

GOV 2001 / 1002 / E-2001 Section 12

Research Designs and Multiple Equation Models

Solé Prillaman

Harvard University

April 23, 2014

LOGISTICS

Reading Assignment- UPM Ch. 8, King et al 2001, Honaker and King 2010 (Suggested)

Last Section- Wednesday April 30, only at 6pm. Open Q& A.

Last Office Hours-	Stephen	Friday 2-4pm
	Solé	Monday 10am-12pm

REPLICATION PAPER

- ▶ Due May 1 on Canvas. (Optional extension until May 5 at 5:00pm)
- ▶ Upload your paper and an anonymized version of your paper.
- ▶ Upload all of your files to the class dataverse (paper, code, data)
- ▶ You will then receive **four** other papers to read, rank, and comment on
- ▶ Your comments will be due on May 12

FINAL EXAM

- ▶ Take-home exam through Canvas
- ▶ Available starting on May 1
- ▶ Due May 8 on Canvas
- ▶ ONLY for extension school students

QUIZ BREAK!

Why can we say that $L(\theta) \propto \prod_{i=1}^n p(y_i|\theta)$?

OUTLINE

Research Design and Causal Inference

Multiple Equation Models

Missing Data: A Preview

CAUSAL EFFECTS

Causal Effect:

$$Y_i(1) - Y_i(0)$$

Average treatment effect (ATE):

$$E[Y(1) - Y(0)] = E[Y(1)] - E[Y(0)]$$

Average treatment effect on the treated (ATT):

$$E[Y(1|T=1) - Y(0|T=1)] = E[Y_t(1) - Y_t(0)]$$

CAUSAL EFFECTS

Causal Effect:

$$Y_i(1) - Y_i(0)$$

Average treatment effect (ATE):

$$E[Y(1) - Y(0)] = E[Y(1)] - E[Y(0)]$$

Average treatment effect on the treated (ATT):

$$E[Y(1|T=1) - Y(0|T=1)] = E[Y_t(1) - Y_t(0)]$$

We can't estimate the ATE and ATT because of **unobserved** potential outcomes.

FUNDAMENTAL PROBLEM OF CAUSAL INFERENCE

In an ideal world, we would see this:

$Unit_i$	X_i^1	X_i^2	X_i^3	T_i	$Y_i(0)$	$Y_i(1)$	$Y_i(1) - Y_i(0)$
1	2	1	50	0	69	75	6
2	3	1	98	0	111	108	-3
3	2	2	80	1	92	102	10
4	3	1	98	1	112	111	-1

FUNDAMENTAL PROBLEM OF CAUSAL INFERENCE

But in the real world, we see this:

$Unit_i$	X_i^1	X_i^2	X_i^3	T_i	$Y_i(0)$	$Y_i(1)$	$Y_i(1) - Y_i(0)$
1	2	1	50	0	69	?	?
2	3	1	98	0	111	?	?
3	2	2	80	1	?	102	?
4	3	1	98	1	?	111	?

FUNDAMENTAL PROBLEM OF CAUSAL INFERENCE

- ▶ For control units, $Y_i(1)$ is the **counterfactual**.
- ▶ For treatment units, $Y_i(0)$ is the **counterfactual**.

FUNDAMENTAL PROBLEM OF CAUSAL INFERENCE

- ▶ For control units, $Y_i(1)$ is the **counterfactual**.
- ▶ For treatment units, $Y_i(0)$ is the **counterfactual**.

Our goal: Find a way to estimate this counterfactual.

DECOMPOSITION OF ESTIMATION ERROR

- Difference in means: $\hat{ATE} = E[Y|D = 1] - E[Y|D = 0]$

DECOMPOSITION OF ESTIMATION ERROR

- ▶ Difference in means: $\hat{ATE} = E[Y|D = 1] - E[Y|D = 0]$
- ▶ Estimation Error:

$$\Delta \equiv \text{PATE} - \hat{ATE}$$

DECOMPOSITION OF ESTIMATION ERROR

- ▶ Difference in means: $\hat{ATE} = E[Y|D = 1] - E[Y|D = 0]$
- ▶ Estimation Error:

$$\begin{aligned}\Delta &\equiv \text{PATE} - \hat{ATE} \\ &= \text{PATE} - \text{SATE} + \text{SATE} - \hat{ATE}\end{aligned}$$

DECOMPOSITION OF ESTIMATION ERROR

- ▶ Difference in means: $\hat{ATE} = E[Y|D = 1] - E[Y|D = 0]$
- ▶ Estimation Error:

$$\begin{aligned}\Delta &\equiv \text{PATE} - \hat{ATE} \\ &= \text{PATE} - \text{SATE} + \text{SATE} - \hat{ATE} \\ &= \Delta_S + \Delta_T\end{aligned}$$

DECOMPOSITION OF ESTIMATION ERROR

- ▶ Difference in means: $\hat{ATE} = E[Y|D = 1] - E[Y|D = 0]$
- ▶ Estimation Error:

$$\begin{aligned}\Delta &\equiv \text{PATE} - \hat{ATE} \\ &= \text{PATE} - \text{SATE} + \text{SATE} - \hat{ATE} \\ &= \Delta_S + \Delta_T \\ &= (\Delta_{S_X} + \Delta_{S_U}) + (\Delta_{T_X} + \Delta_{T_U})\end{aligned}$$

- ▶ Error due to Δ_S (sample selection), Δ_T (treatment imbalance), and each due to observed (X_i) and unobserved (U_i) covariates

EFFECTS OF DESIGN COMPONENTS ON ESTIMATION ERROR

Design Choice

Δ_{S_X}

Δ_{S_U}

Δ_{T_X}

Δ_{T_U}

EFFECTS OF DESIGN COMPONENTS ON ESTIMATION ERROR

Design Choice

Δ_{S_X}

Δ_{S_U}

Δ_{T_X}

Δ_{T_U}

EFFECTS OF DESIGN COMPONENTS ON ESTIMATION ERROR

Design Choice

Random sampling

Δ_{S_X}

Δ_{S_U}

Δ_{T_X}

Δ_{T_U}

EFFECTS OF DESIGN COMPONENTS ON ESTIMATION ERROR

Design Choice

Random sampling

$$\begin{array}{cccc} \Delta_{S_X} & \Delta_{S_U} & \Delta_{T_X} & \Delta_{T_U} \\ \text{avg} & \text{avg} & & \\ \underline{=} 0 & \underline{=} 0 & & \end{array}$$

EFFECTS OF DESIGN COMPONENTS ON ESTIMATION ERROR

Design Choice

Random sampling

Stratified Random Sampling

$$\begin{array}{cccc} \Delta_{S_X} & \Delta_{S_U} & \Delta_{T_X} & \Delta_{T_U} \\ \text{avg} & \text{avg} & & \\ \underline{= 0} & \underline{= 0} & & \end{array}$$

EFFECTS OF DESIGN COMPONENTS ON ESTIMATION ERROR

Design Choice

Random sampling

Stratified Random Sampling

Δ_{S_X}	Δ_{S_U}	Δ_{T_X}	Δ_{T_U}
$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$		
$= 0$	$\stackrel{\text{avg}}{=} 0$		

EFFECTS OF DESIGN COMPONENTS ON ESTIMATION ERROR

Design Choice

Random sampling

Stratified Random Sampling

Focus on SATE rather than PATE

Δ_{S_X}	Δ_{S_U}	Δ_{T_X}	Δ_{T_U}
$\underset{\text{avg}}{=} 0$	$\underset{\text{avg}}{=} 0$		
$= 0$	$\underset{\text{avg}}{=} 0$		

