Accounting and causal effects: challenges and potential remedies

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outline

- accounting ingredients — causal effect of strategic disclosure
- focus on treatment effects
- challenges — identification
- potential remedies
    - ignorable treatment identification strategies
    - instrumental variable identification strategies
    - partial identification strategies
- varieties of treatment effects and data analytic approaches
- examples of effective/ineffective identification strategies

accounting causal effects

- endogenous nature of causal effects makes assessing welfare impact of accounting choice challenging
- strategic disclosure:
    - discrete — recognize/disclose or not
    - continuous — information precision

accounting causal effects

- ingredients:
    - uncertainty
    - asymmetric information
    - multiple sources of information
    - equilibrium behavior
    - audited reports may result in welfare improvement

treatment effects

- special case of causal effects
- for concreteness and simplicity, we'll focus on binary treatment effects; for example, disclose or don't disclose
  - $TE = Y_1 - Y_0$
  - Y_1 is (potential) outcome with treatment
  - Y_0 is (potential) outcome without treatment
  - $D = 1$ treatment is chosen or assigned
  - $D = 0$ no treatment is chosen or assigned
  - observed outcome: $Y = DY_1 + (1 - D)\, Y_0$
  - observable data: $Y_1 | D = 1$ and $Y_0 | D = 0$
  - counterfactuals: $Y_1 | D = 0$ and $Y_0 | D = 1$

common treatment effects

conditional average treatment effects

- average treatment effect for individuals who selected treatment conditional on observables/regressors

$$ATT(X) = E[Y_1 - Y_0 \mid X = x, D = 1]$$

- average treatment effect for individuals who selected no treatment conditional on observables/regressors

$$ATUT(X) = E[Y_1 - Y_0 \mid X = x, D = 0]$$

common treatment effects

conditional average treatment effects

- average treatment effect for individuals chosen or assigned treatment at random conditional on observables/regressors

$$
\begin{aligned}
ATE(X) &= E[Y_1 - Y_0 \mid X = x] \\
&= Pr(D = 1 \mid X)\, ATT(X) + Pr(D = 0 \mid X)\, ATUT(X)
\end{aligned}
$$

- for propensity score matching, covariates $X = x$ are replaced by $P(x) = p$ in the conditional expectation expression

$$\begin{aligned} ATE(P(x)) &= E\left[Y_1 - Y_0 \mid P(x) = p\right] \\ &= Pr(D = 1 \mid P(x) = p)\, ATT(P(x)) \\ &\quad + Pr(D = 0 \mid P(x) = p)\, ATUT(P(x)) \end{aligned}$$

common treatment effects

unconditional average treatment effects

- with full common support (so-called identification at infinity), unconditional average effects are derived from conditional average effects via iterated expectations
  - otherwise, we're only able to identify local average effects
  - description of common support indicates range of evidence
- for example, the average treatment effect for individuals who are selected for treatment at random

$$\begin{aligned} ATE &= E_X\left[E\left[Y_1 - Y_0 \mid X\right]\right] \\ &= E\left[Y_1 - Y_0\right] \end{aligned}$$

challenges

- Identification! Identification! Identification!
  - framing the causal effect problem
    - — rich variety of potential effects of interest makes this step of paramount importance
  - causal effect parameter identification
    - — typically maps observable outcome Y to effect via probability theory

challenges

- counterfactual nature
- common support (treated and untreated)
- unobservability (partial observability) of beliefs, preferences, and potential outcomes

challenges

- observable and unobservable heterogeneity
  - how likely is homogeneity?
- instrumental variable strategies can accommodate outcome heterogeneity but require uniform treatment adoption
- greater explanatory power may **increase** selection bias

challenges

- more ambitious agendas
  - suspend stable unit treatment value assumption (*SUTVA*) — allow interaction effects among individuals
  - Cowles' commission fully structural analysis (including specification of preferences and incentives) of general equilibrium effects in environments with which we have no prior experience

potential remedies

- thought experiments
    - framing problem and focal parameter(s)
    - identification of quantities of interest

potential remedies

- thought experiments
    - probability assignment
        - Jaynes' maximum entropy is one probability assignment approach
        - Ross' recovery theorem assigns probabilities and preferences from state prices
        - Leamer's specification searches with various priors and likelihoods allows the reader to judge the data summaries for themselves
        - Manski's partial identification & law of decreasing credibility

potential remedies

- varieties of treatment effects and questions posed
    - discrete/continuous treatment
    - conditional (on observables/regressors) and unconditional (iterated expectations) treatment effects
    - population-level effect
        - marginal
        - average
        - quantile
    - *MTE* connects to other treatment effects via weighting functions

