Part 8: Policy Evaluation- Local Average Treatment Effects

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Applied Econometrics

What about IV

So what does IV do?

- Let's assume a binary instrument $Z_i = 1$
- $Y_i(1), Y_i(0)$ depends on the value of T_i
- But now we endogenize $T_i(1), T_i(0)$ where the argument is the value of Z_i.
- We observe $\{Z_i, T_i = T_i(Z_i), Y_i = Y_i(T_i(Z_i))\}$.

IV Assumptions

So what does IV do?

Independence $Z_i \perp Y_i(1), Y_i(0), T_i(1), T_i(0)$. Instrument is as if randomly assigned and does not directly affect Y_i

This is not implied by random assignment. In that case there would be four potential outcomes $Y_i(z,t)$

Random Assignment $Z_i \perp Y_i(0,0), Y_i(0,1), Y_i(1,0), Y_i(1,1), T_i(1), T_i(0)$.

Exclusion Restriction $Y_i(z,t) = Y_i(z',t)$ for all z, z', t.

Thus we require both RA and ER to guarantee Independence. The second assumption is a substantive one.

We only observe (Z_i, T_i) not the pair $T_i(0), T_i(1)$ so we cannot determine compliance types directly! (See the picture)

IV Assumptions

	$T_i(0)$	
$T_i(1)$	0	1
0	never-taker	defier
1	complier	always-taker

IV Assumptions

We are stuck without further assumptions, so we assume:

Monotonicity/No Defiers $T_i(1) \geq T_i(0)$

- Works in many applications (classical drug compliance).
- Implied by many latent index models with constant coefficients
- Works as long as sign of $\pi_{1,i}$ doesn't change

$$T_i(z) = 1[\pi_0 + \pi_1 z + \varepsilon_i > 0]$$

IV Assumptions

Table 2: Compliance Type by Treatment and Instrument

		Z_i = 0	Z_i = 1
W_i	0	complier/never-taker	never-taker/defier
	1	always-taker/defier	complier/always-taker

IV Assumptions

Table 3: Compliance Type by Treatment and Instrument given Monotonicity

		Z_i = 0	Z_i = 1
W_i	0	complier/never-taker	never-taker
	1	always-taker	complier/always-taker

LATE Derivation

- We can derive the expression for β_{IV} as:

$$\beta_{IV} = \frac{E[Y_i|Z_i = 1] - E[Y_i|Z_i = 0]}{E[T_i|Z_i = 1] - E[T_i|Z_i = 0]} = E[Y_i(1) - Y_i(0)|complier]$$

- We can derive the expression for π_c (the fraction of compliers):

$$\pi_c = E[T_i|Z_i = 1] - E[T_i|Z_i = 0]$$

- Proof see Angrist and Imbens

How Close to ATE?

Angrist and Imbens give some idea how close to the ATE the LATE is:

- $E[Y_i(0)|\text{never-taker}]$ and $E[Y_i(1)|\text{always-taker}]$ can be estimated from the data
- Compare these to their respective compliers $E[Y_i(0)|\text{complier}]$, $E[Y_i(1)|\text{complier}]$.
- When these are close then possibly $ATE \approx LATE$.

How Close to ATE?

Angrist and Imbens give some idea how close to the ATE the LATE is:

$$\widehat{\beta}_1^{TSLS} \rightarrow^p \frac{E[\beta_{1i}\pi_{1i}]}{E[\pi_{i1}]} = LATE$$

$$LATE = ATE + \frac{Cov(\beta_{1i}, \pi_{1i})}{E[\pi_{1i}]}$$

- Weighted average for people with large π_{1i}.
- Late is treatment effect for those whose probability of treatment is most influenced by Z_i.
- If you always (never) get treated you don't show up in LATE.

How Close to ATE?

- With different instruments you get different π_{1i} and TSLS estimators!
- Even with two valid Z_1, Z_2
  - Can be influential for different members of the population.
  - Using Z_1, TSLS will estimate the treatment effect for people whose probability of treatment X is most influenced by Z_1
  - The LATE for Z_1 might differ from the LATE for Z_2
  - A J-statistic might reject even if both Z_1 and Z_2 are exogenous! (Why?).

Example: Cardiac Catheterization

- $Y_i =$ surival time (days) for AMI patients
- $X_i =$ whether patient received cadiac catheterization (or not) (intensive treatment)
- $Z_i =$ differential distance to CC hospital

$$\begin{aligned} SurvivalDays_i &= \beta_0 + \beta_{1i} CardCath_i + u_i \\ CardCath_i &= \pi_0 + \pi_{1i} Distance_i + v_i \end{aligned}$$

- For whom does distance have the great effect on probability of treatment?
- For those patients what is their β_{1i}?

Example: Cardiac Catheterization

- IV estimates causal effect for patients whose value of X_i is most heavily influenced by Z_i
    - Patients with small positive benefit from CC in the expert judgement of EMT will receive CC if trip to CC hospital is short (compliers)
    - Patients that need CC to survive will always get it (always-takers)
    - Patients for which CC would be unnecessarily risky or harmful will not receive it (never-takers)
    - Patients for who would have gotten CC if they lived further from CC hospital (hopefully don't see) (defiers)
- We mostly weight towards the people with small positive benefits.

Local Average Treatment Effect

So how is this useful?

- It shows why IV can be meaningless when effects are heterogeneous.
- It shows that if the monotonicity assumption can be justified, IV estimates the effect for a particular subset of the population.
- In general the estimates are specific to that instrument and are not generalisable to other contexts.
- As an example consider two alternative policies that can increase participation in higher education.
  - Free tuition is randomly allocated to young people to attend college ($Z_1 = 1$ means that the subsidy is available).
  - The possibility of a competitive scholarship is available for free tuition ($Z_1 = 1$ means that the individual is allowed to compete for the scholarship).

