Coping with the Attribution Problem in Program Evaluation

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The key question in program evaluation is whether the intervention is working or not. But to answer that question, first program outcomes must be attributed to the intervention, and not some other factor. The attribution issue arises in this context because a program, as a means-ends relationship, interacts with contextual factors that may strengthen, reduce, or break the intended causal link between the intervention and the outcomes of interest. These factors therefore represent alternative explanations for any association between a program and observed outcomes. This note reviews two basic frameworks for resolving the attribution problem, including their limitations. Both the counterfactual framework and the theory-based approach rely on the logic of causal inference, which entails establishing a plausible association between the intervention and the outcome of interest, and ruling out alternative explanations of that association.

Useful evaluations provide credible evidence to answer policy or programmatic questions that decision makers and other stakeholders care about (Gertler et al. 2011). An evaluation is therefore characterized fundamentally by the type of questions it answers and not by the designs and methods used. These questions reflect the information needs of decision makers (given the purpose of the evaluation) and determine the appropriate design and methods. Those who have a significant stake in the program in terms of committed resources or the social problem addressed by the program would like to know whether or not, and to what extent, the program is achieving its intended results. This key evaluation question has two dimensions: (i) is the observed change in the development outcome (for example, attitudes, values, knowledge, skills, behavior, or status) due to the program, and (ii) is the observed change consistent with the intended results? The first question raises the attribution issue, which is at the heart of questions about effectiveness and the focus of impact evaluation.

In the context of evidence-based decision making, policy makers and other stakeholders are interested in knowing what works, what does not, and why. The attribution issue arises because a program interacts with contextual factors such as other interventions, socioeconomic trends, and political or environmental conditions. These contextual factors may strengthen or reduce the causal link between the intervention and the outcomes of interest. These factors thus provide alternative explanations of observed outcomes. Resolving the attribution problem entails establishing a credible link between the intervention and the observed effects while accounting for the influence of contextual factors. The information produced in this process can guide budget allocation and other important policy decisions and thus increase accountability.

Answering the question of whether a program is worth its cost also involves attribution. The relationship between the cost of a program and its key outcomes or benefits is meaningful only if it can be established that the benefits under consideration are attributable to the relevant program. Finally, accounting for attribution can support learning by producing knowledge about development effectiveness that policy makers can rely upon to improve current and future policies and programs.
This note reviews basic approaches to accounting for attribution in program evaluation, highlighting each approach’s underlying logic, strengths, and limitations. Since one cannot observe directly how an intervention induces change in a development outcome, such causal relationships must be inferred from observed patterns in the data (de Vaus 2001). This is the key idea underpinning the two basic frameworks considered in this note. The standard counterfactual framework entails a comparison of what happened with an estimate of what would have happened in the absence of the program. The theory-based approach embeds a program theory within the logic of causal inference.

**The Standard Counterfactual Framework**

One can learn a great deal about an evaluative approach by considering its *informational basis*, meaning the information that is required in passing judgments using that approach (Sen 1999). Since evaluation is about answering key policy or programmatic questions, the informational basis of an answer to an evaluation question determines the type of evidence needed to answer the question under consideration. This evidence entails two basic elements: facts, and the logic that organizes those facts into evidence. Here, the focus is on the logic of standard counterfactual analysis and its implications for the design of an impact evaluation, while also highlighting some of its limitations.

**Underlying logic**

To confirm that an observed change in a development outcome is due to a program of interventions, one must show that there is an association between program implementation and the change in outcome, and that the change would not have occurred in the absence of the program. In other words, analysis must show that the change occurs where and when the program is implemented, and that the change does not occur if the program is not implemented (Trochim 2006). The second condition is consistent with the fundamental idea that the effect of a cause can be understood only in relation to another cause (Holland 1986). This idea is the bedrock of standard counterfactual analysis, and is akin to assessing the return to a resource engaged in a socioeconomic activity on the basis of its opportunity cost (that is, what it would have earned in the next best alternative use). Thus, to address the attribution problem, the analysis measures what happened with the intervention and compares it with an estimate of what would have happened had the intervention not been implemented (that is, the counterfactual). The standard counterfactual framework is based on the notion that the intervention is necessary and sufficient for the achievement of the intended change in the development outcome.

