9: Eksperimenter I

Videregående kvantitative metoder i studiet af politisk adfærd

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Sidste gang:

- TEST-kriterierne
- modelbaseret inferens og Leamer-kritikken
- designbaseret inferens og credibility-revolutionen
- 'bad controls'
- case: workplace wellness

Tema i dag:

https://youtu.be/BvUbv4iwbDs

Lad os antage et treatment vi interesserer os for, D, og en påvirket gruppe ($D_i = 1$) og en upåvirket gruppe ($D_i = 0$). Vi definerer nu *for hvert individ i*:

$$\text{Potential ouctome} = \begin{cases} Y_{1i} & \text{if } D_i = 1 \\ Y_{0i} & \text{if } D_i = 0 \end{cases} \tag{1}$$

$\Longrightarrow$ værdien Y_i bestemmes af flg. *switching equation*:

$$Y_i = Y_{0i} + (Y_{1i} - Y_{0i})D_i \tag{2}$$

$\rightarrow$ Y_i antager altid værdien Y_{0i} *eller* Y_{1i}

For hvert individ i kan vi definere effekten af treatment::

$$\tau_i = Y_{1i} - Y_{0i} \tag{3}$$

M.a.o.: *effekten* τ_i *er forskellen mellem* Y_i *når* $D_i = 1$ *og* Y_i *når* $D_i = 0$

- kaldes også 'Rubin causal model' efter Donald B. Rubin
- gennemsnittet på tværs af τ_i kaldes *average treatment effect* (ATE)
- problem: D_i er altid *enten* 1 *eller* 0
- $\rightarrow$ vi observerer altid kun Y_{i1} *eller* Y_{i0}
- $\rightarrow$ vi kan aldrig observere τ_i
- denne uobserverbarhed kaldes the fundamental problem of causal inference

To studerende, **K**huzdar & **M**aria

- Y_{1i}: potentielt outcome hvis *i* tager på hospitalet
- Y_{0i}: potentielt outcome hvis *i* *ikke* tager på hospitalet
- i dette case: hospitalisering gavner Khuzdar, men ikke Maria

$$Y_{1K} - Y_{0K} = 4 - 3 = 1 \tag{4}$$

$$Y_{1M} - Y_{0M} = 5 - 5 = 0 \tag{5}$$

Fuldt potential outcomes schedule:

	Khuzdar	Maria
Y_{0i}	3	5
Y_{1i}	4	5
D_i	1	0
Y_i	4	5
$Y_{1i} - Y_{0i}$	1	0

Observerede outcomes:

	Khuzdar	Maria
Y_{0i}	?	5
Y_{1i}	4	?

$$\rightarrow \bar{Y}_1 - \bar{Y}_0 = 4 - 5 = -1$$

Den direkte sammenligning afspejler både ATE hos de treatede + selection bias:

$$\begin{aligned} Y_K - Y_M &= Y_{1K} - Y_{0M} && (6) \\ &= Y_{1K} - Y_{0K} + Y_{0K} - Y_{0M} && (7) \\ &= 1 + (-2) && (8) \\ &= -1 && (9) \end{aligned}$$

forskel i gruppegennemsnit = gns. effekt på de treatede + selection bias

M. mere generel notation i AP+GG:

$$E[Y_{1i}|D_i = 1] - E[Y_{0i}|D_i = 0] = \\ E[Y_{1i} - Y_{0i}|D_i = 1] + E[Y_{0i}|D_i = 1] - E[Y_{0i}|D_i = 0] \quad (10)$$

når treatment randomiseres er $Y_0 i$ uafhængig af D_i:

$$E[Y_{0i}|D_i = 1] - E[Y_{0i}|D_i = 0] = 0 \tag{11}$$

m.a.o.: random assignment eliminerer selection bias

Når treatment af de første m af i alt N enheder er randomiseret har vi:

$$
\begin{aligned}
E\left[\frac{\sum_1^m Y_i}{m}\right] - E\left[\frac{\sum_{m+1}^N Y_i}{N-m}\right] &= \quad (12)\\
E[Y_{1i}] - E[Y_{0i}] &= \quad (13)\\
E[\tau_i] &= \quad (14)\\
ATE & \quad (15)
\end{aligned}
$$

$\rightarrow$ difference-in-means estimatoren er en unbiased estimator af ATE

Group	Some HI	No HI	Difference
Husbands	4.01	3.70	.31 (.03)
Wives	4.02	3.62	.39 (.04)

$\rightarrow$ hvad indikerer dette om effekten af sundhedsforsikring på sundhed?

Resultater fra RAND HIE:

TABLE 1.4
Health expenditure and health outcomes in the RAND HIE

	Means	Differences between plan groups			
	Catastrophic plan (1)	Deductible – catastrophic (2)	Coinsurance – catastrophic (3)	Free – catastrophic (4)	Any insurance – catastrophic (5)
A. Health-care use					
Face-to-face visits	2.78	.19	.48	1.66	.90
	[5.50]	(.25)	(.24)	(.25)	(.20)
Outpatient expenses	248	42	60	169	101
	[488]	(21)	(21)	(20)	(17)
Hospital admissions	.099	.016	.002	.029	.017
	[.379]	(.011)	(.011)	(.010)	(.009)
Inpatient expenses	388	72	93	116	97
	[2,308]	(69)	(73)	(60)	(53)
Total expenses	636	114	152	285	198
	[2,535]	(79)	(85)	(72)	(63)
B. Health outcomes					
General health index	68.5	.87	.61	.78	.36