Local Average Treatment Effect

- Suppose the aim is to use these two policies to estimate the returns to college education. In this case, the pair $\{Y^1, Y^0\}$ are log earnings, the treatment is going to college, and the instrument is one of the two randomly allocated programs.
- First, we need to assume that no one who intended to go to college will be discouraged from doing so as a result of the policy (monotonicity).
- This could fail as a result of a General Equilibrium response of the policy; for example, if it is perceived that the returns to college decline as a result of the increased supply, those with better outside opportunities may drop out.

Local Average Treatment Effect

- Now compare the two instruments.
- The subsidy is likely to draw poorer liquidity constrained students into college but not necessarily those with the highest returns.
- The scholarship is likely to draw in the best students, who may also have higher returns.
- It is not a priori possible to believe that the two policies will identify the same parameter, or that one experiment will allow us to learn about the returns for a broader/different group of individuals.

Local Average Treatment Effect

Finally, we need to understand what monotonicity means in terms of restrictions on economic theory.

- To quote from Vytlacil (2002) Econometrica:
  "The LATE assumptions are not weaker than the assumptions of a latent index model, but instead impose the same restrictions on the counterfactual data as the classical selection model if one does not impose parametric functional form or distributional assumptions on the latter."
- This is important because it shows that the LATE assumptions are equivalent to whatever economic modeling assumptions are required to justify the standard Heckman selection model and has no claim to greater generality.
- On the other hand there are no magical solutions to identifying effects when endogeneity/selection is present; this problem is exacerbated when the effects are heterogeneous and individuals select into treatment on the basis of the returns.

Further approaches to evaluation of program effects:
Difference in Differences

- Sometimes we may feel we can impose more structure on the problem.
- Suppose in particular that we can write the outcome equation as

$$Y_{it} = \alpha_i + d_t + \beta_i T_{it} + u_{it}$$

- In the above we have now introduced a time dimension $t = \{1, 2\}$.
- Now suppose that $T_{i1} = 0$ for all i and $T_{i2} = 1$ for a well defined group of individuals in our population.
- This framework allows us to identify the ATT effect under the assumption that the growth of the outcome in the non-treatment state is independent of treatment allocation:

$$E[Y_{i2}^0 - Y_{i1}^0 | T] = E[Y_{i2}^0 - Y_{i1}^0]$$

Before and After

An even simpler estimator is the before and after or event study.

- We look an outcome before or after an event
  - A news event: the announcement of a merger or stock split.
  - A tax change, a new law, etc.

$$
\begin{aligned}
E[Y_{i2} - Y_{i1} | T_{i2} = 1] &= E[Y_{i2}^1 - Y_{i1}^1 | T_{i2} = 1] \\
&= d_2 - d_1 + E[\beta_i | T_{i2} = 1]
\end{aligned}
$$

- Except under strong conditions $d_2 = d_1$ we shouldn't believe the results of the before and after estimator.
- Main Problem: we attribute changes to treatment that might have happened anyway trend.
- e.g: Cigarette consumption drops 4% after a tax hike. (But it dropped 3% the previous four years).

Difference in Differences

Let's try and estimate $d_2 - d_1$ directly and then difference it out. Here we use parallel trends:

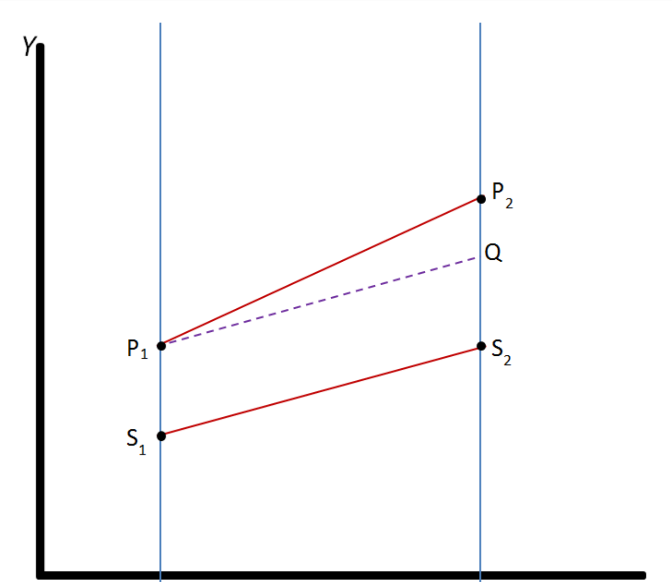
$$
\begin{aligned}
E[Y_{i2}^0 - Y_{i1}^0 | T_{i2} = 1] &= E[Y_{i2}^0 - Y_{i1}^0 | T_{i2} = 0] \\
E[Y_{i2} - Y_{i1} | T_{i2} = 0] &= d_2 - d_1
\end{aligned}
$$

We now obtain an estimator for ATT:

$$
E[\beta_i | T_{i2} = 1] = E[Y_{i2} - Y_{i1} | T_{i2} = 1] - E[Y_{i2} - Y_{i1} | T_{i2} = 0]
$$

which can be estimated by the difference in the growth between the treatment and the control group.

Parallel Trends



Now consider the following problem:

- Suppose we wish to evaluate a training program for those with low earnings. Let the threshold for eligibility be B.
- We have a panel of individuals and those with low earnings qualify for training, forming the treatment group.
- Those with higher earnings form the control group.
- Now the low earning group is low for two reasons
  1. They have low permanent earnings (α_i is low) - this is accounted for by diff in diffs.
  2. They have a negative transitory shock (u_{i1} is low) - this is not accounted for by diff in diffs.