Recall that the attribution problem stems from the interaction between the program and contextual factors (or confounders) that may also affect outcomes. This creates uncertainty about the causal relationship that is supposed to hold between the intervention and the intended results. The credibility of an evaluation is based on the truth or validity of its conclusions. The plausibility of alternative explanations of the association between the intervention and the outcome threatens the validity of the conclusion that the intervention caused the observed change. Thus, the quality of the chosen design and methods hinges critically on the ability to rule out plausible confounders. The basic design strategy therefore seeks to maximize the validity of findings and conclusions subject to relevant constraints. Put differently, to enhance the credibility of an evaluation, one should strive to choose designs and methods that would minimize the number and plausibility of threats to validity that might remain at the end of the process (Shadish, Cook, and Campbell 2002). This basic strategy underlies not only the counterfactual comparisons considered next, but also the theory-based approach to impact evaluation.

**Counterfactual comparisons**

Consider a one-shot case study in which a particular individual is exposed to an intervention and then the outcome of interest is measured. This process clearly indicates that treatment precedes the observed outcome. However, there is nothing to confirm the treatment effect on the basis of this single observation because there is no frame of reference. For instance, because there was no measurement prior to treatment (that is, a pretest), it is difficult to tell whether any change occurred. Furthermore, there is no information on what would have happened to the individual in the absence of this intervention.

By definition, the effect of the intervention on this individual is equal to the difference between the observed outcome and the outcome that would have been observed for the same individual if he or she had not been treated. In other words, the effect of an intervention on an individual is the difference in the outcome of interest for that same individual with and without exposure to the intervention. This individual effect is unobservable since it is impossible to observe the same individual simultaneously in two mutually exclusive states of the world. This is the missing data problem that makes it difficult to answer attribution questions with certainty.

One way of estimating the counterfactual outcome is to consider the outcome of a program participant as a
Self-selection occurs when potential participants are invited by choice (self-selection) or by administrative selection. Usually people get into a program either by choice (self-selection) or by administrative selection. The basic idea is to compare the change in outcome for people of the same type, one of whom received the intervention and the other did not. Suppose that one can find among nonparticipants an individual that is exactly of the same type as the treated individual in the sense of having exactly the same observable and nonobservable characteristics. The outcome of this particular nonparticipant can be used as a proxy of what would have happened to the treated individual, if he or she had not been exposed to treatment. The program effect on the treated individual is therefore equal to the difference between his or her outcome and that of the equivalent nonparticipant. In fact, if the population is entirely homogeneous, then the above individual effect is also the average effect for the entire population.

The validity of the above conclusion hinges critically on the assumption that the population is entirely homogeneous. In real life situations, there is a great deal of diversity among individuals based on characteristics such as internal attributes (for example, will and ability) and external circumstances (for example, ownership of assets, access to social support, and environmental factors). This heterogeneity can confound the identification and hence the estimation of the effect of an intervention. The quality of an evaluation design based on the counterfactual framework depends on how well the design controls for heterogeneity.

The above considerations suggest that the way people are selected into a program affects the ability of a with-and-without comparison to lead to a valid conclusion on attribution. Selection to a program can be random or not. Randomization assigns eligible individuals randomly into the group that will receive the intervention or to a control group that is excluded from the program. Thus, the control group is composed of individuals who would have participated in the program, but were randomly denied access. Random assignment ensures that the distribution of both observed and unobserved characteristics prior to the intervention is the same for both the participants and nonparticipants. Observed ex post differences between these two groups in outcomes must therefore be due to the intervention.

By design, exposure to most development interventions is nonrandom. Such interventions are targeted at specific segments of the population, such as the poor or the vulnerable. Usually people get into a program either by choice (self-selection) or by administrative selection. Self-selection occurs when potential participants are invited to submit an application to participate in a program, for example, when communities are invited to prepare and submit projects for funding to a social fund. Participating communities may be more motivated than nonparticipating ones. This motivation is also a key determinant of outcomes, such that participating communities would fare better than nonparticipating ones even in the absence of the intervention. This difference in achievement between participants and nonparticipants in the absence of the program is known as selection bias, a threat to the validity of conclusions based on with-and-without comparisons. Similarly, there is selection bias associated with administrative selection to the extent that eligibility is based on a set of specific characteristics, and the treatment and comparison groups may differ in other characteristics not included in the eligibility criterion.

