REPUBLIC OF SOUTH AFRICA
GOVERNMENT-WIDE MONITORING & IMPACT EVALUATION SEMINAR

IMPACT EVALUATION: INSTRUMENTAL VARIABLE METHOD

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The main objective of Impact Evaluation…….

- To estimate the treatment effect of an intervention *T* on an outcome *Y*
- For example:
  - What is the effect of an increase in the minimum wage on employment?
  - What is the effect of a school feeding program on learning outcomes?
  - What is the effect of a training program on employment and earnings?
  - What is the effect of micro-finance participation on consumption and employment?

The Counterfactual

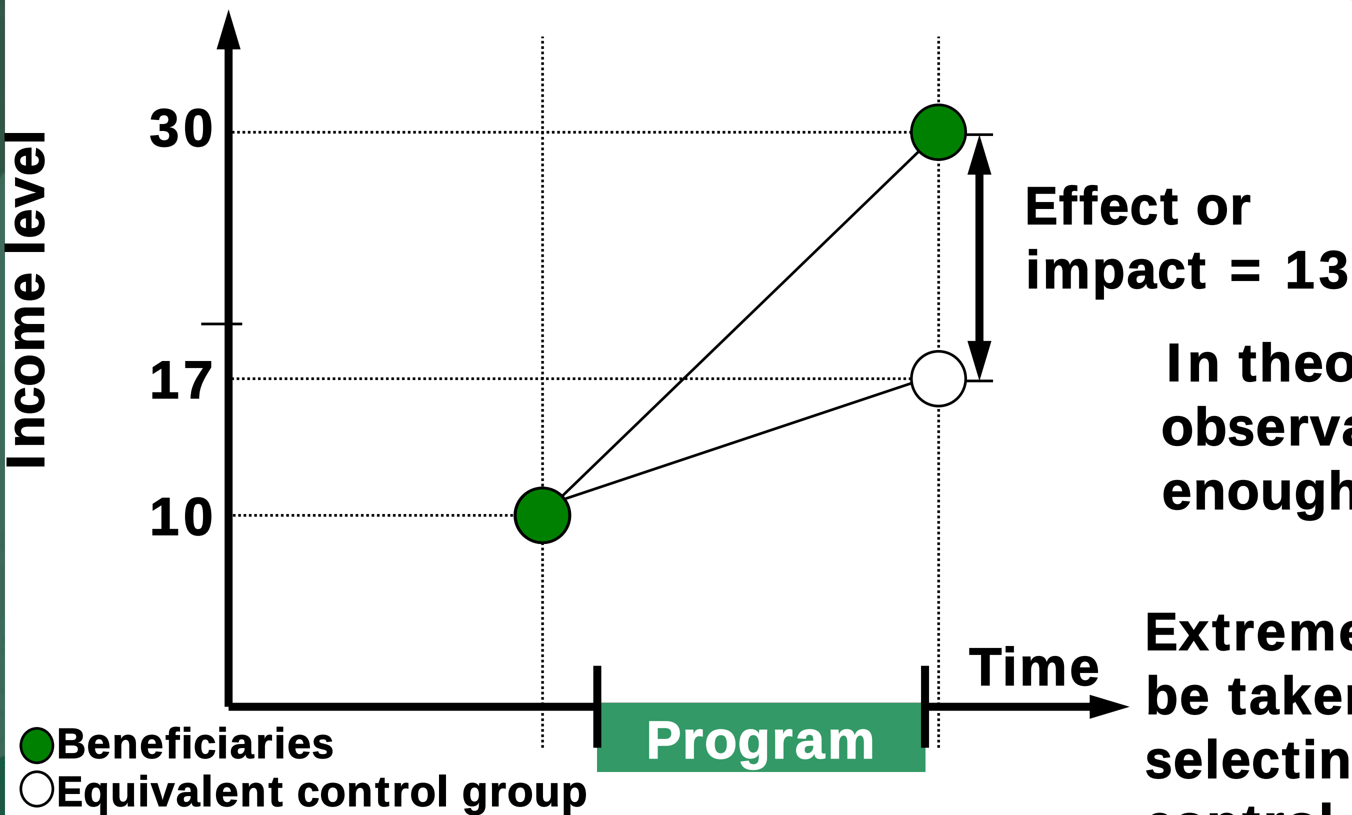
Recall the evaluator's key question is:

What would have happened to the beneficiaries if the program had not existed?

How do you rewrite history? How do you get baseline data?

The Ideal strategy

With equivalent control group.



In theory, a single observation is not enough

Extreme care must be taken when selecting the control group to ensure comparability.