potential remedies

- identification strategies
    - ignorable treatment (selection on observables or unconfoundedness)
        - — treatment is mean conditionally independent
        - — strong ignorability involves stochastic conditional independence

potential remedies

- identification strategies
  - instrumental variables (exclusion restrictions)
    - — instruments are independent of outcomes
    - — instruments are related to treatment adoption
    - — instruments allow manipulation of treatment without affecting outcomes to infer counterfactuals
    - — treatment adoption is uniform in the instruments

potential remedies

- identification strategies
    - bounding/partial identification when point-identification is not feasible or credible

potential remedies

- common support
  - evaluate overlap in covariate distribution
  - examine histograms of the estimated propensity score by treatment status
  - propensity score matching to determine sample

potential remedies
limited common support

- example — nonparametric identification with limited common support
- *DGP*

Y_1	Y_0	TE	Y	D	X
11	4	7	4	0	0
2	6	-4	6	0	-1
1	5	-4	5	0	-1
11	4	7	4	0	0
11	4	7	11	1	0
11	4	7	11	1	0
9	3	6	9	1	1
10	2	8	10	1	1

Examples

limited common support

- various treatment effects

$$\begin{aligned} ATT\left(X=0\right) &= ATT\left(X=1\right) \\ &= ATT = 7 \\ ATUT\left(X=-1\right) &= -4,\ ATUT\left(X=0\right) = 7 \\ &= ATUT = 1.5 \\ ATE\left(X=-1\right) &= -4,\ ATE\left(X=0\right) = ATE\left(X=1\right) = 7 \\ &= ATE = 4.25 \end{aligned}$$

- only conditional treatment effects (at $X=0$) are nonparametrically identified by the data:
  $ATT\left(X=0\right) = ATUT\left(X=0\right) = ATE\left(X=0\right) = 7$

potential remedies

- some varieties of data analytic strategies
    - nonparametric regression
    - general matching
    - propensity score matching
    - fixed effects
    - difference-in-differences (*DID*)
    - linear *2SLS-IV*

        — interpretation depends on instruments & covariates
    - control or replacement functions
    - local *IV* (semi-nonparametric)

potential remedies

- some varieties of data analytic strategies
  - regression discontinuity design
  - doubly-robust combination methods
    - — regression with propensity score weighting (*WLS*)
    - — subclassification and regression
    - — matching and regression
  - correlated random coefficients for continuous treatment
  - simulation, say, of general equilibrium effects
  - *McMC* Bayesian data augmentation
  - and more . . .

potential remedies

- assessing refutability
    - no direct tests due to counterfactual nature — credibility is a thought experiment
    - evaluate refutable partial identification bounds against the data
    - highly context specific — for example, check for change in means or distribution of covariates around the threshold for regression discontinuity designs

potential remedies

- data
  - better data is perhaps the most effective, albeit nontrivial, remedy
  - outcomes, regressors/covariates, instruments, etc. are typically inadequate for addressing the questions we wish to probe (welfare effects)
  - the efforts of empiricists toward this goal are typically undervalued — a disservice to the discipline

examples

2SLS-IV identifies LATE — treatment effect depends on instrument choice

- *DGP — nonignorable, heterogeneous treatment effect*

Y_1	Y_0	TE	Y	D	Z
15	10	5	15	1	1
15	10	5	15	1	0
10	10	0	10	1	1
10	10	0	10	0	0
5	10	-5	10	0	1
5	10	-5	10	0	0

- $LATE = E\left[Y_1 - Y_0 \mid D_1 - D_0 = 1\right] = IVE = \frac{E[Y|Z=1]-E[Y|Z=0]}{E[D|Z=1]-E[D|Z=0]}$
- regressors: $\{1, D\}$; instruments: $\{1, Z\}$
- $LATE$ is the instrument-dependent treatment effect for an unidentified subpopulation of compliers
- compliers are rows 3 and 4

examples

2SLS-IV identifies LATE — treatment effect depends on instrument choice

- various treatment effects

$$\begin{array}{rr} ATT = 3\frac{1}{3} & ATUT = -3\frac{1}{3} \\ ATE = 0 & OLS = 3\frac{1}{3} \\ LATE = 0 & IVE = 0 \end{array}$$