Difference in Differences

- #2 above violates the assumption $E[Y^0_{i2} - Y^0_{i1}|T] = E[Y^0_{i2} - Y^0_{i1}]$.
- To see why note that those participating into the program are such that $Y^0_{i0} < B$. Assume for simplicity that the shocks u are iid. Hence $u_{i1} < B - \alpha_i - d_1$. This implies:

$$E[Y^0_{i2} - Y^0_{i1}|T = 1] = d_2 = d_1 - E[u_{i1}|u_{i1} < B - \alpha_i - d_1]$$

For the control group:

$$E[Y^0_{i2} - Y^0_{i1}|T = 1] = d_2 = d_1 - E[u_{i1}|u_{i1} > B - \alpha_i - d_1]$$

- Hence

$$E[Y^0_{i2} - Y^0_{i1}|T = 1] - E[Y^0_{i2} - Y^0_{i1}|T = 0] =$$
$$E[u_{i1}|u_{i1} > B - \alpha_i - d_1] - E[u_{i1}|u_{i1} < B - \alpha_i - d_1] > 0$$

- This is effectively regression to the mean: those unlucky enough to have a bad shock

Difference in Differences

Ashefelter (1978) was one of the first to consider difference in differences to evaluate training programs.

TABLE 1.—MEAN EARNINGS PRIOR, DURING, AND SUBSEQUENT TO TRAINING FOR 1964 MDTA CLASSROOM TRAINEES AND A COMPARISON GROUP

	White Males		Black Males		White Females		Black Females	
	Trainees	Comparison Group	Trainees	Comparison Group	Trainees	Comparison Group	Trainees	Comparison Group
1959	$1,443	$2,588	$ 904	$1,438	$ 635	$ 987	$ 384	$ 616
1960	1,533	2,699	976	1,521	687	1,076	440	693
1961	1,572	2,782	1,017	1,573	719	1,163	471	737
1962	1,843	2,963	1,211	1,742	813	1,308	566	843
1963	1,810	3,108	1,182	1,896	748	1,433	531	937
1964	1,551	3,275	1,273	2,121	838	1,580	688	1,060
1965	2,923	3,458	2,327	2,338	1,747	1,698	1,441	1,198
1966	3,750	4,351	2,983	2,919	2,024	1,990	1,794	1,461
1967	3,964	4,430	3,048	3,097	2,244	2,144	1,977	1,678
1968	4,401	4,955	3,409	3,487	2,398	2,339	2,160	1,920
1969	$4,717	$5,033	$3,714	$3,681	$2,646	$2,444	$2,457	$2,133
Number of Observations	7,326	40,921	2,133	6,472	2,730	28,142	1,356	5,192

Difference in Differences

Ashenfelter (1978) reports the following results.

TABLE 2.—CRUDE ESTIMATES (AND ESTIMATED STANDARD ERRORS), ASSUMING $B=0$ AND $\beta_j'=0$ FOR $j>1$, OF THE EFFECT OF TRAINING ON EARNINGS DURING AND AFTER TRAINING, WHITE MALE MDTA 1964 CLASSROOM TRAINEES

Effect in (value of t)	Value of Effects for $t-s=1963$	$t-s=1962$	$t-s=1961$
1962	—	—	91 (13)
1963	—	−179 (14)	−88 (17)
1964	−426 (16)	−605 (18)	−514 (20)
1965	763 (20)	584 (22)	675 (23)
1966	697 (25)	518 (27)	609 (28)
1967	833 (28)	655 (30)	746 (31)
1968	745 (34)	566 (35)	657 (36)

Difference in Differences

- The assumption on growth of the non-treatment outcome being independent of assignment to treatment may be violated, but it may still be true conditional on X.
- Consider the assumption

$$E[Y_{i2}^0 - Y_{i1}^0 | X, T] = E[Y_{i2}^0 - Y_{i1}^0 | X]$$

- This is just matching assumption on a redefined variable, namely the growth in the outcomes. In its simplest form the approach is implemented by running the regression

$$Y_{it} = \alpha_i + d_t + \beta_i T_{it} + \gamma_t' X_i + u_{it}$$

which allows for differential trends in the non-treatment growth depending on X_i. More generally one can implement propensity score matching on the growth of outcome variable when panel data is available.

Difference in Differences with Repeated Cross Sections

- Suppose we do not have available panel data but just a random sample from the relevant population in a pre-treatment and a post-treatment period. We can still use difference in differences.
- First consider a simple case where $E[Y^0_{i2} - Y^0_{i1}|T] = E[Y^0_{i2} - Y^0_{i1}]$.
- We need to modify slightly the assumption to

$$E[Y^0_{i2}|_{\text{Group receiving training}}] - E[Y^0_{i1}|_{\text{Group receiving training in the next period}}]$$
$$= E[Y^0_{i2} - Y^0_{i1}]$$

  which requires, in addition to the original independence assumption that conditioned on particular individuals that population we will be sampling from does not change composition.
- We can then obtain immediately an estimator for ATT as

$$E[\beta_i|T_{i2} = 1]$$

Difference in Differences with Repeated Cross Sections

- More generally we need an assumption of conditional independence of the form

$$
\begin{aligned}
E[Y_{i2}^0|X, \text{Group receiving training}] - E[Y_{i1}^0|X, \text{Group receiving training next period}] \\
= E[Y_{i2}^0|X] - E[Y_{i1}^0|X]
\end{aligned}
$$

- Under this assumption (and some auxiliary parametric assumptions) we can obtain an estimate of the effect of treatment on the treated by the regression

$$Y_{it} = \alpha_g + d_t + \beta T_{it} + \gamma' X_{it} + u_{it}$$

Difference in Differences with Repeated Cross Sections

- More generally we can first run the regression

$$Y_{it} = \alpha_g + d_t + \beta(X_{it})T_{it} + \gamma' X_{it} + u_{it}$$

  where α_g is a dummy for the treatment of comparison group, and $\beta(X_{it})$ can be parameterized as $\beta(X_{it}) = \beta' X_{it}$. The ATT can then be estimated as the average of $\beta' X_{it}$ over the (empirical) distribution of X.
- A non parametric alternative is offered by Blundell, Dias, Meghir and van Reenen (2004).