Problem with ideal strategy is:

It is extremely difficult to assemble an exactly comparable control group:

- Ethical problem (to condemn a group to not being beneficiaries)
- Difficulties associated with finding an equivalent group outside the project
- Costs
- People change behaviour over time

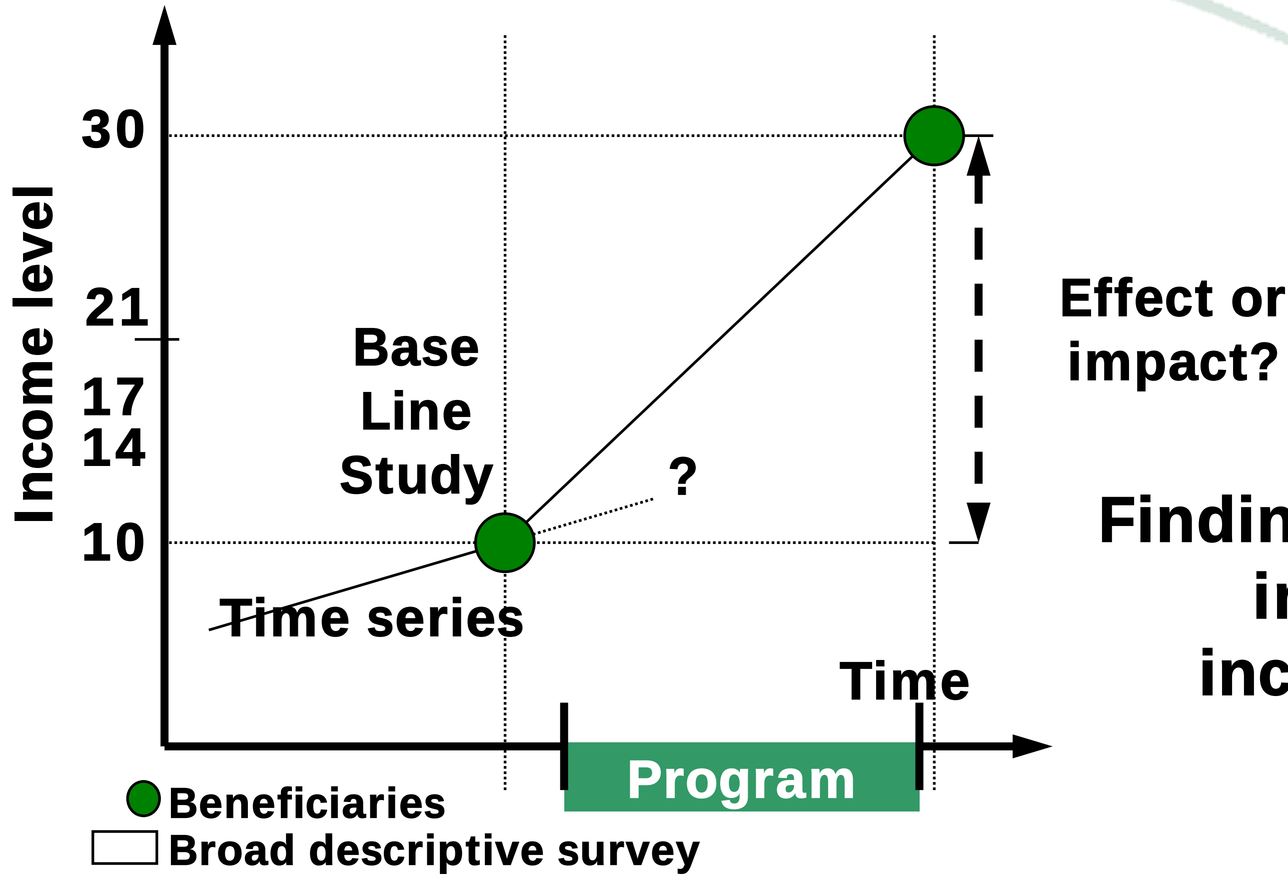
<u>Therefore, this approach is practically difficult</u>

Examples of *less than ideal* strategies…

- Comparing individuals before and after the intervention
- Comparing individuals that participated in the intervention to those that did not