EFFECTS OF DESIGN COMPONENTS ON ESTIMATION ERROR

Design Choice

	Δ_{S_X}	Δ_{S_U}	Δ_{T_X}	Δ_{T_U}
Random sampling	$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$		
Stratified Random Sampling	$= 0$	$\stackrel{\text{avg}}{=} 0$		
Focus on SATE rather than PATE	$= 0$	$= 0$		

EFFECTS OF DESIGN COMPONENTS ON ESTIMATION ERROR

Design Choice

	Δ_{S_X}	Δ_{S_U}	Δ_{T_X}	Δ_{T_U}
Random sampling	$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$		
Stratified Random Sampling	$= 0$	$\stackrel{\text{avg}}{=} 0$		
Focus on SATE rather than PATE	$= 0$	$= 0$		
Weighting for nonrandom sampling				

EFFECTS OF DESIGN COMPONENTS ON ESTIMATION ERROR

Design Choice

	Δ_{S_X}	Δ_{S_U}	Δ_{T_X}	Δ_{T_U}
Random sampling	$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$		
Stratified Random Sampling	$= 0$	$\stackrel{\text{avg}}{=} 0$		
Focus on SATE rather than PATE	$= 0$	$= 0$		
Weighting for nonrandom sampling	$= 0$	$= ?$		

EFFECTS OF DESIGN COMPONENTS ON ESTIMATION ERROR

Design Choice

	Δ_{S_X}	Δ_{S_U}	Δ_{T_X}	Δ_{T_U}
Random sampling	$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$		
Stratified Random Sampling	$= 0$	$\stackrel{\text{avg}}{=} 0$		
Focus on SATE rather than PATE	$= 0$	$= 0$		
Weighting for nonrandom sampling	$= 0$	$= ?$		
Large sample size				

EFFECTS OF DESIGN COMPONENTS ON ESTIMATION ERROR

Design Choice

	Δ_{S_X}	Δ_{S_U}	Δ_{T_X}	Δ_{T_U}
Random sampling	$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$		
Stratified Random Sampling	$= 0$	$\stackrel{\text{avg}}{=} 0$		
Focus on SATE rather than PATE	$= 0$	$= 0$		
Weighting for nonrandom sampling	$= 0$	$= ?$		
Large sample size	$\rightarrow ?$	$\rightarrow ?$	$\rightarrow ?$	$\rightarrow ?$

EFFECTS OF DESIGN COMPONENTS ON ESTIMATION ERROR

Design Choice

	Δ_{S_X}	Δ_{S_U}	Δ_{T_X}	Δ_{T_U}
Random sampling	$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$		
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Focus on SATE rather than PATE	$= 0$	$= 0$		
Weighting for nonrandom sampling	$= 0$	$= ?$		
Large sample size	$\rightarrow ?$	$\rightarrow ?$	$\rightarrow ?$	$\rightarrow ?$
Random treatment assignment				

EFFECTS OF DESIGN COMPONENTS ON ESTIMATION ERROR

Design Choice

	Δ_{S_X}	Δ_{S_U}	Δ_{T_X}	Δ_{T_U}
Random sampling	$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$		
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Focus on SATE rather than PATE	$= 0$	$= 0$		
Weighting for nonrandom sampling	$= 0$	$= ?$		
Large sample size	$\rightarrow ?$	$\rightarrow ?$	$\rightarrow ?$	$\rightarrow ?$
Random treatment assignment			$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$

EFFECTS OF DESIGN COMPONENTS ON ESTIMATION ERROR

Design Choice

	Δ_{S_X}	Δ_{S_U}	Δ_{T_X}	Δ_{T_U}
Random sampling	$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$		
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Focus on SATE rather than PATE	$= 0$	$= 0$		
Weighting for nonrandom sampling	$= 0$	$= ?$		
Large sample size	$\rightarrow ?$	$\rightarrow ?$	$\rightarrow ?$	$\rightarrow ?$
Random treatment assignment			$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$
Complete blocking				

EFFECTS OF DESIGN COMPONENTS ON ESTIMATION ERROR

Design Choice

	Δ_{S_X}	Δ_{S_U}	Δ_{T_X}	Δ_{T_U}
Random sampling	$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$		
Stratified Random Sampling	$= 0$	$\stackrel{\text{avg}}{=} 0$		
Focus on SATE rather than PATE	$= 0$	$= 0$		
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Large sample size	$\rightarrow ?$	$\rightarrow ?$	$\rightarrow ?$	$\rightarrow ?$
Random treatment assignment			$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$
Complete blocking			$= 0$	$= ?$

EFFECTS OF DESIGN COMPONENTS ON ESTIMATION ERROR

Design Choice

	Δ_{S_X}	Δ_{S_U}	Δ_{T_X}	Δ_{T_U}
Random sampling	$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$		
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Large sample size	$\rightarrow ?$	$\rightarrow ?$	$\rightarrow ?$	$\rightarrow ?$
Random treatment assignment			$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$
Complete blocking			$= 0$	$= ?$
Exact matching				

EFFECTS OF DESIGN COMPONENTS ON ESTIMATION ERROR

Design Choice

	Δ_{S_X}	Δ_{S_U}	Δ_{T_X}	Δ_{T_U}
Random sampling	$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$		
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Complete blocking			$= 0$	$= ?$
Exact matching			$= 0$	$= ?$

EFFECTS OF DESIGN COMPONENTS ON ESTIMATION ERROR

Design Choice

	Δ_{S_X}	Δ_{S_U}	Δ_{T_X}	Δ_{T_U}
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Large sample size	$\rightarrow ?$	$\rightarrow ?$	$\rightarrow ?$	$\rightarrow ?$
Random treatment assignment			$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$
Complete blocking			$= 0$	$= ?$
Exact matching			$= 0$	$= ?$

Assumption

EFFECTS OF DESIGN COMPONENTS ON ESTIMATION ERROR

Design Choice

	Δ_{S_X}	Δ_{S_U}	Δ_{T_X}	Δ_{T_U}
Random sampling	$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$		
Stratified Random Sampling	$= 0$	$\stackrel{\text{avg}}{=} 0$		
Focus on SATE rather than PATE	$= 0$	$= 0$		
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Large sample size	$\rightarrow ?$	$\rightarrow ?$	$\rightarrow ?$	$\rightarrow ?$
Random treatment assignment			$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$
Complete blocking			$= 0$	$= ?$
Exact matching			$= 0$	$= ?$

Assumption

No selection bias

EFFECTS OF DESIGN COMPONENTS ON ESTIMATION ERROR

Design Choice

	Δ_{S_X}	Δ_{S_U}	Δ_{T_X}	Δ_{T_U}
Random sampling	$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$		
Stratified Random Sampling	$= 0$	$\stackrel{\text{avg}}{=} 0$		
Focus on SATE rather than PATE	$= 0$	$= 0$		
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Large sample size	$\rightarrow ?$	$\rightarrow ?$	$\rightarrow ?$	$\rightarrow ?$
Random treatment assignment			$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$
Complete blocking			$= 0$	$= ?$
Exact matching			$= 0$	$= ?$

Assumption

No selection bias	$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$
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EFFECTS OF DESIGN COMPONENTS ON ESTIMATION ERROR