Difference in Differences and Selection on Unobservables

- Suppose we relax the assumption of *no selection* on unobservables.
- Instead we can start by assuming that

$$E[Y_{i2}^0|X,Z] - E[Y_{i1}^0|X,Z] = E[Y_{i2}^0|X] - E[Y_{i1}^0|X]$$

  where Z is an instrument which determines training eligibility say but does not determine outcomes in the non-training state. Take Z as binary (1,0).
- Non-Compliance: not all members of the eligible group ($Z = 1$) will take up training and some of those ineligible ($Z = 0$) may obtain training by other means.
- A difference in differences approach based on grouping by Z will estimate the impact of being allocated to the eligible group, but not the impact of training itself.

Difference in Differences and Selection on Unobservables

- Now suppose we still wish to estimate the impact of training on those being trained (rather than just the effect of being eligible)
- This becomes an IV problem and following up from the discussion of LATE we need stronger assumptions
  - Independence: for $Z = a, \{Y^0_{i2} - Y^0_{i1}, Y^1_{i2} - Y^1_{i1}, T(Z = a)\}$ is independent of Z.
  - Monotonicity $T_i(1) \geq T_i(0) \,\forall\, i$
- In this case LATE is defined by

$$[E(\Delta Y|Z = 1) - E(\Delta Y|Z = 0)]/[Pr(T(1) = 1) - Pr(T(0) = 1)]$$

assuming that the probability of training in the first period is zero.

RDD

Regression Discontinuity Design

- Another popular research design is the Regression Discontinuity Design.
- In some sense this is a special case of IV regression. (RDD estimates a LATE).
- Most of this is taken from the JEL Paper by Lee and Lemieux (2010).

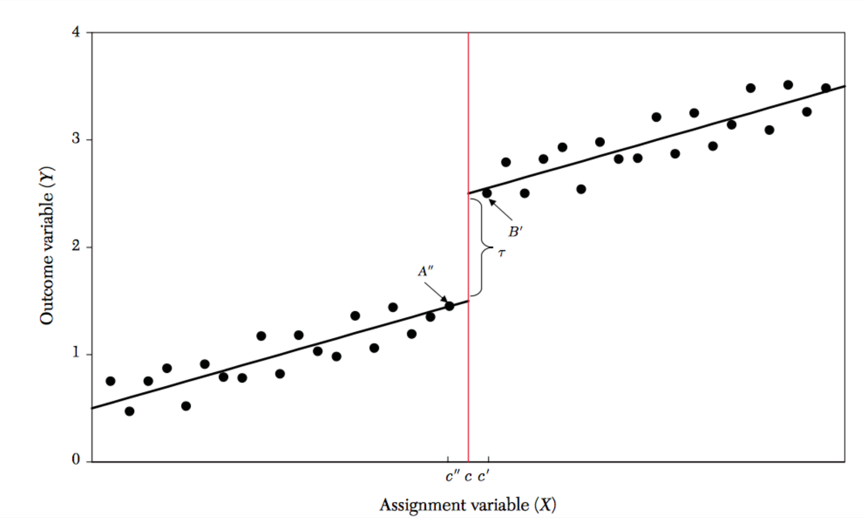
RDD: Basics

- We have a running or forcing variable x such that

$$\lim_{x \to c^+} P(T_i|X_i = x) \neq \lim_{x \to c^-} P(T_i|X_i = x)$$

- The idea is that there is a discontinuous jump in the probability of being treated.
- For now we focus on the sharp discontinuity:
  $P(T_i|X_i \geq c) = 1$ and $P(T_i|X_i < c) = 0$
- There is no single x for which we observe treatment and control. (Compare to Propensity Score!).
- The most important assumption is that of no manipulability $\tau_i \perp D_i$ in some neighborhood of c.
- Example: a social program is available to people who earned less than \$25,000.
  - If we could compare people earning \$24,999 to people earning \$25,001 we would have as-if random assignment. (MAYBE)
  - But we might not have that many people...

RDD: In Pictures



RDD: Sharp RD Case

RDD uses a set of assumptions distinct from our LATE/IV assumptions. Instead it depends on continuity.

- We need that $E[Y^{(1)}|X]$ and $E[Y^{(0)}|X]$ both be continuous at $X = c$.
- People just to the left of c are a valid control for those just to the right of c.
- This is not a testable assumption $\rightarrow$ draw pictures!
- We could run the regression where $D_i = \mathbf{1}[X_i > c]$.

$$Y_i = \beta_0 + \tau D_i + X_i \beta + \epsilon_i$$

- This puts a lot of restrictions (linearity) on the relationship between Y and X.
- Also (without additional assumptions) we only learn about τ_i at the point $X = c$.

RDD: Nonlinearity

First thing to relax is assumption of linearity.

$$Y_i = f(x_i) + \tau D_i + \epsilon_i$$

This is known as partially linear model.

- Two options for $f(x_i)$:
  1. Kernels: Local Linear Regression
  2. Polynomials: $Y_i = \beta_0 + \beta_1 x_i + \beta_2 x_i^2 + \cdots + \beta_p x^p + \tau D_i + \epsilon_i$.
     - Actually, people suggest different polynomials on each side of cutoff! (Interact everything with D_i).
- Same objective. Want to flexibly capture what happens on both sides of cutoff.
- Otherwise risk confusing nonlinearity with discontinuity!