One may think that a possible way around selection bias is to measure the outcome of the same individual before and after treatment and use the difference between the two as an estimate of the effect of the intervention. For sure, this before-and-after comparison is an improvement to the one-shot case study to the extent that it provides a frame of reference for computing the change in outcome. However, one cannot conclusively claim that the observed change in this individual’s condition is due to the intervention and the intervention only. Indeed, external events unrelated to program participation may have also affected the outcome. In other words, the before-and-after comparison is vulnerable to the threat of history.

One way to effectively deal with this threat is to find a comparable individual who is not exposed to treatment and observe his or her outcome at the same time as observing the outcome of the treated individual. The presumption here is that both individuals are subject to the same external events, so that whatever happens to one is supposed to happen to the other, except for the treatment. Hence any difference in the variation of their outcomes must be due to the intervention. In this case, the comparison individual acts as a recorder of history (Mohr 1995). One can therefore use the change in his or her outcome over time to account for the effect of external events. In particular, the effect of the intervention is obtained by a comparison of outcomes not only before and after the intervention, but also with and without it (Morra Imas and Rist 2009). The before-and-after comparison applies to each group separately, while the with-and-without comparison is made across groups within each time period.

Box 1 presents an interesting application of counterfactual comparison to an impact evaluation of a nutrition program in Madagascar. In particular, the application relies on both before-and-after and with-and-without comparisons to deal with selection bias and threats of history.
Table 1. The Logic of Counterfactual Comparisons

<table>
<thead>
<tr>
<th></th>
<th>Intervention</th>
<th>No intervention</th>
<th>Difference across groups</th>
</tr>
</thead>
<tbody>
<tr>
<td>Before</td>
<td>b</td>
<td>b</td>
<td>0</td>
</tr>
<tr>
<td>After</td>
<td>a</td>
<td>c</td>
<td>a-c</td>
</tr>
<tr>
<td>Difference over time</td>
<td>a-b</td>
<td>c-b</td>
<td>(a-c)</td>
</tr>
</tbody>
</table>

Source: Author’s compilation.

Table 1 illustrates the logic underlying counterfactual comparisons. Before the intervention, the outcome is $b$ for both the participants and nonparticipants. After the intervention, the outcome is $a$ for the treatment group and $c$ for the comparison group. The contribution of the intervention to the observed change in outcomes for the participants is equal to $(a-c)$. Gertler et al. (2011) explain that for counterfactual comparisons to lead to valid conclusions on attribution, at the very least, the treatment and the comparison groups must be similar in the following ways:

(i) in the absence of the intervention, the participants and the comparison group must have the same characteristics;
(ii) both groups should react the same way to the intervention; and
(iii) during treatment, both groups should have the same exposure to contextual factors (including other interventions)—the key point here is to avoid “comparing apples and oranges.”

There are a variety of quantitative and qualitative techniques for implementing the logic presented in table 1. A review of such techniques is beyond the scope of this note, however, Essama-Nssah (2004) provides a general discussion of some of these techniques.

One key limitation of the standard counterfactual framework is the range of interventions that can be evaluated using this approach. Counterfactual comparisons are more useful in cases of simple, discrete, and homogenous interventions such as conditional cash transfers. Simple interventions tend to involve standardized activities implemented by a single organization, and work more or less the same everywhere (Funnell and Rogers 2011). Development programs designed to promote the Millennium Development Goals (MDGs), for instance, tend have complicated or complex aspects in the form of multiple components or nonstandardized and changing strategy. Furthermore, results are sensitive to initial conditions and context. These types of interventions present a significant challenge to an evaluation based on the standard counterfactual framework. In particular, the scope of an intervention can be so wide that it would not be possible to define and credibly estimate a counterfactual in terms of a control or comparison group.