Design Choice

	Δ_{S_X}	Δ_{S_U}	Δ_{T_X}	Δ_{T_U}
Random sampling	$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$		
Stratified Random Sampling	$= 0$	$\stackrel{\text{avg}}{=} 0$		
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Weighting for nonrandom sampling	$= 0$	$= ?$		
Large sample size	$\rightarrow ?$	$\rightarrow ?$	$\rightarrow ?$	$\rightarrow ?$
Random treatment assignment			$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$
Complete blocking			$= 0$	$= ?$
Exact matching			$= 0$	$= ?$

Assumption

No selection bias	$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$
Ignorability		

EFFECTS OF DESIGN COMPONENTS ON ESTIMATION ERROR

Design Choice

	Δ_{S_X}	Δ_{S_U}	Δ_{T_X}	Δ_{T_U}
Random sampling	$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$		
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Weighting for nonrandom sampling	$= 0$	$= ?$		
Large sample size	$\rightarrow ?$	$\rightarrow ?$	$\rightarrow ?$	$\rightarrow ?$
Random treatment assignment			$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$
Complete blocking			$= 0$	$= ?$
Exact matching			$= 0$	$= ?$

Assumption

No selection bias	$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$		
Ignorability				$\stackrel{\text{avg}}{=} 0$

EFFECTS OF DESIGN COMPONENTS ON ESTIMATION ERROR

Design Choice

	Δ_{S_X}	Δ_{S_U}	Δ_{T_X}	Δ_{T_U}
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Focus on SATE rather than PATE	$= 0$	$= 0$		
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Large sample size	$\rightarrow ?$	$\rightarrow ?$	$\rightarrow ?$	$\rightarrow ?$
Random treatment assignment			$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$
Complete blocking			$= 0$	$= ?$
Exact matching			$= 0$	$= ?$

Assumption

No selection bias	$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$		
Ignorability				$\stackrel{\text{avg}}{=} 0$
No omitted variables				

EFFECTS OF DESIGN COMPONENTS ON ESTIMATION ERROR

Design Choice

	Δ_{S_X}	Δ_{S_U}	Δ_{T_X}	Δ_{T_U}
Random sampling	$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$		
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Focus on SATE rather than PATE	$= 0$	$= 0$		
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Large sample size	$\rightarrow ?$	$\rightarrow ?$	$\rightarrow ?$	$\rightarrow ?$
Random treatment assignment			$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$
Complete blocking			$= 0$	$= ?$
Exact matching			$= 0$	$= ?$

Assumption

No selection bias	$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$		
Ignorability				$\stackrel{\text{avg}}{=} 0$
No omitted variables				$= 0$

THE BENEFITS OF MAJOR RESEARCH DESIGNS

$$\Delta_{S_X} \quad \Delta_{S_U} \quad \Delta_{T_X} \quad \Delta_{T_U}$$

THE BENEFITS OF MAJOR RESEARCH DESIGNS

	Δ_{S_X}	Δ_{S_U}	Δ_{T_X}	Δ_{T_U}
Randomized clinical trials	$\neq 0$	$\neq 0$	$\stackrel{\text{avg}}{=} 0$	$\stackrel{\text{avg}}{=} 0$

THE BENEFITS OF MAJOR RESEARCH DESIGNS

	Δ_{S_X}	Δ_{S_U}	Δ_{T_X}	Δ_{T_U}
Randomized clinical trials	$\neq 0$	$\neq 0$	$\overset{\text{avg}}{=} 0$	$\overset{\text{avg}}{=} 0$
Social Science				
Field Experiment	$\neq 0$	$\neq 0$	$\rightarrow 0$	$\rightarrow 0$

THE BENEFITS OF MAJOR RESEARCH DESIGNS

	Δ_{S_X}	Δ_{S_U}	Δ_{T_X}	Δ_{T_U}
Randomized clinical trials	$\neq 0$	$\neq 0$	$\overset{\text{avg}}{=} 0$	$\overset{\text{avg}}{=} 0$
Social Science				
Field Experiment	$\neq 0$	$\neq 0$	$\rightarrow 0$	$\rightarrow 0$
Survey Experiment	$\rightarrow 0$	$\rightarrow 0$	$\rightarrow 0$	$\rightarrow 0$

THE BENEFITS OF MAJOR RESEARCH DESIGNS

	Δ_{S_X}	Δ_{S_U}	Δ_{T_X}	Δ_{T_U}
Randomized clinical trials	$\neq 0$	$\neq 0$	$\overset{\text{avg}}{=} 0$	$\overset{\text{avg}}{=} 0$
Social Science				
Field Experiment	$\neq 0$	$\neq 0$	$\rightarrow 0$	$\rightarrow 0$
Survey Experiment	$\rightarrow 0$	$\rightarrow 0$	$\rightarrow 0$	$\rightarrow 0$
Observational Study				
(Representative data set, Well-matched)	≈ 0	≈ 0	≈ 0	$\neq 0$

THE BENEFITS OF MAJOR RESEARCH DESIGNS

	Δ_{S_X}	Δ_{S_U}	Δ_{T_X}	Δ_{T_U}
Randomized clinical trials	$\neq 0$	$\neq 0$	$\overset{\text{avg}}{=} 0$	$\overset{\text{avg}}{=} 0$
Social Science				
Field Experiment	$\neq 0$	$\neq 0$	$\rightarrow 0$	$\rightarrow 0$
Survey Experiment	$\rightarrow 0$	$\rightarrow 0$	$\rightarrow 0$	$\rightarrow 0$
Observational Study (Representative data set, Well-matched)	≈ 0	≈ 0	≈ 0	$\neq 0$
Observational Study (Unrepresentative but partially, correctable data, well-matched)	≈ 0	$\neq 0$	≈ 0	$\neq 0$

THE BENEFITS OF MAJOR RESEARCH DESIGNS

	Δ_{S_X}	Δ_{S_U}	Δ_{T_X}	Δ_{T_U}
Randomized clinical trials	$\neq 0$	$\neq 0$	$\overset{\text{avg}}{=} 0$	$\overset{\text{avg}}{=} 0$
Social Science				
Field Experiment	$\neq 0$	$\neq 0$	$\rightarrow 0$	$\rightarrow 0$
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Observational Study (Representative data set, Well-matched)	≈ 0	≈ 0	≈ 0	$\neq 0$
Observational Study (Unrepresentative but partially, correctable data, well-matched)	≈ 0	$\neq 0$	≈ 0	$\neq 0$
Observational Study (Unrepresentative data set, Well-matched)	$\neq 0$	$\neq 0$	≈ 0	$\neq 0$

RESEARCH DESIGNS AND ESTIMATION STRATEGIES

So here are some possible solutions

1. Randomized or Experimental Studies
2. Observational studies
 - ▶ Matching
 - ▶ Instrumental Variables
 - ▶ Regression Discontinuity Design
 - ▶ Difference-in-Differences
 - ▶ Synthetic Controls

RESEARCH DESIGNS AND ESTIMATION STRATEGIES

Always think about:

- What is your assignment mechanism? How was treatment assigned?

RESEARCH DESIGNS AND ESTIMATION STRATEGIES

Always think about:

- ▶ What is your assignment mechanism? How was treatment assigned?
- ▶ How can we estimate a counterfactual?

RANDOMIZED EXPERIMENTS

The gold standard of scientific research.

- ▶ The simplest way (conceptually) to compare treated and control units.
- ▶ Units are randomly assigned to receive treatment and control.
- ▶ We still can't estimate individual-level causal effects, but we can estimate the population **average treatment effect**, $E[Y_i(1) - Y_i(0)]$.
- ▶ Principles of Experimentation: Replication, Randomization, Blocking

RANDOMIZED EXPERIMENTS - ASSUMPTIONS

1. **Ignorability:** There are no factors out there that affect both the probability of treatment and the outcome.
 - ▶ If the treatment was assigned in a truly random fashion, you are generally ok.
 - ▶ But if it wasn't, then you are in trouble!