RDD: Kernel Boundary Problem

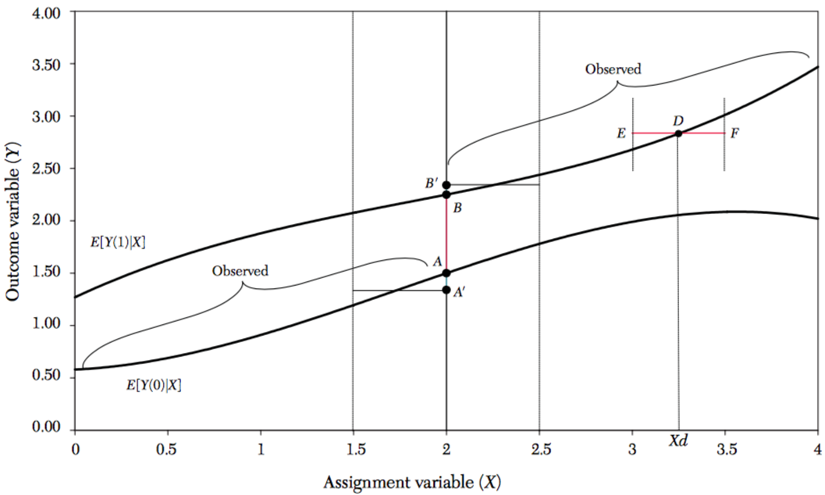


Figure 2. Nonlinear RD

RDD: Polynomial Implementation Details

To make life easier:

- replace $\tilde{x}_i = x_i - c$.
- Estimate coefficients β: $(1, \tilde{x}, \tilde{x}^2, \ldots, \tilde{x}^p)$ and $\tilde{\beta}$: $(D_i, D_i\tilde{x}, D_i\tilde{x}^2, \ldots, D_i\tilde{x}^p)$.
- Now treatment effect at c just the coefficient on D_i. (We can ignore the interaction terms).
- If we want treatment effect at $x_i > c$ then we have to account for interactions.
    - Identification away from c is somewhat dubious.
- Lee and Lemieux (2010) suggest estimating a coefficient on a dummy for each bin in the polynomial regression $\sum_k \phi_k B_k$.
    - Add polynomials until you can satisfy the test that the joint hypothesis test that $\phi_1 = \cdots \phi_k = 0$.
    - There are better ways to choose polynomial order...

RDD: Checklist

Most RDD papers follow the same formula (so should yours)

- Plot of $P(D|X)$ so that we can see the discontinuity
- Plot of $E[Y|X]$ so that we see discontinuity there also
- Plot of $E[W|X]$ so that we don't see a discontinuity in controls.
- Density of X (check for manipulation).
- Show robustness to different "windows"
- The OLS RDD estimates
- The Local Linear RDD estimates
- The polynomial (from each side) RDD estimates
- An f-test of "bins" showing that the polynomial is flexible enough.

Read Lee and Lemieux (2010) before you get started.

Application: Lee (2008)

Looked at incumbency advantage in the US House of Representatives

- Running variable was vote share in previous election
  - Problem of naive approach: good candidates get lots of votes!
  - Compare outcomes of districts with barely D to barely R.
- First we plot bin-scatter plots and quartic (from each side) polynomials.
- Discussion about how to choose bin-scatter bandwidth (CV).

Lee (2008)

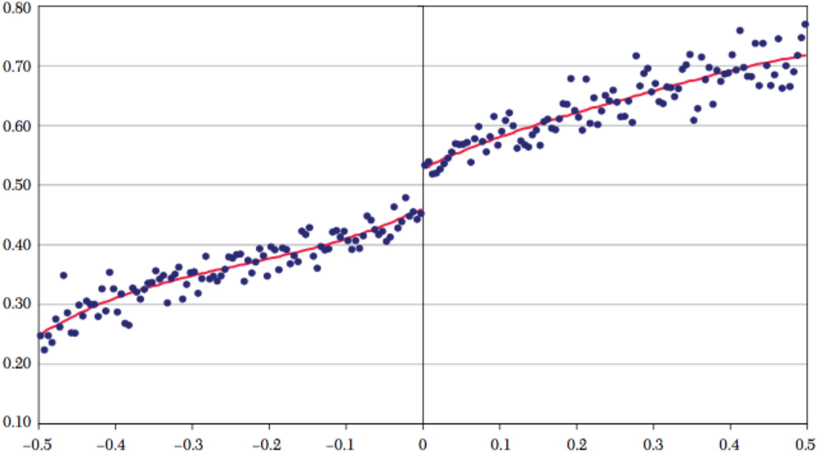


Figure 8. Share of Vote in Next Election, Bandwidth of 0.005 (200 bins)

Lee (2008)

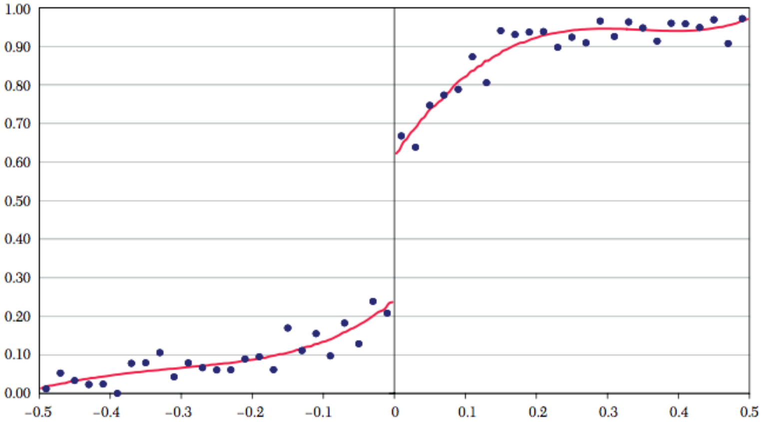


Figure 9. Winning the Next Election, Bandwidth of 0.02 (50 bins)

Lee (2008)