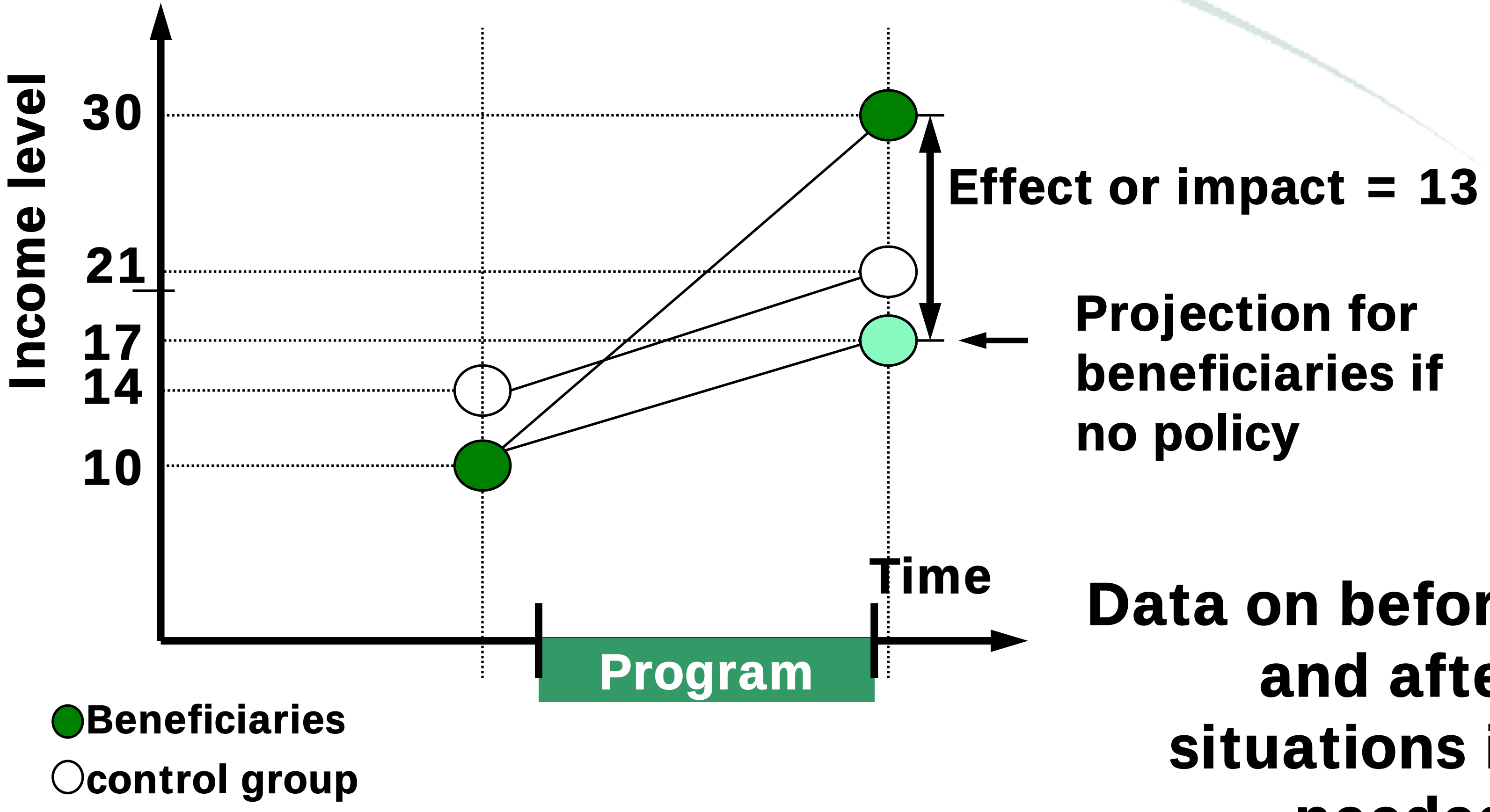
- Example of a micro-credit program
  - Often targeted – not everyone is eligible
  - Some individuals chose to participate and some don’t even among
    the eligible households/individuals
  - Why can’t we estimate the program’s impact by comparing outcomes of participants to non-participants?

Example with less than ideal strategies such as using a before/after comparison



Findings on the impact are inconclusive

Comparison with non-equivalent control group



Data on before and after situations is needed.

Micro-credit program

- Participation is a choice variable ➔ endogenous
- Observed and unobserved characteristics about participants that make them different from those who did not participate
  - Omitted variables – can control for many observed characteristics, but are still missing things that are hard or impossible to measure
  - Our treatment effect is “picking up” the effect of other characteristics that explain treatment, resulting in a biased estimate of the treatment effect

Micro-credit program

- Estimation: compare outcomes of individuals that chose to participate to those who did not:

$$y = \alpha + \beta_1 T + \beta_2 x + \varepsilon$$

Where T = 1 if enrolled in program

T = 0 if not enrolled in program

x = exogenous regressors (i.e. controls)

- The problem:
  - Corr (T, ε) ≠ 0

Micro-credit Program

- Examples of why Corr (T , ε) ≠ 0
  - Different motivation
  - Different ability
  - Different information
  - Different opportunity cost of participating
  - Different level of access

- There are characteristics of the treated that should be in ε, but are being "picked up" by T.
- We have violated one of the key assumptions of OLS: Independence of regressors x from disturbance term ε

What can we do about this problem?