RANDOMIZED EXPERIMENTS - ASSUMPTIONS

1. **Ignorability:** There are no factors out there that affect both the probability of treatment and the outcome.
 - ▶ If the treatment was assigned in a truly random fashion, you are generally ok.
 - ▶ But if it wasn't, then you are in trouble!
2. **SUTVA:** We have assumed that assigning treatment to one unit doesn't affect the outcome for another unit.
 - ▶ Not always reasonable if there are potential peer effects!

RANDOMIZED EXPERIMENTS - DEALING WITH NON-COMPLIANCE

What about non-compliance?

- ▶ Did the units actually “take” the treatment as assigned?

RANDOMIZED EXPERIMENTS - DEALING WITH NON-COMPLIANCE

What about non-compliance?

- ▶ Did the units actually “take” the treatment as assigned?
 - ▶ **Never-taker:** Unit never takes treatment

RANDOMIZED EXPERIMENTS - DEALING WITH NON-COMPLIANCE

What about non-compliance?

- ▶ Did the units actually “take” the treatment as assigned?
 - ▶ **Never-taker:** Unit never takes treatment
 - ▶ **Always-taker:** Unit always takes treatment

RANDOMIZED EXPERIMENTS - DEALING WITH NON-COMPLIANCE

What about non-compliance?

- ▶ Did the units actually “take” the treatment as assigned?
 - ▶ **Never-taker:** Unit never takes treatment
 - ▶ **Always-taker:** Unit always takes treatment
 - ▶ **Complier:** Unit takes the treatment only when assigned

RANDOMIZED EXPERIMENTS - DEALING WITH NON-COMPLIANCE

What about non-compliance?

- ▶ Did the units actually “take” the treatment as assigned?
 - ▶ **Never-taker:** Unit never takes treatment
 - ▶ **Always-taker:** Unit always takes treatment
 - ▶ **Complier:** Unit takes the treatment only when assigned
 - ▶ **Defier:** Unit takes treatment only when not assigned

RANDOMIZED EXPERIMENTS - DEALING WITH NON-COMPLIANCE

What about non-compliance?

- ▶ Did the units actually “take” the treatment as assigned?
 - ▶ **Never-taker:** Unit never takes treatment
 - ▶ **Always-taker:** Unit always takes treatment
 - ▶ **Complier:** Unit takes the treatment only when assigned
 - ▶ **Defier:** Unit takes treatment only when not assigned

RANDOMIZED EXPERIMENTS - DEALING WITH NON-COMPLIANCE

With non-compliance, what did you randomize?

RANDOMIZED EXPERIMENTS - DEALING WITH NON-COMPLIANCE

With non-compliance, what did you randomize? **Assignment to treatment, not treatment**

RANDOMIZED EXPERIMENTS - DEALING WITH NON-COMPLIANCE

With non-compliance, what did you randomize? **Assignment to treatment, not treatment**

Solutions:

RANDOMIZED EXPERIMENTS - DEALING WITH NON-COMPLIANCE

With non-compliance, what did you randomize? **Assignment to treatment, not treatment**

Solutions:

- Focus on “intent to treat” rather than on actual treatment.

RANDOMIZED EXPERIMENTS - DEALING WITH NON-COMPLIANCE

With non-compliance, what did you randomize? **Assignment to treatment, not treatment**

Solutions:

- ▶ Focus on “intent to treat” rather than on actual treatment.
- ▶ Use an Instrumental Variables approach with the intent to treat as an instrument.

RANDOMIZED EXPERIMENTS - DEALING WITH NON-COMPLIANCE

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Solutions:

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 - ▶ Only estimates the effect of treatment on **compliers!**

OBSERVATIONAL DATA

We only observe our data **after** the experiment occurred and we have no control over treatment assignment.

1. Gather dataset.
2. Estimate ATE or ATT with a *model*.

OBSERVATIONAL DATA - ASSUMPTIONS

1. **Ignorability:** Include covariates to get conditional ignorability. Goal: Treatment assignment is independent of the outcomes (Y) given covariates X (must know assignment mechanism for this).

$$(Y(1), Y(0)) \perp T | X$$

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2. **SUTVA:** simply assumed (a problematic assumption most of the time)

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- ▶ SUTVA assumption
- ▶ Model Dependence
 - ▶ We try to alleviate the curse of dimensionality and problem of continuous covariates by specifying a model.
 - ▶ Estimates of ATE or ATT may differ depending on the model you specify.

MATCHING TO AMELIORATE MODEL DEPENDENCE

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- ▶ Assume that **X** perfectly characterizes the assignment mechanism – no unmeasured confounders (omitted variables) and no unobserved confounders!
- ▶ And remember: Don't match on post-treatment variables!

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Example: Y = post-Vietnam War civilian mortality; T = serving in the military during Vietnam War; Z = draft lottery

INSTRUMENTAL VARIABLES - ASSUMPTIONS

1. **Ignorability** of the Instrument Z

Example: Assignment of draft status was random.

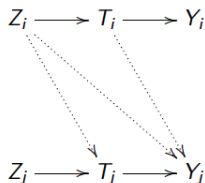
INSTRUMENTAL VARIABLES - ASSUMPTIONS

1. **Ignorability** of the Instrument Z

Example: Assignment of draft status was random.

2. **SUTVA**: Z_i does not affect T_j and Y_j and T_i does not affect Y_j for all $i \neq j$ (non-interference).

Figure : SUTVA Assumption implies absence of dotted arrows.

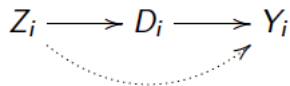


Example: The veteran status and civilian mortality of any man was not affected by the draft status of others.

INSTRUMENTAL VARIABLES - ASSUMPTIONS

3. **Exclusion Restriction:** Any effect of Z on Y must be via an effect of Z on T .

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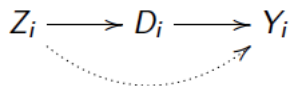


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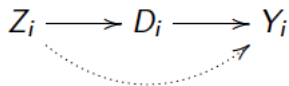
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Example: Having a low lottery number increases the average probability of service.

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5. **Monotonicity:** No Defiers

Example: There is no one who would have served if given a high lottery number, but not if given a low lottery number.

INSTRUMENTAL VARIABLES - ESTIMATION

If all the assumptions hold, then the **Local Average Treatment Effect (LATE)** of T on Y is

$$\text{LATE} = \frac{\text{Effect of } Z \text{ on } Y}{\text{Effect of } Z \text{ on } T}$$

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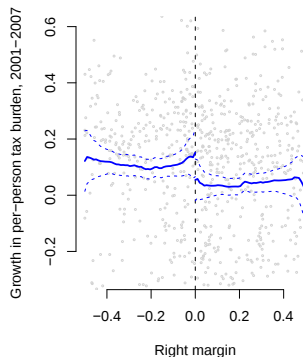
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- ▶ $\Rightarrow$ ignorability

REGRESSION DISCONTINUITY - EXAMPLE

In an election a right-wing party wins if they get just over 50% of the vote and loses if they get just under 50%. What is the effect of right-wing governments on tax policies? (Eggers 2013)



REGRESSION DISCONTINUITY - ISSUES

- ▶ Gives us an estimate of the **LATE** - estimate around the cutoff value of x .
- ▶ Always pay attention to the bandwidth size.
- ▶ Is the rule truly random?