Policy makers are interested in evidence on what works, what does not, and why. The standard counterfactual framework is capable of generating evidence on the first two issues, however, it cannot provide an explanation for the observed outcome. This requires causal explanation as opposed to causal description. Causal description is a statement of the consequences attributable to a deliberate variation of an intervention (Shadish, Cook, and Campbell 2002). Causal explanation clarifies the mechanisms and conditions under which the intervention works.
Accounting for Causal Pathways and Implementation Processes

Theory-based or theory-driven evaluation is an approach guided by an explicit theory or model of how the program causes the intended outcomes. It helps assess the effect of a program in a way that accounts for both the underlying causal mechanisms and implementation processes. It can thus provide information not only on what works and what does not, but on how and why a program succeeds or fails. A program theory provides the rationale for the intervention under consideration to the extent that it explains why the intervention is the right thing to do for the target population given the problem and the circumstances they face. It also explains how the intervention will be delivered to the target population. In essence, a program theory is the knowledge base of an intervention. It has two basic components: the theory of change, which deals with causal pathways, and the theory of action, which provides a rationale for implementation processes.

Gertler et al. (2011) identify three basic elements in a theory of change: (i) a causal chain, (ii) outside conditions and influences, and (iii) key assumptions. The causal chain translates the understanding of the technical and behavioral relationships that determine the transformation of program inputs into outputs, and then outputs into outcomes. A theory of action prescribes program components and activities considered by stakeholders as critical for the success of the intervention. Implementation processes are designed to “put the relevant theory of change on wheels,” in the sense that they are the means for making the theory of change operational. In particular, these processes involve (Chen 2005):

(i) the implementing organization,
(ii) intervention and service delivery protocols,
(iii) program staff (or implementers),
(iv) partners such as peer organizations and the community,
(v) program context, and
(vi) the target population.

Approach

There is a core question that underlies most evaluations, whether the evaluation is for accountability or learning. The question is whether and the extent to which what was supposed to happen did, in fact, happen. In the case of an impact evaluation, the intervention is supposed to cause a change in the relevant development outcomes. To answer this core question, the analysis needs to: (i) measure what does happen, (ii) compare it with what was supposed to happen, and (iii) explain any significant discrepancies between the two states (Frechtling 2007). This is the outline of the informational basis of the theory-based approach to program evaluation: the performance measurement system provides information on what is happening, while the program theory describes what was supposed to happen.

In general, the theory-based approach is characterized by the following core activities (Coryn et al. 2011):

(i) development of a plausible program theory;
(ii) formulation and prioritization of evaluation questions around the selected program theory;
(iii) designing, planning, and conducting evaluation on the basis of the chosen program theory;
(iv) focus of measurement on processes, outcomes, and contextual factors presented in the program theory; and
(v) use of program theory to identify breakdowns and side effects (or unintended results), and to determine the extent and causes of program effectiveness.

Mayne (2001) proposes a theory-based approach to causal inference designed to address attribution through what he calls contribution analysis. The notion of contribution stems from the view that an intervention works alongside contextual factors to produce the observed outcomes. The attribution question can therefore be equivalent to asking what difference the program makes in bringing about the observed outcomes. To answer this question, we need to look for consistency of outcomes with program theory and investigate alternative explanations.

In essence, addressing attribution through contribution analysis amounts to integrating performance (or process) and impact evaluations. The implementation of that approach entails four core steps:

(i) developing a results framework;
(ii) checking for congruence between observed results and program theory;
(iii) identifying and assessing alternative explanations; and
(iv) assembling and assessing the performance story.

If doubts remain at the last step, and there are resources available, Mayne (2001) suggests two additional steps that involve seeking additional evidence to reduce the leftover uncertainty and revising and strengthening the performance story.

Results framework. A results framework is an explicit representation of the logical connections between different levels of results expected from a specific intervention (IEG 2012). In particular, it shows causal links between the goods and services provided by an intervention (outputs) and the outcomes that represent the benefits that the target population derives from these goods and services. Three levels of outcomes are usually distinguished in a results framework: short-term, intermediate, and long-term outcomes. The de-
development of a results framework is based on the theory of change underlying the intervention. Knowlton and Phillips (2009) characterize a theory of change as a set of strategies that could produce the intended results. They recommend that theories of change be developed on the basis of experience or other evidence that suggests plausibility. These considerations lead to a three-step approach to developing a theory of change: (i) a clear identification of intended results; (ii) the specification of relevant strategies; and (iii) a definition of the assumptions supporting the selected strategies. The results framework should involve process and outcome indicators that can be legitimately and reliably measured, subject to prevailing constraints.