- the local average treatment effect is identified via *2SLS-IV* as the binary instrument, Z, satisfies the exclusion restriction, $\Pr(Y_i \mid Z = 1) = \Pr(Y_i \mid Z = 0)$ for $i = 0, 1$, and is related to selection, D, $\Pr(D \mid Z) \neq \Pr(D)$
- treatment effect identified depends on instrument and applies to an unidentified subpopulation of compliers, $D_1 - D_0 = 1$

examples

LATE = ATT

- *DGP — nonignorable, heterogeneous treatment effect*

Y_1	Y_0	TE	Y	D	Z
15	10	5	15	1	1
15	10	5	10	0	0
20	20	0	20	0	1
20	20	0	20	0	0
10	10	0	10	1	1
10	10	0	10	0	0

- $LATE = E\left[Y_1 - Y_0 \mid D_1 - D_0 = 1\right] = IVE = \frac{E[Y|Z=1]-E[Y|Z=0]}{E[D|Z=1]-E[D|Z=0]}$
- regressors: $\{1, D\}$; instruments: $\{1, Z\}$
- compliers are rows 1, 2, 5, and 6

examples
LATE = ATT

- various treatment effects

$$\begin{array}{cc} ATT = 2.5 & ATUT = 1.25 \\ ATE = 1\frac{2}{3} & OLS = -2.5 \\ LATE = 2.5 & IVE = 2.5 \end{array}$$

- the local average treatment effect is identified via *2SLS-IV* as the binary instrument, Z, satisfies the exclusion restriction, $\Pr(Y_i \mid Z = 1) = \Pr(Y_i \mid Z = 0)$ for $i = 0, 1$
- $LATE$ equals ATT as $\Pr(D = 1 \mid Z = 0) = 0$

- *DGP — nonignorable, heterogeneous treatment effect*

Y_1	Y_0	TE	Y	D	Z
15	10	5	15	1	1
15	10	5	10	0	0
20	10	10	20	1	1
20	10	10	20	1	0
10	10	0	10	1	1
10	10	0	10	0	0

- $LATE = E\left[Y_1 - Y_0 \mid D_1 - D_0 = 1\right] = IVE = \frac{E[Y|Z=1]-E[Y|Z=0]}{E[D|Z=1]-E[D|Z=0]}$
- regressors: $\{1, D\}$; instruments: $\{1, Z\}$
- compliers are rows 1, 2, 5, and 6

examples

LATE = ATUT

- various treatment effects

$$\begin{array}{cc} ATT = 6.25 & ATUT = 2.5 \\ ATE = 5 & OLS = 6.25 \\ LATE = 2.5 & IVE = 2.5 \end{array}$$

- the local average treatment effect is identified via *2SLS-IV* as the binary instrument, Z, satisfies the exclusion restriction, $\Pr(Y_i \mid Z = 1) = \Pr(Y_i \mid Z = 0)$ for $i = 0, 1$
- $LATE$ equals $ATUT$ as $\Pr(D = 1 \mid Z = 1) = 1$

examples

LATE — defiers, 2SLS-IV fails

- *DGP — nonignorable, heterogeneous treatment effect*

Y_1	Y_0	TE	Y	D	Z
10	5	5	10	1	1
10	5	5	5	0	0
10	5	10	10	1	1
10	5	10	10	1	0
10	5	0	10	1	1
10	5	0	5	0	0
10	-5	0	-5	0	1
10	-5	0	10	1	0

- regressors: $\{1, D\}$; instruments: $\{1, Z\}$
- compliers are rows 1, 2, 5, and 6
- last two rows are defiers

examples

LATE — defiers, 2SLS-IV fails

- various treatment effects

$$\begin{array}{rr} ATT = 7 & ATUT = 8\frac{1}{3} \\ ATE = 7.5 & OLS = 8\frac{1}{3} \\ LATE = 5 & IVE = -5 \end{array}$$

- the local average treatment effect is not identified via *2SLS-IV* as there are defiers in the population
- *2SLS-IV is grossly misleading — sign of the estimand IVE is opposite of LATE*
- failure of treatment adoption uniformity can mean *OLS* is closer to identifying *LATE* than *2SLS-IV*

examples

2SLS-IV with covariates — treatment effect depends on covariates as well as instrument choice

- let X be a fixed design matrix of three indicator variables (a varying intercept model)
- regressors (conditional effects): $\{X_1, X_2, X_3, X_1 D, X_2 D, X_3 D\}$ or regressors (unconditional effect): $\{X_1, X_2, X_3, D\}$; instruments: $\{X_1, X_2, X_3, X_1 Z, X_2 Z, X_3 Z\}$

examples

2SLS-IV with covariates — treatment effect depends on covariates as well as instrument choice

- $2SLS - IV$ effect is a weighted average of the $2SLS - IV$ effects identified at each $X_k = 1$

$$\begin{aligned}\gamma &= \frac{E\left[Y \cdot \left(E\left[D \mid X, Z\right] - E\left[D \mid X\right]\right)\right]}{E\left[D \cdot \left(E\left[D \mid X, Z\right] - E\left[D \mid X\right]\right)\right]} \\ &= \frac{E_X\left[\omega\left(X_k = 1\right)\gamma\left(X_k = 1\right)\right]}{E_X\left[\omega\left(X_k = 1\right)\right]}\end{aligned}$$