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$$(E[Y|D = 1, T = 2] - E[Y|D = 1, T = 1]) - (E[Y|D = 0, T = 2] - E[Y|D = 0, T = 1])$$

DIFFERENCE-IN-DIFFERENCES - EXAMPLE

How do inflows of immigrants affect unemployment for native workers? One natural experiment is the Mariel Boatlift - in 1980 thousands of cubans emigrated to the Miami. (Card 1990)

Differences-in-differences estimates of the effect of immigration on unemployment^a

Group		Year		
		1979 (1)	1981 (2)	1981-1979 (3)
Whites				
(1)	Miami	5.1 (1.1)	3.9 (0.9)	- 1.2 (1.4)
(2)	Comparison cities	4.4 (0.3)	4.3 (0.3)	- 0.1 (0.4)
(3)	Difference Miami-comparison	0.7 (1.1)	- 0.4 (0.95)	- 1.1 (1.5)
Blacks				
(4)	Miami	8.3 (1.7)	9.6 (1.8)	1.3 (2.5)
(5)	Comparison cities	10.3 (0.8)	12.6 (0.9)	2.3 (1.2)
(6)	Difference Miami-comparison	- 2.0 (1.9)	- 3.0 (2.0)	- 1.0 (2.8)

^a Notes: Adapted from Card (1990, Tables 3 and 6). Standard errors are shown in parentheses.

DIFFERENCE-IN-DIFFERENCES - ASSUMPTIONS

- ▶ **Parallel Trends:** In the absence of treatment, the treated and control units would have had the same trend over time.

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- ▶ **Parallel Trends:** In the absence of treatment, the treated and control units would have had the same trend over time.
- ▶ **Sample Composition:** The composition of the sample should be the same over the pre-treatment and post-treatment periods.

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If we have a treated unit and several untreated units, we can find an appropriate and ignorable comparison by taking a combination of the untreated units.

- ▶ The **synthetic control** unit is a weighted average of all potential comparison units.
- ▶ Weights are chosen by comparing pre-treatment covariates of the untreated units with the treated unit.

SYNTHETIC CONTROLS - EXAMPLE

Did reunification affect GDP per capita in West Germany?
(Abadie, Diamond, and Hainmueller 2012)

Country	Weight	Country	Weight
Australia	0	Netherlands	0.10
Austria	0.42	New Zealand	0
Belgium	0	Norway	0
Denmark	0	Portugal	0
France	0	Spain	0
Greece	0	Switzerland	0.11
Italy	0	United Kingdom	0
Japan	0.16	United States	0.22

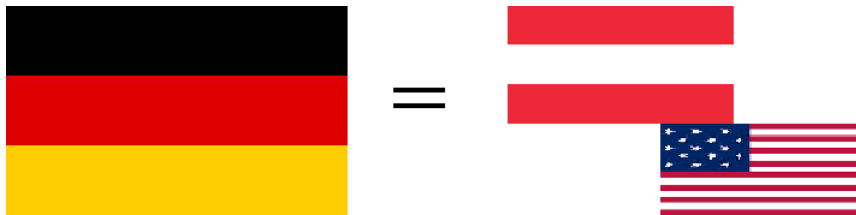
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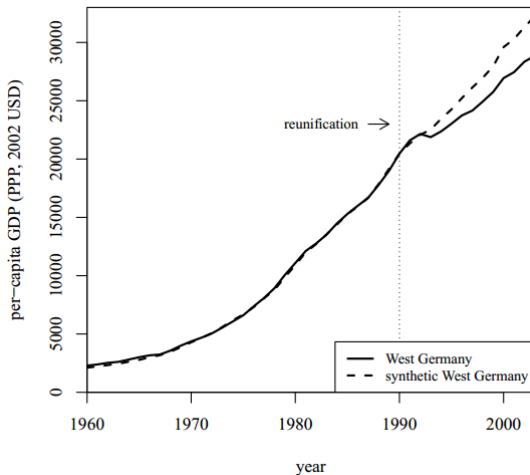
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	West Germany	Synthetic West Germany	OECD Sample
GDP per capita	15808.9	15800.9	8021.1
Trade Openness	56.8	56.9	31.9
Inflation Rate	2.6	3.5	7.4
Industry Share	34.5	34.4	34.2
Schooling	55.5	55.2	44.1
Investment Rate	27.0	27.0	25.9

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- ▶ In the absence of treatment, the treated unit and the synthetic control would behave the same.
- ▶ Treated unit must not be an extreme case to ensure the existence of a comparison unit.

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OUTLINE

Research Design and Causal Inference

Multiple Equation Models

Missing Data: A Preview

MULTIPLE EQUATION MODELS

Occasionally, the system we are studying produces multiple outputs:

- ▶ The number of presidential vetoes and the number of Congressional overrides in any given year
- ▶ The number of hostile acts directed toward Israel and the number of hostile acts directed toward Palestinians
- ▶ The monthly unemployment rate in the United States and the monthly inflation rate.

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STOCHASTIC DEPENDENCE

Example model:

1. $\vec{Y}_i \sim f_{BVN}(\vec{y}_i | \vec{\mu}_i, \Sigma)$
2. $\vec{\mu}_i = X_i \beta$

$$\vec{y}_i = [y_{1i}, y_{2i}]$$

$$\vec{\mu}_i = [\mu_{1i}, \mu_{2i}]$$

$$\Sigma = \begin{pmatrix} \sigma_1^2 & \sigma_{1,2} \\ \sigma_{2,1} & \sigma_2^2 \end{pmatrix}$$

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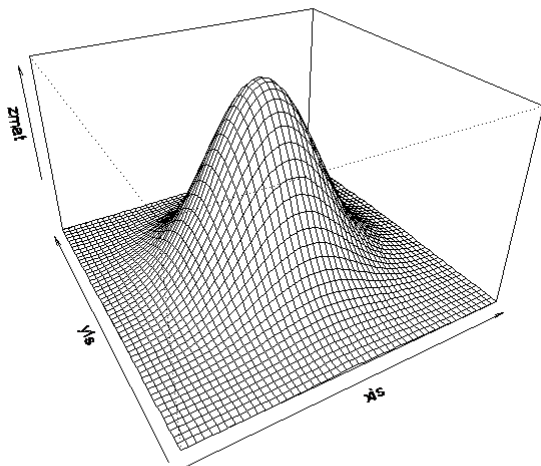
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- ▶ With stochastic dependence we can't do this. The vector of y 's are **jointly determined** by the probability density function.

$$L(\mu, \Sigma|y) \propto \prod_{i=1}^n BVN(\vec{y}_i|\vec{\mu}_i, \Sigma)$$

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What does the distribution of $\vec{y}_i$ look like?



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PARAMETRIC DEPENDENCE

- ▶ Parametric independence means we can treat one of the θ 's as a constant.

$$\max_{\mu_1, \Sigma} \ell(\mu, \Sigma | y) = \sum_{i=1}^n \ln f(y_{1i} | \mu_{1i}, \sigma_1^2) + \sum_{i=1}^n \ln f(y_{2i} | \mu_{2i}, \sigma_2^2)$$

$$\Rightarrow \max_{\mu_1, \Sigma} \ell(\mu, \Sigma | y) = \sum_{i=1}^n \ln f(y_{1i} | \mu_{1i}, \sigma_1^2)$$

- ▶ With parametric dependence we can't do this. Say $\mu_{1i} = \mu_{2i}$, then we can't drop any terms because:

$$\begin{aligned} \ell(\mu, \Sigma | y) &= \sum_{i=1}^n \ln f(y_{1i} | \mu_{1i}, \sigma_1^2) + \sum_{i=1}^n \ln f(y_{2i} | \mu_{2i}, \sigma_2^2) \\ &= \sum_{i=1}^n \ln f(y_{1i} | \mu_{1i}, \sigma_1^2) + \sum_{i=1}^n \ln f(y_{2i} | \mu_{1i}, \sigma_2^2) \end{aligned}$$

MULTIPLE EQUATION EXAMPLE

Let's look at the `grunfeld` data in `zelig`.