**Congruence.** If the program is the right thing to do for the target population, then there should be a plausible association between program activities and the expected outcomes. This step is meant to answer the question: Did what was supposed to happen in fact happen the way it was supposed to? In general, to conclude that the observed results are consistent with the underlying program theory, first one must establish that (Funnell and Rogers 2011):

(i) the intervention was appropriate and adequately implemented;
(ii) there was sufficient engagement, uptake, and adherence to treatment;
(iii) intermediate outcomes were achieved; and
(iv) final outcomes were achieved as well.

One indicator of congruence is that participants who achieved the intended outcomes also achieved the intermediate outcomes predicted by the prevailing program theory, and those who have not achieved the intended results also failed to achieve the relevant intermediate outcomes.

**Alternative explanations.** Consistency of outcomes with program theory establishes a plausible association between the program and the observed outcomes. This association, however strong it might be, does not provide a firm enough basis for one to conclude that these observed outcomes were caused by the program. As noted earlier, the attribution problem stems from potential alternative explanations for the association between observed outcomes and the intervention. To conclude that this association is causal, one must identity at least the most plausible alternative explanations and assess their contribution relative to the program. Such an assessment is based on evidence or logical arguments for or against the contribution of nonprogram factors relative to the program. The discussion of counterfactual comparisons suggests that selection bias and history may provide alternative explanations for the observed outcomes. If such alternative explanations can be ruled out, then a credible causal link can be said to exist between the intervention and the observed outcomes. Box 2 offers an example from Bangladesh of a theory-based evaluation of a nutrition program that revealed that the observed outcomes were due to contextual factors rather than to the intervention.

It may or may not be possible to conclude that the intervention made a significant contribution to the observed outcomes relative to other possible influences. For policy learning purposes, success or failure should be explained. The criteria used to establish congruence also suggest a framework for explaining success or failure. When contextual factors are favorable to the intervention, the likelihood of success is higher when the intervention is appropriate, adequate, and targeted to the right segment of the population who will make use of it as prescribed (Essama-Nissah 2013). Clearly, the results of an intervention are an outcome of the interaction of the supply and demand sides of the intervention, subject to contextual constraints. If contextual factors are favorable, an intervention may fail due to poor design, meaning the intervention is based on a bad idea (theory failure). It may also fail because of inadequate implementation, engagement, or adherence. The incentives facing key actors play a critical role in accounting for these types of failures. Thus, in the delivery of social services, for instance, it should not be assumed that inputs into an activity would produce the intended results regardless of the incentive structure facing the key actors. In the case of health services delivery, Hammer (1997) points out that: “Just because a health clinic is built does not mean the providers will show up for work. And if they do come to work, there is no guarantee they will devote themselves to the care of their patients.” This is clearly a supply side failure. As an example of demand side failure, the initial design of the Nicaragua safety net program, the Red de Protección Social, contained a condition that eligible children be promoted to the next grade in order to stay in the program. Apparently, this led some schools to practice automatic promotion. The condition was later dropped when policy makers became aware of this distortion (Bastagli 2008).

**Performance story.** The performance story is designed to present credible evidence to answer the question of whether and to what extent the intervention did contribute to the observed outcomes. To claim success, it must be shown that: (i) the intervention was the right thing to do for the target population given the problem and circumstances they faced; (ii) what was supposed to happen did in fact happen the way it was supposed to happen, both on the supply and demand sides of the intervention; and (iii) alternative explanations are not plausible, or are less so. If possible, the degree of confidence associated with the conclusions presented should be stated as well.
Limitations
As the above discussion clearly demonstrates, theory-based approach to impact evaluation involves the use of program theory within the logic of causal inference. The approach can be hard to implement if there is no reliable theory to lean on, or when there are many competing theories. In the context of an impact evaluation of social fund investments, Carvalho and White (2004) provide an example of how to define and test competing program theories. Difficulties may also arise when the underlying theory involves concepts that are hard to measure. This latter difficulty can be alleviated by adopting mixed methods that combine quantitative and qualitative approaches. Qualitative approaches have a clear advantage over quantitative methods when dealing with attribution in the context of complex interventions (Leeuw and Vaessen 2009). These approaches can help with the identification of the objectives and scope of the program as well as with the specification of the theory of change underlying the intervention. Qualitative data can also help with the interpretation of quantitative results. Adato (2011) provides an excellent discussion of why mixed-method designs are best. She also identifies key elements of a good mixed-method design.

