Causal inference and Selection Bias: M&E vs. IE

Malathi Velamuri

Victoria University of Wellington

Workshop on Impact Evaluation of Public Health Programs

July 11-15, 2011 – Chennai

Causal Analysis

Seeks to determine the effects of particular interventions or policies, or estimate behavioural relationships

Three key criteria for inferring a cause and effect relationship:

(a) covariation between the presumed cause(s) and effect(s);

(b) temporal precedence of the cause(s); and

(c) exclusion of alternative explanations for cause-effect linkages.

- What is effect of scholarships on school attendance & performance (test scores)?
- Does access to piped water reduce prevalence of diarrhea among children?
- Does promotion of condom use and distribution of free condoms reduce transmission of STIs?
- Can rural ICTs be an effective tool to bridge the digital divide?

Why are causal answers needed?

- Association is not causation

  Ex: Does use of traditional fuels and cooking stoves cause respiratory illness?

- Perceived wisdom is not always correct

- Policymakers need to know the relative effectiveness of particular policy changes or interventions for prioritizing public actions

- Provide greater accountability in the use of aid and greater rigour in the assessment of development outcomes

- Increases transparency/ accountability – promotes good governance

M&E Vs. IE

M&E plays an important role in the management of programmes

Management tool

Objective: To ensure that resources going into the programme are being utilized, services are being accessed, activities are occurring in a timely manner, and expected results are being achieved.

Monitoring is concerned with *routine* tracking of service and programme performance using input, process and outcome information collected on a regular and ongoing basis from policy guidelines, routine record-keeping, regular reporting and surveillance systems, (health) facility observations and client surveys.

Evaluation is the *episodic* assessment of results that can be attributed to programme activities; it uses monitoring data and often indicators that are not collected through routine information systems.

Evaluation allows exploration of the causes of failure to achieve expected results on schedule and the mid-course corrections that might be necessary.

Process evaluation assesses progress in programme implementation and coverage.

Outcome/Impact evaluation measures the effect of programme activities on the target population.

Impact Evaluation is concerned with the net impact of an intervention on individuals, households and institutions, attributable exclusively to that intervention.

The purposes of impact evaluation

- Ex post evaluation

Objective is to ensure *internal* validity – is there a causal relationship between programme outputs and intervention leading to outcomes and impacts

- Ex ante evaluations

Objective is to ensure *external* validity - estimating the casual impact:

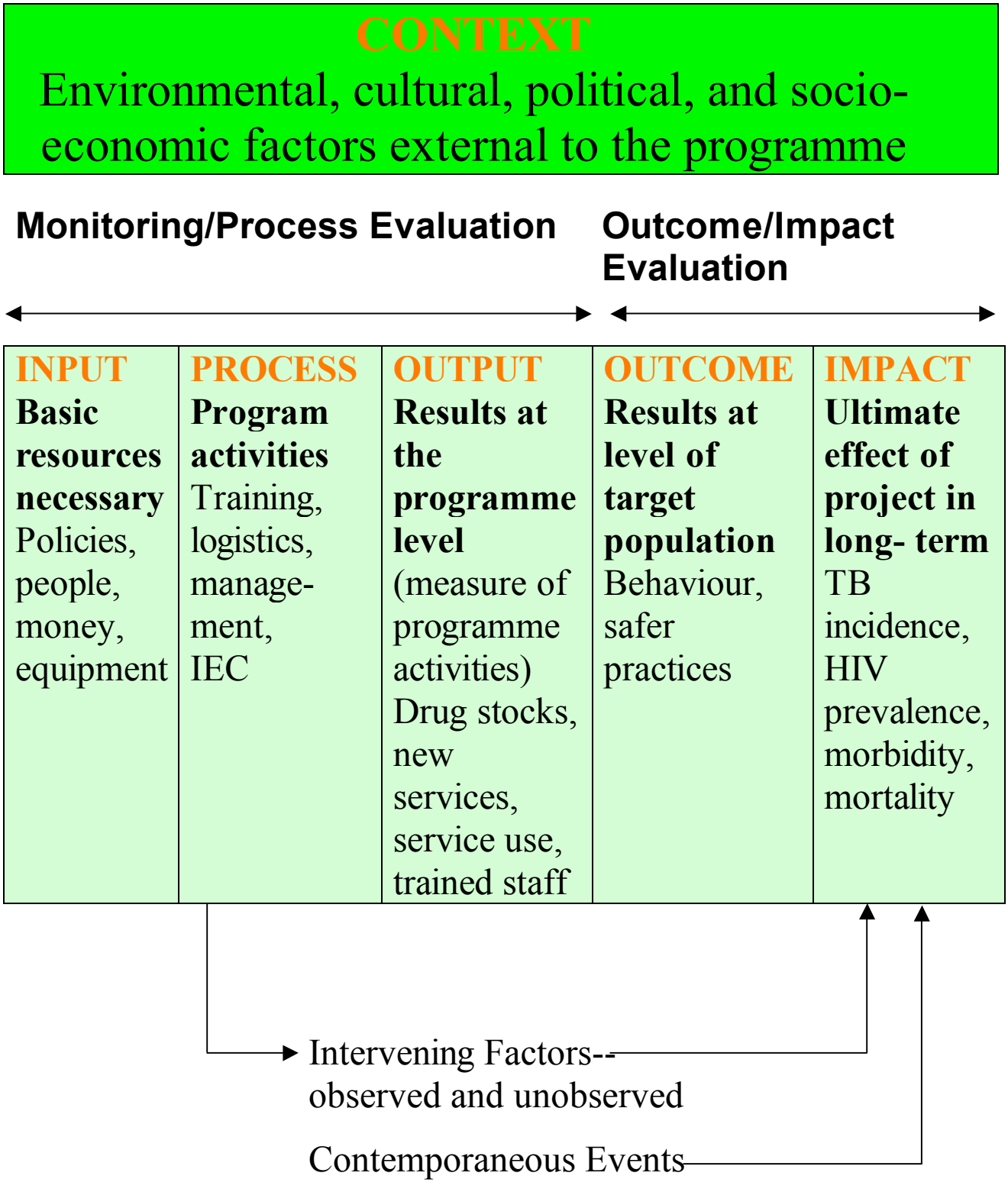
- on groups that have not (yet) received the treatment.

Requires extrapolation – is the effect of treatment the same

- of policies that have not yet been implemented

Requires a 'structural' approach

M&E Framework



Task of “netting out” the effect of the interventions from other factors is facilitated if control group(s) are introduced

Defining these control group(s) correctly is a key to identifying what would have occurred in the absence of the intervention

Basic organizing principle for any good impact evaluation of an intervention is to ask the question: what would have happened in the absence of the intervention?

This involves counterfactual analysis

What can an IE estimate? What are the parameters of interest? Different evaluation parameters are an average over parts of the distribution of impacts

Average Treatment Effect (ATE) = estimated effect of treatment for someone randomly chosen to be assigned to treatment (averages over the entire distribution)

Effect of Treatment on the Treated (TT) = estimated effect of treatment for someone chosen randomly from those who received treatment (averages over the distribution of impacts for those who are somehow allocated into treatment)

Local Average Treatment Effect (LATE) = estimated effect of treatment for those whose treatment status is changed because of some instrument (averages over the distribution of impacts for those who switch into treatment as a result of a reform or more precisely, as a result of a change of the value of some instrument affecting decisions to participate.)

Potential Outcomes Framework

Matrix of possible outcomes:

	Outcome if treatment is NOT received (Y_0)	*Outcome if treatment IS received (Y_1)*
Individuals who do not receive treatment (D=0)	$E[y_{i0}\|D=0]$	$E[y_{i1}\|D=0]$
Individuals who receive treatment (D=1)	$E[y_{i0}\|D=1]$	$E[y_{i1}\|D=1]$

For example, let

y_{0i} = health status of individual i if he is not administered the drug under study

y_{1i} = health status if he is administered the drug.

