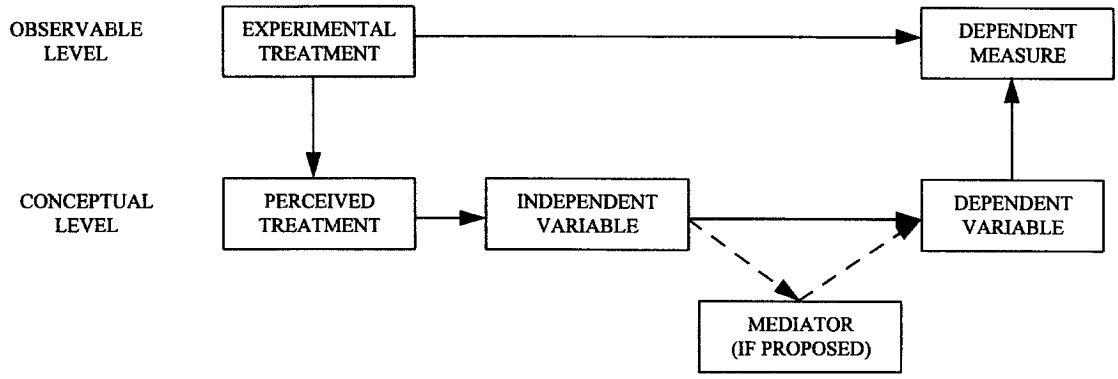

Figure 1. The conceptual interpretation of the effect of the experimental treatment on the dependent measure.

communicator was described in the different experimental conditions did in fact create the intended differences between the conditions in the conceptual variable of perceived expertise.[5] Showing that the conditions differed on a measure of the independent variable is especially useful if the assumption that the experimental treatment manipulates the conceptual independent variable lacks plausibility.

A measure of the independent variable is usually called a *manipulation check,* and that was the meaning we had in mind when the term *manipulation check* was used in the survey of social psychologists. In writing this article, we encountered a different usage of the term *manipulation check.* Sometimes that term is used to refer to measures taken to check on the assumption that the differences between the experimental conditions were noticed. The participants in the experiment might be asked to recall what was said about the communicator. If, in answer to the question, "Who was the communicator?" all participants recall correctly that the communicator was described as a mutual fund manager in the high-expertise condition and a video rental store clerk in the low-expertise condition, that would demonstrate that the differences between the experimental conditions were noticed.

Demonstrating that the experimental treatment was perceived would not, in itself, demonstrate that perceived expertise was greater in the high-expertise condition than in the low-expertise condition. The participants could have noticed the descriptions of the communicator but not perceived a difference in expertise, and thus the conceptual variable of perceived expertise may not have actually differed between the experimental conditions. A measure checking whether differences between experimental conditions were noticed can not be assumed to check whether the conditions differed on the independent variable (except, of course, in cases when the perceived treatment and conceptual independent variable are the same). On the other hand, differences between conditions on a measure of the conceptual independent variable can be taken as evidence that the differences between the conditions were noticed.[6]

To distinguish measures used to check on whether the differences between experimental conditions were noticed (e.g., recall measures) from measures used to check on whether the conceptual independent variable was manipulated by the experimental conditions, we will use the term *treatment check* to refer to measures of the perceived treatment.[7] Because of the different possible meanings of the term *manipulation check,* when discussing measures used to check whether the conceptual independent variable was manipulated, we refer simply to independent variable checks.

We should note that in some cases a measure intended as an independent variable check may not provide a valid measure of the conceptual independent variable even though it appears to be an obvious measure of the independent variable. When an independent

[5] An important part of the routine pilot testing of experimental procedures involves questioning participants to determine whether the manipulation of the independent variable is likely to be strong enough to produce the predicted experimental result. Such questioning is often informal and not part of the evidence presented in the formal report of the experiment.

[6] Differences between the experimental conditions on the dependent measure can also be taken as evidence that the treatment was noticed.

[7] A treatment check can be used to exclude a participant's data if the treatment check shows that the participant did not attend to the treatment. That is similar to excluding data because of an equipment failure. Excluding data based on a participant's score on a measure of the independent variable is not appropriate in an experimental study.

variable check is obvious, differences between the experimental conditions on the measure may be due to experimenter demand and may not provide good evidence that the experimental conditions successfully manipulated the conceptual independent variable.