Succesfuld randomisering kan efterprøves m. *balance tests*

TABLE 1.3
Demographic characteristics and baseline health in the RAND HIE

	Means	Differences between plan groups			
	Catastrophic plan (1)	Deductible – catastrophic (2)	Coinsurance – catastrophic (3)	Free – catastrophic (4)	Any insurance – catastrophic (5)
A. Demographic characteristics					
Female	.560	−.023	−.025	−.038	−.030
		(.016)	(.015)	(.015)	(.013)
Nonwhite	.172	−.019	−.027	−.028	−.025
		(.027)	(.025)	(.025)	(.022)
Age	32.4	.56	.97	.43	.64
	[12.9]	(.68)	(.65)	(.61)	(.54)
Education	12.1	−.16	−.06	−.26	−.17
	[2.9]	(.19)	(.19)	(.18)	(.16)
Family income	31,603	−2,104	970	−976	−654
	[18,148]	(1,384)	(1,389)	(1,345)	(1,181)
Hospitalized last year	.115	.004	−.002	.001	.001
		(.016)	(.015)	(.015)	(.013)

Gerber & Greens procedure:

»First, determine N, the number of subjects in your experiment, and m, the number of subjects who will be allocated to the treatment group. Second, set a random number 'seed' using a statistics package, so that your random numbers may be reproduced by anyone who cares to replicate your work. Third, generate a random number for each subject. Fourth, sort the subjects by the random numbers in ascending order. Finally, classify the first m observations as the treatment group.« (37)

Regressionsanalyse af randomiseret treatment D_i:

$$Y_i = \alpha + \beta D_i + \epsilon_i \tag{16}$$

Med tilføjet vektor af kontrolvariable X_i':

$$Y_i = \alpha + \beta D_i + X_i'\gamma + \epsilon_i \tag{17}$$

Hvorfor tilføjer vi X_i'?

1. For at rette op på kendte ubalancer i treatment
2. For at reducere residual varians i outcome $\rightarrow \downarrow$ standardfejl

$\rightarrow$ mere om dette i næste uge!

To kritiske antagelser om potential outcomes:

1. excludability
2. non-interferens (SUTVA)

Ad (1):

Lad $Y_i(z, d)$ være potential outcome for treatment assignment $z_i = z$ og faktisk treatment status $d_i = d$

Eksklusionsrestriktionsantagelsen: $Y_i(1, d) = Y_i(0, d)$

Ad (2):

Lad $Y_i(\mathbf{z}, \mathbf{d})$ være PO for Y_i for den fulde mængde af assignments og treatments

Under non-interferens: $Y_i(\mathbf{z}, \mathbf{d}) = Y_i(z, d)$

TABLE 2. Effects of Four Mail Treatments on Voter Turnout in the August 2006 Primary Election

	Experimental Group				
	Control	Civic Duty	Hawthorne	Self	Neighbors
Percentage Voting	29.7%	31.5%	32.2%	34.5%	37.8%
N of Individuals	191,243	38,218	38,204	38,218	38,201

Neighbors mailing

30423-3

For more information: (517) 351-1975
email: etov@grebner.com
Practical Political Consulting
P. O. Box 6249
East Lansing, MI 48826

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THE JACKSON FAMILY
9999 MAPLE DR
FLINT MI 48507

Dear Registered Voter:

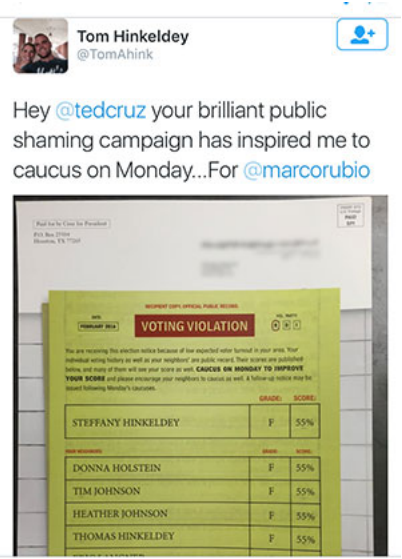
WHAT IF YOUR NEIGHBORS KNEW WHETHER YOU VOTED?

Why do so many people fail to vote? We've been talking about the problem for years, but it only seems to get worse. This year, we're taking a new approach. We're sending this mailing to you and your neighbors to publicize who does and does not vote.

The chart shows the names of some of your neighbors, showing which have voted in the past. After the August 8 election, we intend to mail an updated chart. You and your neighbors will all know who voted and who did not.

DO YOUR CIVIC DUTY — VOTE!

MAPLE DR	Aug 04	Nov 04	Aug 06
9995 JOSEPH JAMES SMITH	Voted	Voted	______
9995 JENNIFER KAY SMITH		Voted	______
9997 RICHARD B JACKSON		Voted	______


Tom Hinkeldey
@TomAhink
Hey @tedcruz your brilliant public shaming campaign has inspired me to caucus on Monday...For @marcorubio
VOTING VIOLATION
GRADE: SCORE:
STEFFANY HINKELDEY F 55%
DONNA HOLSTEIN F 55%
TIM JOHNSON F 55%
HEATHER JOHNSON F 55%
THOMAS HINKELDEY F 55%

Næste gang:

- eksperimenter II
- læsefokus i GG: cluster random assignment, covariate adjustment, noncompliance
- case: Gerber & Green (2000) → fokus på afsnit om "The Personal Canvassing Experiment"s. 657f
- udfyld meget gerne halvvejsevaluering! `https://fghjorth.typeform.com/to/nen7Xb`

Tak for i dag!