- ▶ Observations from 1935 to 1954 of 7 variables for two firms: General Electric and Westinghouse. The variables are
 - ▶ I_{ge} and I_w = Gross investment for GE and W, respectively;
 - ▶ F_{ge} and F_w = Market value of Firm at beginning of the year;
 - ▶ C_{ge} and C_w = Capital stock measure at beginning of the year.
- ▶ We are interested in modeling investment as a function of market value and capital stock.

MULTIPLE EQUATION EXAMPLE

- ▶ Y : $J \times n$ matrix with $j = 1, \dots, J$ and $i = 1, \dots, n$
 - ▶ j indexes dependent variables (2)
 - ▶ i indexes observations (20 years)

For the grunfeld data, $Y =$

Ige	Iw
33.1	12.93
45.0	25.90
77.2	35.05
44.6	22.89
48.1	18.84
74.4	28.57

MULTIPLE EQUATION EXAMPLE

- ▶ X : We're going to separate X into two parts:
 - ▶ X_{i1} : the covariates specific to the mean for dependent variable 1, in this case GE.
 - ▶ X_{i2} the covariates specific to the mean for dependent variable 2, in this case Westinghouse.

For the grunfeld data, $X =$

X_{i1}		X_{i2}	
Fge	Cge	Fw	Cw
1170.6	97.8	191.5	1.8
2015.8	104.4	516.0	0.8
2803.3	118.0	729.0	7.4
2039.7	156.2	560.4	18.1
2256.2	172.6	519.9	23.5
2132.2	186.6	628.5	26.5

SURM

Seemingly Unrelated Regression Model:

- ▶ Stochastic Component

$$\vec{Y}_i \sim MVN(\vec{y}_i | \vec{\mu}_i, \Sigma)$$

where

- ▶ $\vec{Y}_i$ and $\vec{\mu}_i$ are $J \times 1$
- ▶ Σ is $J \times J$
- ▶ Σ is symmetric and full; Why?

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where

- ▶ $\vec{Y}_i$ and $\vec{\mu}_i$ are $J \times 1$
 - ▶ Σ is $J \times J$
 - ▶ Σ is symmetric and full; Why? **Stochastic Dependence**
- ▶ Systematic Component

$$\mu_{i1} = X_{i1}\vec{\beta}$$

$$\mu_{i2} = X_{i2}\vec{\gamma}$$

where

- ▶ β : Estimates that predict GE mean
- ▶ γ : Estimates that predict Westinghouse mean

SURM

- The Likelihood

$$\begin{aligned} L(\mu, \Sigma) &= \prod_{i=1}^n MVN(\vec{y}_i | \vec{\mu}_i, \Sigma) \\ &= \prod_{i=1}^n (2\pi)^{-\frac{I}{2}} |\Sigma|^{-\frac{1}{2}} \exp \left[-\frac{1}{2} (\vec{y}_i - \vec{\mu}_i)' \Sigma^{-1} (\vec{y}_i - \vec{\mu}_i) \right] \end{aligned}$$

- What if Σ had off-diagonal elements that were zero?

SURM

- The Likelihood

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- What if Σ had off-diagonal elements that were zero?
Stochastic Independence

MULTIPLE EQUATION EXAMPLE

We can operationalize this model in R

```
formula <- list(mu1 = Ige ~ Fge + Cge,
                 mu2 = Iw ~ Fw + Cw)
sur.out <- systemfit(formula = formula,
                     method = "SUR", data = grunfeld)
sur.out

# To get the variance - covariance matrix
vcov.sur<-vcov(sur.out)
vcov.sur
# We can see that there are dependencies in our parameters across mo
```

MULTIPLE EQUATION EXAMPLE

We can operationalize this model in R

```
sur.out
```

```
Coefficients:
```

mu1_(Intercept)	mu1_Fge	mu1_Cge	mu2_(Intercept)
-27.7193171	0.0383102	0.1390363	-1.2519882

MULTIPLE EQUATION EXAMPLE

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-27.7193171	0.0383102	0.1390363	-1.2519882

Are the μ 's correlated?

```
summary(sur.out)
```

```
The correlations of the residuals
```

	mu1	mu2
mu1	1.000000	0.765043
mu2	0.765043	1.000000

MULTINOMIAL CHOICE MODELS

Suppose you have a choice among different, non-ordered things:



- ▶ Democrat, Republican, Independent
- ▶ Republican candidates for president
- ▶ at a restaurant between beef, chicken, or vegetarian
- ▶ to wear a blue, yellow, or green sweater

The choice sets are not ordered. But we can use generalizations of ordered logit and ordered probit to analyze.

MULTINOMIAL CHOICE EXAMPLE

We'll use a running example of vote choice between three candidates:

- Suppose for each individual i , we observe her vote with

$$V_{ij} = (V_{i1}, V_{i2}, V_{i3})$$

where $V_{ij} = 1$ if the person votes for candidate j and is zero otherwise.

- We could use the ordered logit/probit from the beginning of the semester. But this assumes candidates are ordered along a single dimension.
- Multinomial choice models relax this assumption; we assume there is no order to the choice under consideration, just categories.

MULTINOMIAL LOGIT

► **Stochastic Component:**

$$\vec{V}_i \sim \text{Multinomial}(\vec{v}_i | \vec{\pi}_i)$$

where

$$Pr(V_{ij} = 1 | X_i, \beta_j) = \pi_{ij}$$

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$$\text{Pr}(V_{ij} = 1 | X_i, \beta_j) = \pi_{ij}$$

► **Systematic Component:**

$$\pi_{ij} = \frac{\exp(X_i \beta_j)}{\sum_{k=1}^3 \exp(X_i \beta_k)}$$

MULTINOMIAL LOGIT

- ▶ Suppose we have n voters and 3 candidates.
- ▶ The likelihood model for the data, having observed an $n \times K$ matrix of covariates X and an $n \times 3$ matrix of votes V is:

$$L(\beta|X, V) \propto \prod_{i=1}^n \prod_{j=1}^3 \pi_{ij}^{v_{ij}}$$

We can operationalize this in R using `Zelig` or the `mlogit` package.

MUTLINOMIAL LOGIT

Let's look at the 1988 presidential vote in Mexico.