If a mediating variable is proposed as part of the experimental hypothesis, obtaining a measure of the mediating variable, that is, obtaining a mediator check, may be possible. Obtaining a good mediator check can not be taken for granted because good mediator checks can be even more difficult to obtain than good independent variable checks. If one hypothesizes that favorable thoughts mediate the effect of perceived expertise on attitude change, a measure may be taken of the audience's thoughts to see if thoughts favorable to the communicator's conclusion were greater in the high-expertise condition than in the low-expertise condition.

Figure 2 depicts the use of different measures (treatment checks, independent variable checks, and mediator checks) to check on assumptions in the conceptual interpretation of the effect of the experimental treatment on the dependent measure. Differences between the experimental conditions on the treatment check provide evidence for the assumption that the experimental treatment was perceived. Differences on the independent variable check provide evidence for the assumption that the experimental conditions differed on the conceptual independent variable, and differences on the mediator check that they differed on the mediating variable.

In addition to treatment checks, independent variable checks, and mediator checks, an experiment might include other checks as well. Typically experiments include a suspicion check to determine whether the participants had doubts about the cover story or suspected the purpose of the experiment, suspicions that would invalidate the test of the hypothesis. Experiments dealing with the effects of stimuli presented at levels intended to be below the level of awareness routinely include checks to determine whether participants were aware of the stimuli. Such awareness checks share a similarity with treatment checks, except that the experimenter is not looking for differences between the experimental conditions, but rather hopes to find no evidence of awareness of the stimuli and no differences between the conditions.

The Role of Measures of Independent Variables

Let us consider the role of data from an independent variable check in drawing conclusions from an experiment. Assume that the results of the experiment are that the expected differences on the dependent measure were indeed observed (participants in the high-expertise condition showed more agreement with the communicator on the dependent measure than did participants in the low-expertise condition), that no plausible alternative explanation of those differences exists and that an independent variable check indicated the manipulation of the conceptual independent variable was effective (participants in the high-expertise condition rated the communicator as having more expertise than did participants in the low-expertise condition). Does the independent variable check provide additional information concerning the validity of the conclusion that the conceptual independent variable influenced the conceptual dependent variable?

The validity of the conclusion that the conceptual independent variable influenced the conceptual dependent variable depends on what Cook and Campbell (1979) referred to as the construct validity of causes and

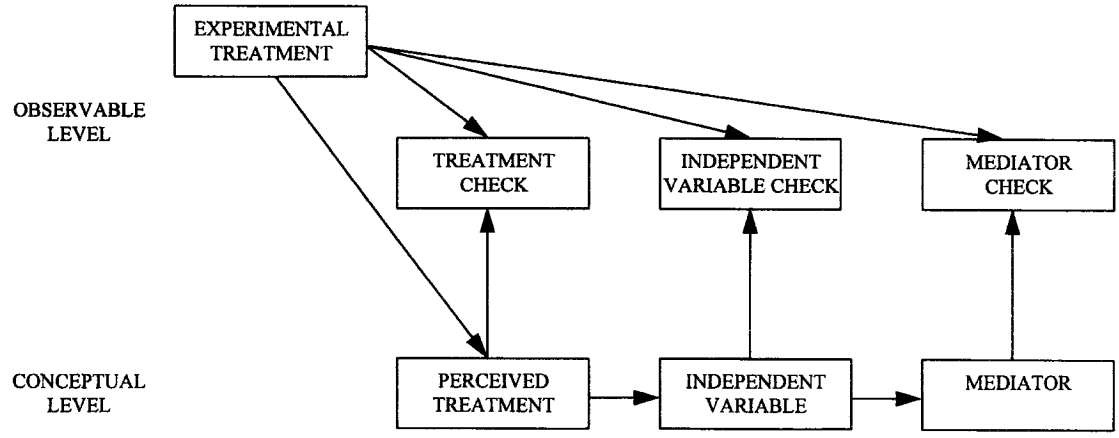

Figure 2. Measures used to check on assumptions in the conceptual interpretation.

effects. In discussing construct validity, Cook and Campbell state:

> Construct validity is what experimental psychologists are concerned with when they worry about "confounding." This refers to the possibility that the operations which are meant to represent a particular cause or effect construct can be construed in terms of more than one construct, each of which is stated at the same level of reduction. Confounding means that what one investigator interprets as a causal relationship between theoretical constructs labeled A and B, another investigator might interpret as a causal relationship between two constructs A and Y or between X and B or even between X and Y. (p. 59)

If an experimental result is open to the criticism of confounding because the operations can be construed in terms of more than one construct, that means a plausible alternative explanation of the effect of the experimental treatment on the dependent measure exists. If no plausible alternative explanation exists, there is no plausible way to account for the effect of the experimental treatment on the dependent measure without assuming the conceptual independent variable influenced the conceptual dependent variable.