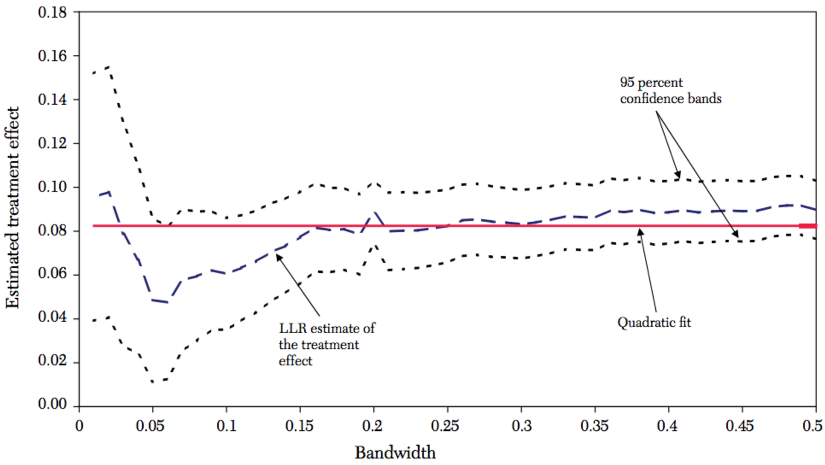


Figure 18. Local Linear Regression with Varying Bandwidth: Share of Vote at Next Election

Other Examples

Luca on Yelp

- Have data on restaurant revenues and yelp ratings.
- Yelp produces a yelp score (weighted average rating) to two decimals ie: 4.32.
- Score gets rounded to nearest half star
- Compare 4.24 to 4.26 to see the impact of an extra half star.
- Now there are multiple discontinuities: Pool them? Estimate multiple effects?

Fuzzy RD

An important extension in the Fuzzy RD. Back to where we started:

$$\lim_{x \to c^+} P(T_i | X_i = x) \neq \lim_{x \to c^-} P(T_i | X_i = x)$$

- We need a discontinuous jump in probability of treatment, but it doesn't need to be $0 \to 1$.

$$\tau_i(c) = \frac{\lim_{x \to c^+} P(Y_i | X_i = x) - \lim_{x \to c^-} P(Y_i | X_i = x)}{\lim_{x \to c^+} P(T_i | X_i = x) - \lim_{x \to c^-} P(T_i | X_i = x)}$$

- Under sharp RD everyone was a complier, now we have some always takers and some never takers too.
- Now we are estimating the treatment effect only for the population of compliers at $x = c$.

Related Idea: Kinks

A related idea is that of kinks.

- Instead of a discontinuous jump in the outcome there is a discontinuous jump in β_i on x_i.
- Often things like tax schedules or government benefits have a kinked pattern.

One quantity to rule them all: MTE

Heckman and Vytlacil provide a unifying non-parametric framework to categorize treatment effects. Their approach is known as the marginal treatment effect or MTE

- The MTE isn't a number it is a function.
- All of the other objects (LATE, ATE, ATT, etc.) can be written as integrals (weighted averages) of the MTE.
- The idea is to bridge the treatment effect parameters (stuff we get from running regressions) and the structural parameters: features of $f(\beta_i)$.

One quantity to rule them all: MTE

- Consider a treatment effect $\beta_i = Y_i(1) - Y_i(0)$.
- Think about a single-index such that $T_i = 1(v_i \leq Z_i'\gamma)$.
- Think about the person for whom $v_i = Z_i'\gamma$ (just barely untreated).

$$\Delta^{MTE}(X_i, v_i) = E[\beta_i | X_i, v_i = Z_i'\gamma]$$

- MTE is average impact of receiving a treatment for everyone with the same $Z'\gamma$.
- For any single index model we can rewrite

$$T_i = 1(v_i \leq Z_i'\gamma) = 1(u_{is} \leq F(Z_i'\gamma)) \text{ for } u_s \in [0,1]$$

- F is just the cdf of v_i
- Now we can write $P(Z) = Pr(T = 1|Z) = F(Z'\gamma)$.

MTE: Derivation

Now we can write,

$$
\begin{aligned}
Y_0 &= \gamma_0' X + U_0 \\
Y_1 &= \gamma_1' X + U_1
\end{aligned}
$$

$P(T = 1|Z) = P(Z)$ works as our instrument with two assumptions:

1. $(U_0, U_1, u_s) \perp P(Z)|X$. (Exogeneity)
2. Conditional on X there is enough variation in Z for $P(Z)$ to take on all values $\in (0, 1)$.
   - This is much stronger than typical relevance condition. Much more like the special regressor method we will discus next time.

MTE: Derivation

Now we can write,

$$
\begin{aligned}
Y &= \gamma_0' X + T(\gamma_1 - \gamma_0)' X + U_0 + T(U_1 - U_0) \\
E[Y|X, P(Z) = p] &= \gamma_0' X + p(\gamma_1 - \gamma_0)' X + E[T(U_1 - U_0)|X, P(Z) = p]
\end{aligned}
$$

Observe $T = 1$ over the interval $u_s = [0, p]$ and zero for higher values of u_s. Let $U_1 - U_0 \equiv \eta$.

$$
\begin{aligned}
E[T(U_1 - U_0)|P(Z) = p, X] &= \int_{-\infty}^{\infty} \int_0^p (U_1 - U_0) f((U_1 - U_0)|U_s = u_s) du_s d(U_1 - U_0) \\
E[T(\eta)|P(Z) = p, X] &= \int_{-\infty}^{\infty} \int_0^p \eta f(\eta|U_s = u_s) d\eta d u_s
\end{aligned}
$$

$$
\begin{aligned}
\Delta^{MTE}(p) &= \frac{\partial E[Y|X, P(Z) = p]}{\partial p} = (\gamma_1 - \gamma_0)' X + \int_{-\infty}^{\infty} \eta f(\eta|U_s = p) d\eta \\
&= (\gamma_1 - \gamma_0)' X + E[\eta|u_s = p]
\end{aligned}
$$

What is $E[\eta|u_s = p]$? The expected unobserved gain from treatment of those people who are on the treatment/no-treatment margin $P(Z) = p$.

How to Estimate an MTE

Easy

1. Estimate $P(Z) = Pr(T = 1|Z)$ nonparametrically (include exogenous part of X in Z).
2. Nonparametric regression of Y on X and $P(Z)$ (polynomials?)
3. Differentiate w.r.t. $P(Z)$
4. plot it for all values of $P(Z) = p$.