Our hypothesis is that vote for the ruling PRI party varies with age.

```
data(mexico)
```

```
ml.out <- zelig(as.factor(vote88) ~ age + female,  
               model = "mlogit", data = mexico)
```

```
ml.out
```

```
Coefficients:
```

(Intercept):1	(Intercept):2	age:1	age:2	female:
0.104832885	-0.500933247	0.016842559	0.008101289	0.26598923

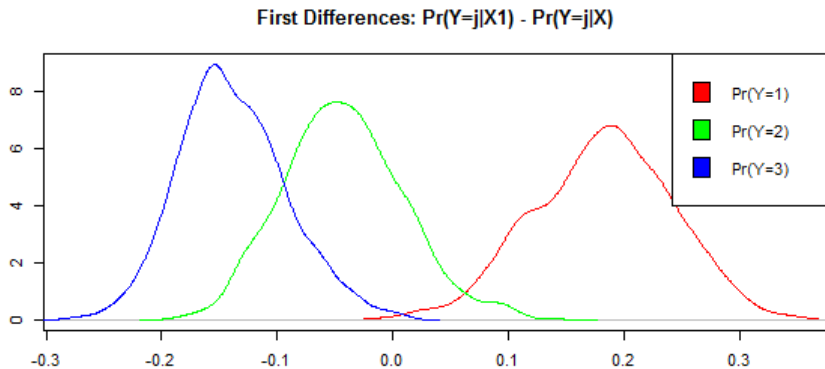
MULTINOMIAL LOGIT

Let's look at the difference in expected vote choice for the youngest and the oldest.

```
x.young <- setx(ml.out, age = min(mexico$age))
x.old<- setx(ml.out, age = max(mexico$age))
ml.sim <- sim(ml.out, x1 = x.old, x = x.young)

summary(ml.sim)
First Differences: Pr(Y=k|X1) - Pr(Y=k|X)
              mean          sd          2.5%          97.5%
Pr(Y=1)  0.1831760 0.06264429  0.0618293  0.30277899
Pr(Y=2) -0.0406195 0.05047512 -0.1350517  0.06689080
Pr(Y=3) -0.1425565 0.04747605 -0.2322232 -0.04139632
```

MULTINOMIAL LOGIT



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- ▶ The multinomial logit assumes **independence of irrelevant alternatives (IIA)**.

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- ▶ My choice between Shake Shack and Russell House does not depend on the option of Grafton Street. I.e. if Grafton Street closed, the customers would equally go between Shake Shack and Russell House.

MULTINOMIAL LOGIT

- ▶ The multinomial logit assumes **independence of irrelevant alternatives (IIA)**.
- ▶ My choice between Shake Shack and Russell House does not depend on the option of Grafton Street. I.e. if Grafton Street closed, the customers would equally go between Shake Shack and Russell House.
- ▶ Why do you need IIA? Because, under multinomial logit, your decision between options 1 and 2 never depends on option 3:

$$\frac{\pi_i(1)}{\pi_i(2)} = \frac{\frac{\exp(X_i\beta_1)}{\sum \exp(X_i\beta_j)}}{\frac{\exp(X_i\beta_2)}{\sum \exp(X_i\beta_j)}} = \frac{\exp(X_i\beta_1)}{\exp(X_i\beta_2)}$$

MULTINOMIAL PROBIT

- Relaxes the IIA assumption.

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- Relaxes the IIA assumption.

Suppose that voter i receives utility from voting for candidate j :

$$U_{ij}^* \sim N(u_{ij}^* | \mu_{ij}, \Sigma)$$

where Σ is the 3×3 var-cov matrix.

The voter will vote for whoever gives her the highest utility, so we have the following observation mechanism

$$V_{ij} = \begin{cases} 1 & \text{if } U_{ij}^* > U_{ij'}^* \forall j' \neq j \\ 0 & \text{otherwise.} \end{cases}$$

MULTINOMIAL PROBIT

Stochastic Component:

$$V_i \sim \text{Multinomial}(v_i | \pi_i)$$

MULTINOMIAL PROBIT

Stochastic Component:

$$V_i \sim \text{Multinomial}(v_i | \pi_i)$$

Systematic Component:

$$\Pr(V_{ij} = 1) = \pi_{ij}$$

such that

$$\sum_{j=1}^3 \pi_{ij} = 1$$

MULTINOMIAL PROBIT

To compute π_{ij} , we need to calculate the following:

$$\pi_{ij} = Pr(U_{ij}^* > U_{ij'}^*) \forall j' \neq j$$

which is a messy series of integrals. For example, for $j = 3$:

$$Pr(U_{i3}^* > U_{ij}^* \forall j \neq 3) = \int_{-\infty}^{\infty} \int_{-\infty}^{U_{i3}} \int_{-\infty}^{U_{i3}} f(U_1, U_2, U_3) dU_1 dU_2 dU_3.$$

But it's less messy than it looks!

MULTINOMIAL PROBIT

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- ▶ We no longer need to assume IIA because we have included other options directly in this integral.
- ▶ However, this is hard to identify because of potential correlations between unobserved latent dimensions.
- ▶ Note: Imai and Van Dyck's MNP package will calculate multinomial probit using Bayesian methods.

OUTLINE

Research Design and Causal Inference

Multiple Equation Models

Missing Data: A Preview

MISSING DATA: A PREVIEW

This could be our data matrix:

<i>Observation</i>	<i>Age</i>	<i>Education</i>	<i>Income</i>
1	13	4	
2	56	32	\$100,000
3	24		\$ 30,000
4		12	\$ 20,000
⋮	⋮	⋮	⋮

MULTIPLE EQUATION MODELS AND MISSING DATA

Wait, it's a multiple equation model!

<i>Observation</i>	<i>Age</i>	<i>Education</i>	<i>Income</i>
$Y_{1j} =$	(13	4	)
$Y_{2j} =$	(56	32	\$100,000)
$Y_{3j} =$	(24		\$ 30,000)
$Y_{4j} =$	(	12	\$ 20,000)
$\vdots$	$\vdots$	$\vdots$	$\vdots$

UNDERSTANDING MISSINGNESS IN OUR DATA

How was this missingness generated? We can characterize the missingness mechanism as:

- ▶ **Missing completely at random (MCAR):** missingness purely random; unrelated to variables in data or any unobserved variables

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When can we use multiple imputation?

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When can we use multiple imputation? **When our data are MAR**

MULTIPLE EQUATION MODELS AND MISSING DATA

We can model this the same way we've modeled multiple equation models:

$$L(\mu, \Sigma|D) \propto \prod_{i=1}^n \text{MVN}(D_i|\mu, \Sigma)$$

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MULTIPLE EQUATION MODELS AND MISSING DATA

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$$L(\mu, \Sigma|D) \propto \prod_{i=1}^n \text{MVN}(D_i|\mu, \Sigma)$$

- As usual, we can make μ a function of some covariates (what predicts our data?).

MULTIPLE EQUATION MODELS AND MISSING DATA

But aren't some of the data missing?

<i>Observation</i>	<i>Age</i>	<i>Education</i>	<i>Income</i>
$Y_{1j} =$	(13	4	?)
$Y_{2j} =$	(56	32	\$100,000)
$Y_{3j} =$	(24	?	\$ 30,000)
$Y_{4j} =$	(?	12	\$ 20,000)
$\vdots$	$\vdots$	$\vdots$	$\vdots$

MULTIPLE IMPUTATION

We'll integrate out the missing components:

$$\begin{aligned} L(\mu, \Sigma | D_{obs}) &\propto \prod_{i=1}^n \int MVN(D_i | \mu, \Sigma) dD_{mis} \\ &= \prod_{i=1}^n MVN(D_{i,obs} | \mu_{obs}, \Sigma_{obs}) \end{aligned}$$

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- ▶ Use the SURM model for the observed data to get estimates of μ and Σ
- ▶ Use these estimates to simulate to estimate the missing observations
- ▶ Include in your model all X 's that you think affect missingness! (And all X 's in your model)

MULTIPLE IMPUTATION

Steps to multiple imputation:

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1. Impute m values for each missing element

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MULTIPLE IMPUTATION

Steps to multiple imputation:

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3. Run your statistical model on each imputed data set
4. Calculate the point estimate *across* imputed data sets:

$$\bar{q} = \frac{1}{m} \sum_{j=1}^m q_j$$

5. Calculate the standard error for this estimate *across* imputed data sets:

$$S[\bar{q}] = \sqrt{\text{mean}(SE_k^2) + \text{Var}[q_j] \times \left(1 + \frac{1}{m}\right)}$$

AMELIA

```
library(Amelia)
data(africa)

africa[1,]
  year      country gdp_pc  infl trade civlib population
1 1972 Burkina Faso   377 -2.92 29.69    0.5    5848380

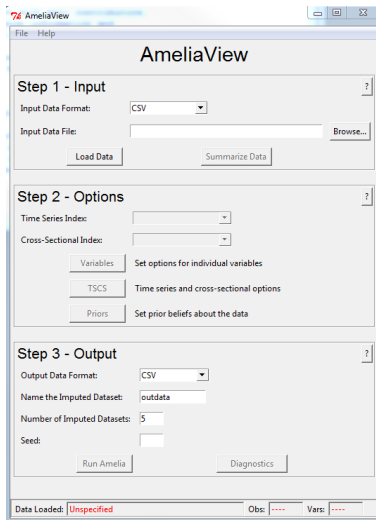
set.seed(1234)
a.out <- amelia(x = africa, cs = "country", ts = "year",
  logs = "gdp_pc", m = 5)
```

`a.out` is a *list* of 5 imputed datasets, each of which can be accessed using `a.out$imputations[[i]]`.

GUI

You can use the GUI by typing:

```
AmeliaView()
```



ESTIMATION WITH IMPUTED DATA

Now, we just want to estimate a basic regression model with our imputed data,

ESTIMATION WITH IMPUTED DATA

Now, we just want to estimate a basic regression model with our imputed data, but we have 5 datasets!

```
z.out.imp <- zelig(trade ~ log(population) + log(gdp_pc) + infl
  + civlib, data = a.out$imputations, model = "ls")
summary(z.out.imp)
```

Coefficients:

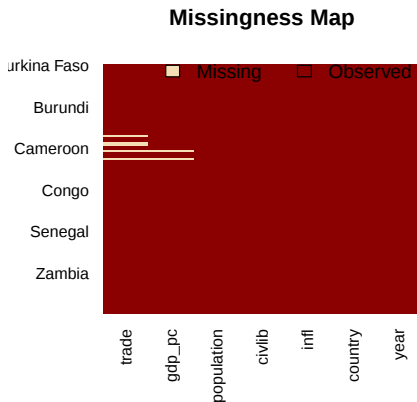
	Value	Std. Error	t-stat	p-value
(Intercept)	112.4097	45.47779	2.472	1.345e-02
log(population)	-17.8513	2.36646	-7.543	4.595e-14
log(gdp_pc)	31.3875	2.31108	13.581	1.834e-41
infl	0.2605	0.05836	4.463	8.075e-06
civlib	27.2104	6.67210	4.078	4.595e-05

Zelig will automatically combine the results of the different models, but if a model you are using isn't programmed in Zelig, it isn't hard to combine your estimates.

DIAGNOSTICS

The missingness map gives an overall sense of the shape and extent of the missingness.

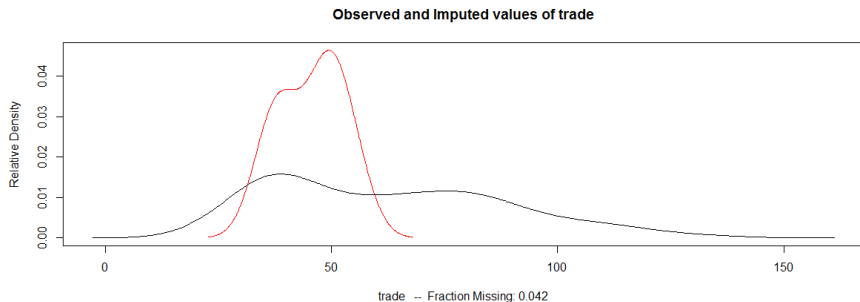
```
missmap(a.out)
```



DIAGNOSTICS

Plotting the Amelia object contrasts empirical and imputed densities.

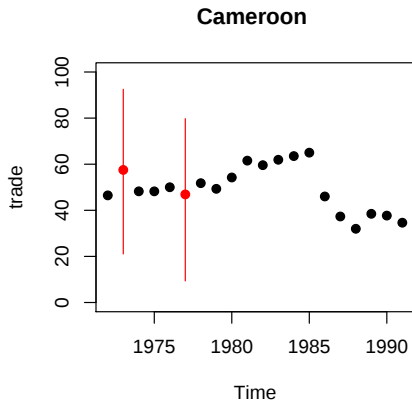
```
plot(a.out)
```



DIAGNOSTICS

Plotting Time-series cross-sectional plots

```
tscsPlot(a.out, var = "trade", cs = "Cameroon")
```

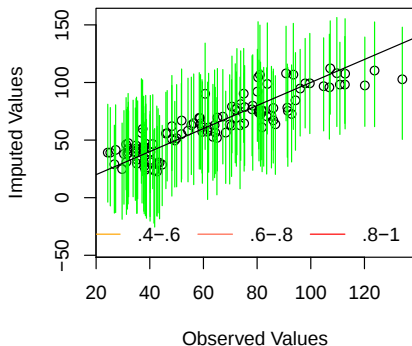


DIAGNOSTICS

Overimputation for a specific variable tests the imputation model by imagining that each observation is missing and generating some imputations to check performance.

```
overimpute(a.out, var = "trade")
```

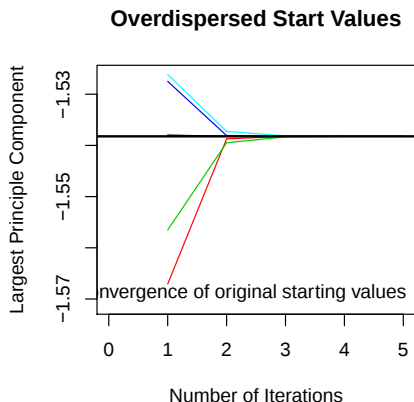
Observed versus Imputed Values of trade



DIAGNOSTICS

The `disperse` function starts the algorithm at some unlikely values to check that `amelia` hasn't found a local rather than a global maximum for the likelihood of the complete data.

```
disperse(a.out, dims = 1, m = 5)
```



CONSIDERATIONS: TRANSFORMATIONS

Recall that our model assumes multivariate normal data. This suggests some issues:

1. Ordinal variables: imputation is faster and more informative if ordinal variables are permitted to be continuous, but if undesirable use `ords` argument to constrain.
2. Nominal (unordered) variables: `amelia` automatically converts into factors and imputes accordingly if a variable is passed to the `noms` argument.
3. Various transformations (logarithmic, root) are pre-programmed to make skewed distributions more normal.

CONSIDERATIONS: TIME SERIES/PANEL DATA

1. Use the `ts` and `cs` arguments to tell `amelia` the panel structure. It uses this information for constructing time series correctly.
2. `polytime` can be used to add in polynomial time trends (up to a cubic) and `intercs = TRUE` creates a separate trend for each panel.
3. Add in lags and leads if you think it will help predict the missing data. Use `lags` and `leads` with some variable.

THINGS TO REMEMBER

1. Set the seed!
2. Include any variable in the analysis model in your imputation model.
3. Don't impute things that don't make sense.
4. Check diagnostics (and think carefully about applicability).
5. Remember transformations, polynomials and data structure.