- Try to "clean up" the correlation between T and ε:
- Isolate the variation in T that is uncorrelated with ε
- To do this, we need to find an instrumental variable (IV)
- OR, design programs with instrumental variables in mind

Basic idea behind IV

- Find a variable *Z* which satisfies two conditions:
  1. Correlated with *T*: corr $(Z, T) \neq 0$
  2. Uncorrelated with *ε*: corr $(Z, \varepsilon) = 0$

- Examples of Z in the micro-credit program example?
  – interest rate or the price of credit

Two Stage Least Squares (2SLS)

$$y = \alpha + \beta_1 T + \beta_2 x + \varepsilon$$

- Stage 1: Regress endogenous variable on the IV (*Z*) and other exogenous regressors

$$T = \delta_0 + \delta_1 x + \theta_1 Z + \tau$$

  – Calculate predicted value for each observation: *T hat*

Two stage Least Squares (2SLS)

- Stage 2: Regress outcome y on predicted variable (and other exogenous variables)

$$y = \alpha + \beta_1(\hat{T}) + \beta_2 x + \varepsilon$$

  - Need to correct Standard Errors (they are based on *T hat* rather than *T*)
- In practice just use STATA - ivreg
- Intuition: *T* has been "cleaned" of its correlation with *ε.*

Two stage Least Squares (2SLS)

- Another way of presenting the same resul
- β_1= Cov(y,z|X)/Cov(T,z)
- Cov(y,z) is called “The Reduced Form”
- Cov(T,z) is called “The First Stage”

Example: Grameen Bank impact

- C is credit demand;
- Y is outcome variable (consumption, assets, employment, schooling, family planning);
- Subscripts: household i; village j; male m; female f; time t;
- Want to allow separate effects on outcome of male and female credit;
- X represents individual or household characteristics.

Measuring Impacts: Cross-Sectional data

Credit demand equations:

$$C_{ijm} = X_{ijm}\beta_c + Z_{ijm}\gamma_c + \mu^c{}_{jm} + \varepsilon^c{}_{ijm}$$

$$C_{ijf} = X_{ijf}\beta_c + Z_{ijf}\gamma_c + \mu^c{}_{jf} + \varepsilon^c{}_{ijf}$$

Outcome equation:

$$Y_{ij} = X_{ij}\beta_y + C_{ijm}\delta_m + C_{ijf}\delta_f + \mu^y{}_j + \varepsilon^y{}_{ij}$$

Note: the *Z* represent variables assumed to affect credit demand but to have no *direct* effect on the outcome.

Endogeneity Issues

- Correlation among μ^c_{jm}, μ^c_{jf} and μ^y_j, and among ε^c_{ijm}, ε^c_{ijf} and ε^y_{ij}
- Estimation that ignores these correlations have endogeneity bias.
- Endogeneity arises from three sources:

  1) Non-random placement of credit programs;

  2) Unmeasured village attributes that affect both program credit demand and household outcomes;

  3) Unmeasured household attributes that affect both program credit demand and household outcomes.

Resolving endogeneity

- Village-level endogeneity – resolved by village FE;
- Household-level endogeneity – resolved by instrumental variables (IV);
- In credit demand equation Z_{ijm} and Z_{ijf} represent instrumental variables;
- Selecting Z_{ij} variables is difficult;

Possible solution: identification using quasi-experimental survey design.

Quasi-experimental survey design

- Households are sampled from program and non-program villages;
- Both eligible and non-eligible households are sampled from both types of villages;
- Both participants and non-participants are sampled from eligible households.

Identification conditions:

- Exogenous land-holding;
- Gender-based program design.

Instruments for identification

- **Exogenous land-holding criteria:**

  - Only households owning up to 0.5 acre of land qualify for program participation. In practice there is some deviation from this cutoff.
- **Gender-based program participation criteria:**

  - Male members of qualifying households cannot participate in program if village does not have a male program group.

  - Female members of qualifying households cannot participate in program if village does not have a female program group.

Construction of Zij variables

Male choice=1 if household has up to 0.5 acre of land and village has male credit group

=0 otherwise

Female choice=1 if household has up to 0.5 acre of land and village has female credit group

=0 otherwise

Male and female choice variables are interacted with household characteristics to form Z_{ij} Variables.

What this means…..