Treatment effect for individual i =

$$y_{1i} - y_{0i} = \beta_i$$

$$ATE = \frac{1}{N}\sum_{i=1}^{N}(y_{1i} - y_{0i})$$

N = size of sample (N large)

Problem: we cannot observe both y_{1i} and y_{0i} for each individual

Effect of Treatment on the Treated

$$\alpha = E[y_{i1} - y_{i0} | D_i = 1]$$
$$= E[y_{i1} | D_i = 1] - E[y_{i0} | D_i = 1]$$

Comparison of Treatment and Control

$$E[y_{i1} | D_i = 1] - E[y_{i0} | D_i = 0]$$
$$= \alpha + \{E[y_{i0} | D_i = 0] - E[y_{i0} | D_i = 1]\} = = \alpha + \text{Selection Bias}$$

Reasons: Self selection/Targeting; Related to observables/ unobservables

Sources of Selection Bias

1. *Self-Selection*

   - Individuals may be given the choice whether to participate

   - Individuals with the most to gain may be the most likely to join

   - More motivated individuals also may be the more likely to join

Leads to positive selection bias - impacts will be overstated

Sources of Selection Bias

2. *Targeting*

- Particular individuals may be forced or encourage to participate

- Targeting is often directed at individuals likely to have poor outcomes (e.g. the long-term unemployed)

Often leads to negative selection bias - impacts will be understated

Sources of Selection Bias

3. *Observables vs. Unobservables*

- Selection may be related mainly to observable characteristics (education or poverty status)

- Or unobserved characteristics such as motivation might be equally important or more important

Confoundedness & Heterogeneous Treatment Effects

For causal inference, we require that potential outcomes (y_s) are independent of treatment (D)

$y_s \perp D$ $s = 0,1$ (control and treatment)

Violations:

1. Confoundedness

$y_0 \perp D$: *non-treatment* outcomes are different

2. Heterogeneous treatment effects

$(y_1 - y_0)$ not $\perp D$: the *effect of treatment* is different

Knowing the characteristics of treatment and non-treatment groups helps, but . . .

We still require 'conditional' independence for causal inference

Conditional Independence Assumption (CIA):

Potential outcomes (y_s) independent of treatment (D), conditional on X

$$y_s \perp D \,|X \qquad s = 0,1$$

If CIA holds, we might still have problems

1. (conditional) Confoundedness (on unobservables)
2. (conditional) Heterogeneous treatment effects

Potential Outcomes

- Main identification problem is the lack of a counterfactual
- Causal inference requires independence of outcomes and assignment (possibly conditional on observables)
- Violations of CIA:

  1. Confoundedness: Different non-treatment outcomes

  2. Heterogeneous TE: Different impacts of treatment

Type of Method used to generate counterfactual determines IE design

Type of method:

1. Experimental design
2. Quasi-experimental design
3. Non-Experimental design

Experimental design (RCTs)

Treatment and comparison groups are selected randomly

If done correctly, the counterfactual for the treatment group will be same as outcomes for the control group

$$E[y_{i0} \mid D_i = 0] = E[y_{i0} \mid D_i = 1]$$

Thus, a simple comparison of mean outcomes will be unbiased

'Gold standard' for causal analysis

What Can We Learn If There Is Non-Compliance?

1. Intention-to-Treat (ITT)

The effect of being assigned to the treatment group = difference in mean outcomes between the treatment and control groups

Can be a useful measure depending on the treatment

2. Effect of Treatment on the Treated (TT), Adjusted for Dropout Bias

ITT / proportion of dropouts in the treatment group
Assumes: i) no substitution into the treatment group, ii) dropouts are unaffected by assignment to the treatment group, iii) dropouts have the same outcomes as controls who would drop out

3. Instrumental Variables Estimate of TT (treatment on treated)
Assumes: being assigned to the treatment (control) group does not affect outcomes for dropouts.