When No Alternative Explanation Exists

The case we are considering posits that no plausible alternative explanation of the differences obtained on the dependent measure exists. Thus, the answer to the question about whether the independent variable check provides additional information concerning the validity of the conclusion that the conceptual independent variable influenced the conceptual dependent variable is that it does not. The results on the dependent measure were as expected from the hypothesis that the conceptual independent variable influenced the conceptual dependent variable, and with no plausible alternative explanation we can conclude that the hypothesis was supported regardless of whether an independent variable check was taken.

Now suppose that, although the results on the dependent measure were as expected and no plausible alternative explanation has been raised, the independent variable check showed no differences (ratings of the expertise of the communicator in the high- and low-expertise conditions did not differ). This might be a surprising result, but how should it affect the interpretation of the main finding? If the proposed explanation is not threatened by a plausible alternative explanation, the results on the independent variable check would not change the evidence provided by the differences between the experimental conditions on the dependent measure. The investigator would probably try to account for the results on the independent variable check on the grounds that the measure of the independent variable was insensitive and failed to detect differences in the conceptual independent variable. But whatever is said about the failure to find differences on the independent variable check, what remains is that the experimental conditions designed to vary the conceptual independent variable produced the expected variation in the measure of the conceptual dependent variable, and there is no plausible alternative explanation for the results.

When no plausible alternative explanation for an experimental result exists, a successful independent variable check does not add anything necessary, and an unsuccessful independent variable check does not subtract anything necessary. It follows that in such experiments, failure to have a independent variable check is not a flaw. An unambiguous finding is not made ambiguous by the absence of a independent variable check.

Our analysis of the role of independent variable checks when no plausible alternative explanation exists would have no relevance if, as is sometimes said, one can always find an alternative explanation for every experimental result. However, assuming that all experimental results are or will be subject to a plausible alternative explanation is not reasonable. A plausible alternative explanation, which would state how the experimental conditions influenced the dependent measure without assuming that the presumed conceptual independent variable influenced the presumed conceptual dependent variable, is not inevitable in experiments. Many experiments are not subject to a plausible alternative explanation undermining the conclusion that the conceptual independent variable influenced the conceptual dependent variable (e.g., Zajonc's, 1968, experiments that showed that the number of exposures to unfamiliar stimuli influenced preferences for the stimuli).

The belief that alternative interpretations are ubiquitous stems from not distinguishing an alternative explanation from a different general theoretical account of the experimental result (Mills, 1977). A different general theoretical account may well be offered for each and every experimental result. However, that is not the same as proposing an alternative explanation. A different general theoretical account accepts the existence of a relation between the conceptual independent variable and the conceptual dependent variable and interprets the relation in a different way. An alternative explanation questions the existence of a relation between the conceptual independent variable and the conceptual dependent variable and explains away the experimental result.

The distinction between explaining away a result and interpreting it in a different way may be illustrated by considering Aronson and Mills's (1959) experiment. An alternative explanation to the conclusion that the severity of the initiation undergone to join a group

(conceptual independent variable) influences liking for the group (conceptual dependent variable) might state that the experimental conditions varied sexual arousal (a different conceptual variable) that influenced liking for the group. A different general theoretical account of the Aronson and Mills experiment would not assume that the experimental result was due to the operation of some variable other than the effect of severity of initiation on liking for the group. For example, a self-perception interpretation of the Aronson and Mills experiment would assume that the experimental result occurred because severity of initiation increases liking for a group, but it would provide a different reason for why severity of initiation increases liking for a group than the dissonance interpretation by Aronson and Mills.