Concluding Remarks
A program is a means-ends relationship that is influenced by contextual factors. The attribution problem arises from the fact that these factors potentially provide alternative explanations for the observed outcomes. One must clearly factor in this issue when answering the question of whether an intervention is working or not. This note reviews the counterfactual framework and the theory-based approach for attributing observed outcomes to an intervention. Both approaches rely on the logic of causal inference, which entails establishing a plausible association between the intervention and the outcome and ruling out alternative explanations for that association. However, the standard counterfactual approach cannot explain the observed outcomes and is not applicable in cases of large and complex programs with a universal scope. Causal explanation requires the integration of performance and impact evaluations within a theory-based approach. In the end, there is no “gold standard” in dealing with attribution issues other than rigor in the collection, analysis, and interpretation of facts.

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B. Essama-Nssah worked for 17 years for the World Bank and retired as a Senior Economist in 2011. During his tenure at the World Bank, he performed economic analyses, prepared policy research and technical papers, and conducted an annual training course on impact evaluation methodologies for staff from the World Bank and client countries. Prior to joining the World Bank, Dr. Essama-Nssah worked for two years as a Senior Research Associate with the Food and Nutrition Program at Cornell University, and for six years as Head of the Economics Department and Vice Dean of the Faculty of Law and Economics of the University of

Box 2. A Theory-Based Evaluation of the Bangladesh Integrated Nutrition Project

The Bangladesh Integrated Nutrition Project (BINP) was a pilot predicated on the belief that poor nutrition was due more to ignorance than poverty. The program targeted pregnant women and malnourished children and provided counseling and supplemental feeding. White (2009) reports that an initial analysis of monitoring data found a substantial reduction in severe malnutrition in project areas. On the basis of this evidence, a decision was made to scale up the project at the national level to become the National Nutrition Program (NNP). A more rigorous impact evaluation based on the counterfactual framework was conducted later, comparing project areas with similar areas that did not receive the intervention. This evaluation found no significant impact of the program on nutrition status. A theory-based evaluation was conducted to try to understand these conflicting findings (White and Masset 2007). The evaluation focused on testing the assumptions underlying the way in which BINP was supposed to bring about change in nutritional status of the target population. The focal assumptions include:

(i) the target population knows about the program and participates,
(ii) the women attending the sessions do acquire knowledge,
(iii) the program targets the right people,
(iv) mothers are the right persons to receive nutritional counseling,
(v) acquired knowledge is turned into practice,
(vi) supplemental feeding is additional food for the beneficiaries, and
(vii) adopted changes are sufficient to improve nutritional outcomes.

Only the first two assumptions could be substantiated. It turned out the observed improvements in project areas were part of a national trend driven by an increase in rice yields and in incomes, and a fall in the price of rice. No wonder counterfactual comparisons found no significant difference between project and nonproject areas. This case clearly demonstrates that attribution cannot be settled on the basis of monitoring data alone; other plausible explanations must be ruled out.

Source: Author’s compilation.
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Notes

1. In the 1990s, evaluation experts discovered that the choice of methods depends much more on the nature of the questions asked rather than on the qualities of a particular method. This fundamental importance of policy and programmatic questions led to more consideration given to the use of mixed methods to counter the weaknesses of individual methods (Chelimsky 1995).

2. In general, impact evaluation methods based on counterfactual comparisons fall into two broad categories: (i) experimental (for example, randomized controlled trials or RCTs), and (ii) quasi-experimental (for example, matching, propensity score matching or PSM, regression discontinuity or RD, and difference-in-differences or DD). The difference-in-differences method follows closely the logic of table 1. Each method relies on a set of assumptions for the estimation of the counterfactual. Hence there is a risk of bias in the results to the extent that some of these assumptions may fail to hold. One mitigation strategy is to combine methods in order to offset the limitations of individual methods and achieve a robust estimate of the counterfactual.

3. An intervention protocol describes the content, schedule, and intensity of services (treatment) to be provided to the target group.

4. In principle, one can use counterfactual comparisons in this context to rule out the contribution of confounding factors to the observed outcomes.

References


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