So long as $P(Z)$ covers $(0, 1)$ then we can trace out the full distribution of $\Delta^{MTE}(p)$.

Everything is an MTE

Calculate the outcome given (X, Z) (actually X and $P(Z) = p$).

- ATE : This one is obvious. We treat everyone!

$$\int_{-\infty}^{\infty} \Delta^{MTE}(p) = (\gamma_1 - \gamma_0)'X + \underbrace{\int_{-\infty}^{\infty} E(\eta|u_s)d\,u_s}_{0}$$

- LATE: Fix an X and $P(Z)$ varies from $b(X)$ to $a(X)$ and we integrated over the area between (compliers).

$$LATE(X) = \int_{-\infty}^{\infty} \Delta^{MTE}(p) = (\gamma_1 - \gamma_0)'X + \frac{1}{a(X) - b(X)} \int_{b(X)}^{a(X)} E(\eta|u_s)d\,u_s$$

- ATT

$$TT(X) = \int_{-\infty}^{\infty} \Delta^{MTE}(p) \frac{Pr(P(Z|X) > p)}{E[P(Z|X)]} d\,p$$

- Weights for IV and OLS are a bit more complicated. See the Heckman and Vytlacil paper(s).

Carneiro, Heckman and Vytlacil (AER 2010)

- Estimate returns to college (including heterogeneity of returns).
- NLSY 1979
- $Y = \log(wage)$
- Covariates X: Experience (years), Ability (AFQT Score), Mother's Education, Cohort Dummies, State Unemployment, MSA level average wage.
- Instruments Z: College in MSA at age 14, average earnings in MSA at 17 (opportunity cost), avg unemployment rate in state.

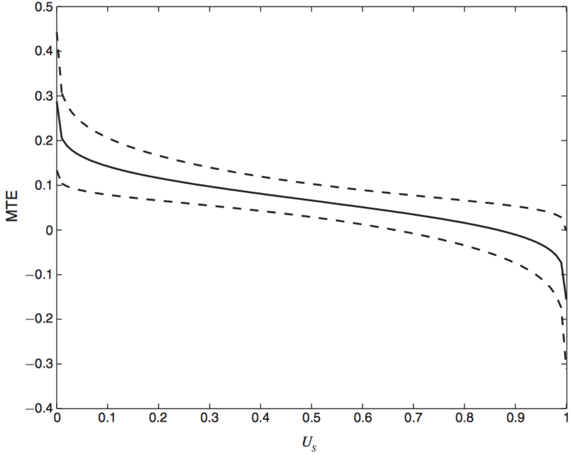


FIGURE 1. MTE ESTIMATED FROM A NORMAL SELECTION MODEL

Notes: To estimate the function plotted here, we estimate a parametric normal selection model by maximum likelihood. The figure is computed using the following formula:

TABLE 4— TEST OF LINEARITY OF $E(Y|\mathbf{X},P=p)$ USING POLYNOMIALS IN P; AND TEST OF EQUALITY OF LATEs OVER DIFFERENT INTERVALS (H_0: $LATE^j\,(U_S^{Lj},U_S^{Hj}) - LATE^{j+1}\,(U_S^{Lj+1},U_S^{Hj+1}) = 0$)

Panel A. Test of linearity of $E(Y|\mathbf{X},P=p)$ using models with different orders of polynomials in P[a]

Degree of polynomial for model	2	3	4	5
p-value of joint test of nonlinear terms	0.035	0.049	0.086	0.122
Adjusted critical value		0.057		
Outcome of test		Reject		

Panel B. Test of equality of LATEs (H_0: $LATE^j\,(U_S^{Lj},U_S^{Hj}) - LATE^{j+1}\,(U_S^{Lj+1},U_S^{Hj+1}) = 0$)[b]

Ranges of U_S for LATE^j	$(0, 0.04)$	$(0.08, 0.12)$	$(0.16, 0.20)$	$(0.24, 0.28)$	$(0.32, 0.36)$	$(0.40, 0.44)$
Ranges of U_S for LATE^{j+1}	$(0.08, 0.12)$	$(0.16, 0.20)$	$(0.24, 0.28)$	$(0.32, 0.36)$	$(0.40, 0.44)$	$(0.48, 0.52)$
Difference in LATEs	0.0689	0.0629	0.0577	0.0531	0.0492	0.0459
p-value	0.0240	0.0280	0.0280	0.0320	0.0320	0.0520
Ranges of U_S for LATE^j	$(0.48, 0.52)$	$(0.56, 0.60)$	$(0.64, 0.68)$	$(0.72, 0.76)$	$(0.80, 0.84)$	$(0.88, 0.92)$
Ranges of U_S for LATE^{j+1}	$(0.56, 0.60)$	$(0.64, 0.68)$	$(0.72, 0.76)$	$(0.80, 0.84)$	$(0.88, 0.92)$	$(0.96, 1)$
Difference in LATEs	0.0431	0.0408	0.0385	0.0364	0.0339	0.0311
p-value	0.0520	0.0760	0.0960	0.1320	0.1800	0.2400
Joint p-value				0.0520		

Carneiro, Heckman and Vytlacil

TABLE 5—RETURNS TO A YEAR OF COLLEGE

Model		Normal	Semiparametric
$ATE = E(\beta)$		0.0670 (0.0378)	Not identified
$TT = E(\beta \mid S = 1)$		0.1433 (0.0346)	Not identified
$TUT = E(\beta \mid S = 0)$		−0.0066 (0.0707)	Not identified
MPRTE			
Policy perturbation	Metric		
$Z^k_\alpha = Z^k + \alpha$	$\lvert \mathbf{Z}\gamma - V \rvert < e$	0.0662 (0.0373)	0.0802 (0.0424)
$P_\alpha = P + \alpha$	$\lvert P - U \rvert < e$	0.0637 (0.0379)	0.0865 (0.0455)
$P_\alpha = (1 + \alpha)P$	$\lvert \frac{P}{U} - 1 \rvert < e$	0.0363 (0.0569)	0.0148 (0.0589)
Linear IV (Using $P(\mathbf{Z})$ as the instrument)		0.0951 (0.0386)	
OLS		0.0836 (0.0068)	