When There Is an Alternative Explanation

We turn next to the role of independent variable checks in experiments in which the predicted results are found but an alternative explanation is plausible. To extend the example we have been using, suppose that the communication used in the experiment advocated increasing the progressiveness of income tax rates. Although the experimenter might hope to interpret the results as evidence for the effect of perceived expertise on attitude change, an alternative interpretation is plausible. The greater attitude change in the high-expertise condition could be due to differences in perceived objectivity. The mutual fund manager could have been seen as arguing against her own financial self-interest because, as someone well-off financially, she would pay more taxes if what she advocated went into effect, whereas the video rental clerk could have been seen as benefiting financially if tax rates became more progressive. The plausible alternative explanation undermines confidence in the conclusion that perceived expertise increased attitude change.

How would an independent variable check play a role here? If the independent variable check showed greater expertise attributed to the mutual fund manager, that would not reduce the ambiguity. Perceived expertise may indeed have been manipulated in the experiment, but that does not establish that perceived expertise caused the attitude change. The results could still be due to differences in perceived objectivity. Having a check on the independent variable will not in itself protect against alternative interpretations (Mills, 1969, p. 445). An ambiguous finding does not become disambiguated by the presence of a successful independent variable check. Correspondingly, an unsuccessful independent variable check does not make the case for the alternative explanation. Failure to demonstrate that perceived expertise was manipulated effectively does not establish that perceived objectivity caused the attitude change.

If one wants to rule out an alternative interpretation for an experimental result, the best way to do it is to conduct another experiment with an improved experimental procedure that is not subject to the alternative interpretation. In the case of the experiment we have been discussing, one would do another experiment in which perceived objectivity was held constant while perceived expertise was varied experimentally.[8]

In some unusual instances the important experimental result is that the experimental treatment did not have an effect on the dependent measure. Before drawing the conclusion that the conceptual independent variable did not influence the conceptual dependent variable, one would need to rule out the possibility that the experimental treatment did not vary the conceptual independent variable.[9] An independent variable check is obviously useful in such instances. Although obtaining a measure of the independent variable is the preferred approach in such cases, it is not absolutely necessary. Evidence that the manipulation of the independent variable was successful could be provided by other results in the study. If the experiment includes a second dependent measure that measures a different conceptual dependent variable that is influenced by the conceptual independent variable, the effect of the experimental treatment on the second dependent measure can be used

[8] A tangentially related matter is how the initial experiment should be conducted to minimize unforeseen alternative explanations. One can argue that if the initial experiment included a number of manipulations of the independent variable, then it would protect against alternative explanations that might be proposed in the future. This multiple exemplar approach, which involves doing a number of conceptual replications of the experiment simultaneously, can be contrasted with an approach in which one waits until an alternative explanation for an experimental result is specified and then, if it is, attempts to rule it out in subsequent experiments. Whether the multiple exemplar approach is more or less efficient than the "when specified rule it out" approach cannot be answered in the abstract. The multiple exemplar approach rests on the assumption that it would eliminate alternative explanations for the results of the single exemplar experiment that currently cannot be specified, an assumption that would prove warranted only if an alternative explanation for the single exemplar experiment is later proposed and if the particular experimental conditions used in the multiple exemplar experiment would rule out that specific alternative explanation. In the absence of a specific alternative explanation, one has reason to wonder if the multiple exemplar approach, which may incur substantial additional costs, is likely to be efficient. A specific alternative explanation may not be proposed, or, if one is, it may need to be addressed by a new experiment anyway.

[9] Failure to manipulate the independent variable is not the only reason why an experiment will fail to find a statistically significant effect of the experimental treatment on the dependent measure when in fact a causal relation exists between the conceptual independent and dependent variables. The measure of the dependent variable could be inadequate, or it could be that many other factors affecting the dependent variable varied in the experimental situation.

as evidence that the experimental treatment manipulated the independent variable.

The Role of Measures of Mediating Variables

The role of measures of mediating variables in experiments is similar to that of measures of conceptual independent variables. From an experimental point of view, a mediator check is similar to an independent variable check.