Problems with RCTs

- Not always feasible; likely to work better when the trial lasts for a relatively short period of time

- Social Experiments Are Rarely Double Blind
  - Individuals know what group they are in
  - Individuals in the 'bad' group may drop out of the experiment (dropout bias)
  - Individuals in the control group may seek treatment elsewhere (substitution bias)
  - Individuals may try to influence the results of the experiment by changing their behaviour (Hawthorne effect)

Quasi-experimental design

These approaches can remove bias arising from selection on observables and where panel data are available, time invariant unobservables.

Quasi-experimental methods include matching, differencing and instrumental variables, and are usually carried out by multivariate regression analysis

Approach dominated by knowledge of the assignment process

- Regression discontinuity design (RDD)

Approaches dominated by self-selection

- Matching

  case matching/ regression matching/propensity score matching

- Difference-in-difference (DiD)

- Instrumental variables (IV)

Quasi-experimental methods

RDD:

Assignment based on some pre-treatment variable x, with a cutoff value C

Treatments applies for all units i for which $x_i < C$, and not for units for which $x_i > C$.

Ex: Medical experiment where risky new treatment only given to patients in very bad condition.

Estimate treatment effect for units with x in the neighborhood of C

Key assumption: the regression function - the average value of the outcome y, given x and the treatment—is a continuous function of x near the cutoff value C.

Quasi-experimental methods

- Naïve estimator

  $\alpha_n = E[y_{i1} \mid D_i = 1] - E[y_{i0} \mid D_i = 0]$

- Matching on observables

  $\alpha_m = E[y_{i1} \mid X_i , D_i = 1] - E[y_{i0} \mid X_i , D_i = 0]$

- Regression (matching)

  $\alpha_R = E[y_{i1} \mid \beta X_i , D_i = 1] - E[y_{i0} \mid \beta X_i , D_i = 0]$

- Propensity score matching

  $\alpha_P = E[y_{i1} \mid P(X)_i , D_i = 1] - E[y_{i0} \mid P(X)_i , D_i = 0]$

- Instrumental variables

$$\alpha_{IV} = \frac{E\left[y_i \middle| Z_i = 1\right] - E\left[y_i \middle| Z_i = 0\right]}{E\left[D_i \middle| Z_i = 1\right] - E\left[D_i \middle| Z_i = 0\right]}$$

Difference-in-difference

- Use multiple observations on the same people (panel data)
- Allows us to 'net out' different (pre-treatment) levels of Y
- Allows for time period effects

	Without treatment	**Treated**
Treatment group	*Observed in t=0*	*Observed in t=1*
Untreated group	*Observed twice*	*missing*

$$\alpha_r = E[\Delta y_{1i} \mid D_i = 1] - E[[\Delta y_{0i} \mid D_i = 0]$$

Non-experimental design

Evaluations that do not involve a comparison group

Only feasible IE design when we have universally-implemented programmes or national policy reforms in which no isolated comparison groups are likely to exist

Methods used compare intervention groups before and after implementation of the intervention.

Challenge: to show a causal relationship between intervention and outcomes convincingly, the evaluation must demonstrate that any likely alternate explanations for the outcomes are irrelevant.

The Foundations of 'Good' Causal Analysis

Know the Program/Policy Well

- Know its objectives
- Know the rules and incentives for administrators and participants
- Know any quirks (ie sources of exogenous treatment)

Find Good Data

- Data for treated and control should come from the same source and directly measure the outcomes of interest
- Measure as many confounding variables and those related to assignment
- Ideally, data will be available both pre-and post- treatment

Find Exogenous Variation in the Likelihood of Treatment (perhaps, conditional on covariates)

-Programme quirks and/or assignment rules often create exogenous variation

Use Econometric Techniques Wisely

- Econometrics is useless without the first two criteria being met and still dicey w/o the third