- with weights

$$\omega\left(X_k = 1\right) = E\left[E\left[D \mid X, Z\right] \cdot \left(E\left[D \mid X, Z\right] - E\left[D \mid X\right]\right) \mid X_k = 1\right]$$

- and $2SLS - IV$ effects at each $X_k = 1$,

$$\gamma\left(X_k = 1\right) = \frac{E\left[Y \cdot \left(E\left[D \mid X, Z\right] - E\left[D \mid X\right]\right) \mid X_k = 1\right]}{E\left[D \cdot \left(E\left[D \mid X, Z\right] - E\left[D \mid X\right]\right) \mid X_k = 1\right]}$$

examples

2SLS-IV with covariates — 2SLS-IV effect equal to LATE

- *DGP — nonignorable, heterogeneous treatment effect with unobservable outcome* V_i

Y_1	Y_0	TE	Y	D	X_1	X_2	X_3	V_1	V_0	Z
6	4	2	6	1	1	0	0	1	3	1
6	4	2	4	0	1	0	0	1	3	0
4	-2	6	4	1	1	0	0	-1	-3	1
4	-2	6	4	1	1	0	0	-1	-3	0
8	4	4	8	1	0	1	0	2	2	1
8	4	4	4	0	0	1	0	2	2	0
4	0	4	0	0	0	1	0	-2	-2	1
4	0	4	0	0	0	1	0	-2	-2	0
10	4	6	10	1	0	0	1	3	1	1
10	4	6	4	0	0	0	1	3	1	0
4	2	2	2	0	0	0	1	-3	-1	1
4	2	2	2	0	0	0	1	-3	-1	0

examples

2SLS-IV with covariates — 2SLS-IV effect equal to LATE

- compliers are rows 1, 2, 5, 6, 9, and 10
- various treatment effects

	treatment effects			
	conditional			
	$X_1 = 1$	$X_2 = 1$	$X_3 = 1$	unconditional
OLS	0.6667	6.6667	7.3333	4.1143
$LATE$	2	4	6	4
$2SLS - IV$	2	4	6	4
$\omega(X_k = 1)$	0.0625	0.0625	0.0625	
ATT	4.6667	4	6	4.8
$ATUT$	2	4	3.3333	3.4286
ATE	4	4	4	4

- the local average treatment effects are identified via *2SLS-IV* as $E[V_j Z \mid X_k = 1] = E[V_j \mid X_k = 1] = 0$

examples

2SLS-IV with covariates — 2SLS-IV effect unequal to LATE

- *DGP — nonignorable, heterogeneous treatment effect*

Y_1	Y_0	TE	Y	D	X_1	X_2	X_3	V_1	V_0	Z
6	4	2	6	1	1	0	0	1	3	1
6	4	2	4	0	1	0	0	1	3	0
8	0	8	8	1	1	0	0	3	-1	0
8	0	8	8	1	1	0	0	3	-1	0
8	4	4	8	1	0	1	0	2	2	1
8	4	4	4	0	0	1	0	2	2	0
6	-1	7	-1	0	0	1	0	0	-3	1
6	-1	7	-1	0	0	1	0	0	-3	1
4	4	0	4	1	0	0	1	-3	1	1
4	4	0	4	0	0	0	1	-3	1	0
4	1	3	1	0	0	0	1	-3	-2	0
4	1	3	1	0	0	0	1	-3	-2	0

examples

2SLS-IV with covariates — 2SLS-IV effect unequal to LATE

- compliers are rows 1, 2, 5, 6, 9, and 10
- various treatment effects

	treatment effects			
	conditional			
	$X_1 = 1$	$X_2 = 1$	$X_3 = 1$	*unconditional*
OLS	3.3333	7.3333	2	5.0857
LATE	2	4	0	2
$2SLS - IV$	-2	-6	2	0.9091
$\omega\left(X_k = 1\right)$	0.02083	0.02083	0.1875	
ATT	6	4	0	4.4
ATUT	2	6	2	3.7143
ATE	5	5.5	1.5	4

- *2SLS-IV* identifies a different effect than *LATE* as $E\left[V_j Z \mid X_k = 1\right] \neq 0$, $E\left[V_j \mid X_k = 1\right] \neq 0$ but $E\left[V_j Z\right] = E\left[V_j\right] = 0$

conclusions

- analysis of causal effects places a greater demand on explication of the thought experiments which underlie the data analysis
- in other words, framing the problem is key to understanding, critiquing, improving the data summaries