Suppose that the predicted differences on the dependent measure are observed, that no plausible alternative explanation involving a different possible mediator exists, and that the results on the mediator check indicate that the mediator was influenced as expected by the experimental manipulation of the independent variable. Does the mediator check provide evidence that the proposed mediating variable was responsible for the effect of the independent variable on the dependent variable? Again, the answer is *no*. If the only plausible explanation for the experimental results is that favorable thoughts mediated the effect of perceived expertise on attitude change, we could conclude that the hypothesis involving the proposed mediator had been supported with or without the mediator check. If the mediator check failed to reveal differences between the experimental conditions, the experimenter would probably discount the mediator check as insensitive, as long as no plausible alternative explanation of the results involving the operation of a different mediator exists.

In the case of a proposed mediating variable, an alternative explanation states how the experimental result occurred without assuming that the conceptual independent variable influenced the proposed mediating variable that influenced the conceptual dependent variable. Again, an alternative explanation should not be confused with a different general theoretical account of the operation of a mediator. A different general theoretical account would not question the conclusion that the mediator links the independent and dependent variables but would give a different theoretical reason for why the mediator links the independent and dependent variables. For example, using Chaiken's (1980) heuristic-systematic model rather than Petty and Cacioppo's (1981) elaboration likelihood model to interpret why thoughts about the communication is a mediating variable in attitude change would provide a different general theoretical account. It would not be an alternative explanation of an experimental result showing that thoughts about the communication was a mediator of attitude change because it would not assume the result was due to the operation of some other mediating variable.

If the predicted results on the dependent measure are found in an experiment but a plausible alternative explanation involving a different possible mediator exists, a measure of the proposed mediator will not resolve the ambiguity. Showing that the experimental conditions designed to vary the independent variable, perceived expertise, created differences on the check of the proposed mediating variable, favorable thoughts, would support one assumption involved in the hypothesized mediation. However, that, in itself, would not provide evidence that the proposed mediating variable is the actual mediator. Even using Baron and Kenny's (1986) recommended statistical procedures for testing mediational hypotheses would not rule out the possibility that some other variable was responsible for the effect of the conceptual independent variable on the conceptual dependent variable. If it is plausible, say, that the results occurred because perceived expertise increased liking for the communicator, which increased attitude change, that possibility would not be ruled out by showing that the conditions designed to manipulate perceived expertise increased favorable thoughts.

When there is ambiguity in an experiment concerning a proposed mediating variable, the best way to show that the proposed mediator is responsible for the influence of the conceptual independent variable on the conceptual dependent variable is to conduct another experiment that manipulates the proposed mediator. If the experimenter, based on an analysis of the mediational process, can specify the conditions under which the mediating variable will or will not be present, varying those conditions and determining whether they influence the effect of the independent variable on the dependent variable may be possible.

To test whether the effect of perceived expertise on attitude change is mediated by thoughts favorable to the communicator's conclusion, an experiment might be done in which conditions varying the opportunity for favorable thoughts are crossed with conditions varying perceived expertise. For example, in some experimental conditions thoughts favorable to the communicator's conclusion might be prevented by the presence of a distracting task. If the difference in the measure of attitude change between the high- and low-expertise conditions is eliminated when the distracting task is present but occurs when the distracting task is not present, this result would provide evidence that favorable thoughts mediate the influence of perceived expertise on attitude change, assuming, of course, the absence of a plausible alternative explanation.

Why Is There Disagreement?

If our position on the necessity of measures of independent variables and mediating variables in experiments is correct, then why do many professional social psychologists disagree and regard experiments lacking such measures as flawed? The usefulness of employing

independent variable checks and mediator checks in social psychology experiments has led to their increased usage, and independent variable checks and (to a lesser extent) mediator checks have become common in social psychology experiments. Perhaps the common inclusion of such measures in experiments has produced the view that their absence is inappropriate. What is useful and common in experiments has come to be regarded as necessary in experiments.

Another possible explanation for the view that experiments lacking independent variable checks and mediator checks are flawed is that the difference between experimental studies and nonexperimental studies has become blurred. In nonexperimental studies, it is necessary to measure variables other than the dependent variable of interest. Perhaps the necessity of including such measures in nonexperimental studies has become confused with their necessity in experimental studies.

Whatever the origin of the misunderstanding about the necessity of including independent variable checks and mediator checks in experiments, it should be clear that their inclusion does not solve the fundamental problem in experiments of eliminating plausible alternative explanations for the effect of the experimental treatment on the dependent measure. Although the inclusion of measures of independent variables and mediators is useful in most social psychology experiments, it is not a necessity for a well-designed experiment.

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