Notes: This table presents estimates of various returns to college, for the semiparametric and the normal selection models: average treatment effect (ATE), treatment on the treated (TT), treatment on the untreated (TUT), and different versions of the marginal policy relevant treat-

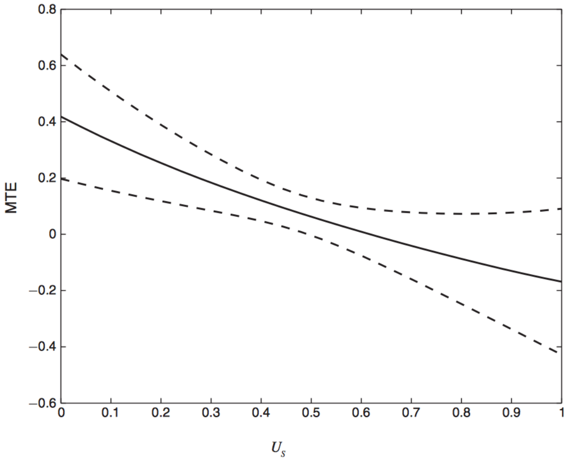


FIGURE 4. $E(Y_1 - Y_0 | \mathbf{X}, U_S)$ WITH 90 PERCENT CONFIDENCE INTERVAL—
LOCALLY QUADRATIC REGRESSION ESTIMATES

Notes: To estimate the function plotted here, we first use a partially linear regression of log wages on polynomials

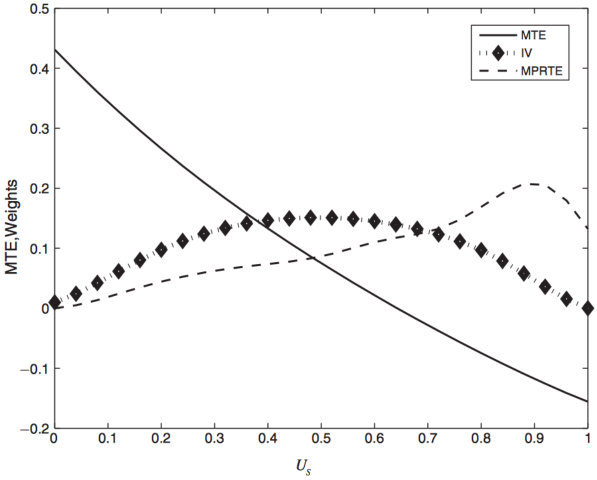


FIGURE 6. WEIGHTS FOR IV AND MPRTE

Note: The scale of the *y*-axis is the scale of the MTE, not the scale of the weights, which are scaled to fit the picture.

Diversion Example

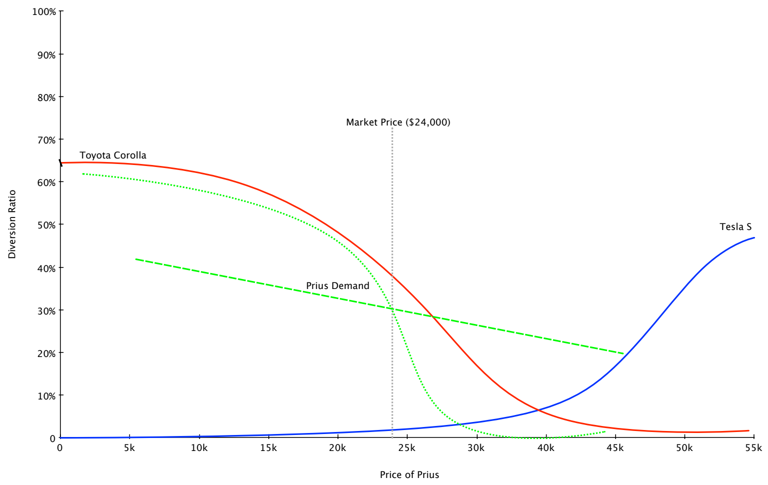
I have done some work trying to bring these methods into merger analysis.

- Key quantity: Diversion Ratio as I raise my price, how much do people switch to a particular competitor's product

$$D_{jk}(p_j, p_{-j}) = \left| \frac{\partial q_k}{\partial p_j}(p_j, p_{-j}) / \frac{\partial q_j}{\partial p_j}(p_j, p_{-j}) \right|$$

- We hold p_{-j} fixed and trace out $D_{jk}(p_j)$.
- The treatment is leaving good j.
- The Y_i is increased sales of good k.
- The Z_i is the price of good j.
- The key is that all changes in sales of k come through people leaving good j (no direct effects).

Diversion for Prius (FAKE!)



Diversion Example

$$\widehat{D_{jk}^{LATE}} = \frac{1}{\Delta q_j} \int_{p_j^0}^{p_j^0+\Delta p_j} \underbrace{\frac{\partial q_k(p_j, p_{-j}^0)}{\partial q_j}}_{\equiv D_{jk}(p_j, p_{-j}^0)} \left| \frac{\partial q_j(p_j, p_{-j}^0)}{\partial p_j} \right| dp_j$$

- $D_{jk}(p_j, p_{-j}^0)$ is the MTE.
- Weights $w(p_j) = \frac{1}{\Delta q_j} \frac{\partial q_j(p_j, p_{-j}^0)}{\partial p_j}$ correspond to the lost sales of j at a particular p_j as a fraction of all lost sales.
- When is $LATE \approx ATE$?
  - Demand for Prius is steep: everyone leaves right away
  - $D_{j,k}(p_j)$ is relatively flat.
  - We might want to think about raising the price to choke price (or eliminating the product from the consumers choice set) same as treating everyone!