Intuitively, the outcome regression now relates variation in Y to variation in C associated with variation in Z.

Table 1: Weighted Mean and Standard Deviations of Dependent Variables

Dependent Variables	Partici-pants	Obs.	Non-partici-pants	Obs.	Total	Obs.	Non-program areas	Obs.	Aggregate	Obs.
Sum of program loans of females (Taka)	5498.854 (7229.351)	779		326	2604.454 (5682.398)	1105-	-		2604.454 (5682.398)	1105
Sum of program loans by males (Taka)	3691.993 (7081.581)	631	-	263	1729.631 (5184.668)	894	-	-	1729.631 (5184.668)	895
Per capita HH weekly food expenditure (Taka)	59.166 (19.865)	2696	62.265 (23.256)	1650	61.242 (22.239)	4326	61.985 (23.897)	872	61.366 (22.522)	5218
Per capita HH weekly non-food expenditure (Taka)	17.848 (31.538)	2696	23.621 (54.791)	1650	21.716 (48.439)	4346	27.676 (51.409)	872	22.706 (48.990)	5218
Per capita HH total weekly expenditure (Taka)	**77.014 (41.496)**	**2696**	**85.886 (64.820)**	**1650**	**82.959 (58.309)**	**4346**	**89.661 (66.823)**	**872**	**84.072 (59.851)**	**5218**

Tables 2: Alternate Estimates of the Impact of Credit on Per Capita Expenditure

Explanatory Variables	Total			Food			Non-food		
	Naive weighted (OLS)	Log Total Expenditure per Capita		Naive weighted (OLS)	Log Total Expenditure per Capita		Naive weighted (OLS)	Log Total Expenditure per Capita	
		IV	IV-FE		IV	IV-FE		IV	IV-FE
Amount borrowed by females from GB	.004 (1.765)	.0317 (2.174)	.0432 (4.249)	.005 (2.700)	.0114 (1.435)	.0114 (1.263)	.009 (1.760)	-.0184 (-.759)	.0199 (.467)
Amount borrowed by males from GB	.001 (.325)	-.0225 (-2.291)	.0179 (1.431)	-.001 (-.471)	-.0142 (-1.602)	.0087 (1.163)	.004 (.476)	-.0220 (-.982)	.0182 (.665)

Alternative Models for Estimation

(--) Ordinary Least Squares (OLS) estimation of outcome equation with no correction for weights;

(-) OLS on outcome equation with correction for sampling weights;

(+) IV (2-stage) estimation of outcome equation with weights correction;

(++) IV, weights, village fixed effects.

Table 3. Comparing Results among Alternative Models: Log-log impacts of GB women's credit on HH per capita consumption

Models	Coefficient (t-stat)
Un-weighted OLS	0.003 (1.400)
Weighted OLS	0.004 (1.765)
Weighted IV	0.0371 (2.174)
Weighted IV, village FE	0.0432 (4.249)

Conclusions

- Participants are observed to consume less on average than counterpart non-participants – does this mean program does not matter?
- Econometric estimation shows participation has indeed caused some 18 percent return to consumption for female and 11 percent for male borrowing
- IV-FE > IV > OLS

IV's in Program Evaluation

- Very hard to find appropriate instrumental variables *ex post*
- Can *build* IVs into design of program

- Two Cases:
  - Problematic Randomization
  - Randomization not feasible despite best attempts

IV's in Program Evaluation

- Problematic Randomization
- Two Groups: Treatment (T=1) and Control (T = 0)
- Noticed
  - Some people in T=1 *did not get the treatment!*
  - Some people in T=0 *did get the treatment*
- So, people who received the treatment (RT for Received Treatment) is different from people chosen: RT ≠ T
- Don't throw up hands in despair!
  - T is a valid IV for RT

IV's in Program Evaluation

- Randomization not feasible
- Working with NGO to evaluate new program
- Suggest quasi-experimental program design
- NGO to require to follow eligibility criteria based on observables such as landholding, gender, poverty, etc
- Construct two-groups: Z=1 and Z=0
- Let NGO choose treated households
  - BUT ensure more treatment samples in Z=1 than Z=0
- Use Z as instrumental variable

Recall classic assumption of OLS

- Assuming we can find valid IVs, we can overcome endogeneity (X is a choice variable correlated with the error):
  - Simulteneity: X and Y cause each other
  - Omitted Variables: X is picking up the effect of other variables
  - Measurement Error: X is not measured precisely

Caution….

- Bad instruments: if corr (Z , ε) ≠ 0, we are in trouble!
  - we must ensure that corr (Z , ε) = 0
  - this is not always easy! We use theory and common sense