Canonical Research Designs VI: Examiner Designs (aka Judge IV, aka leniency IV)

Paul Goldsmith-Pinkham

April 8, 2025

Roadmap for Today

- Today we're focusing on a particular research design: the examiner design
- Like many of the designs we are studying, they have shown up in papers for a while, but have really started to take off recently with the rise of high-quality administrative microdata.
- Key issues we'll touch on:
  - Identification: what are we getting at?
  - Estimation: What's the best estimation method for using the examiner design?
  - Inference: How should we do inference?

High-level description of examiner design

- In many applications, there is an administrator, judge, or monitor who plays an important role in deciding an outcome

- These outcomes include:
  - bail
  - banktrupcy
  - getting a loan
  - parole
  - disability insurance
  - patent granting
  - cancer screening

- In many cases, this examiner is effectively randomly assigned, *and* there is wide-range of differences (and discretion) in how likely they are to decide the outcome

High-level description of examiner design

- In many applications, there is an administrator, judge, or monitor who plays an important role in deciding an outcome

- These outcomes include:
  - bail
  - banktrupcy
  - getting a loan
  - parole
  - disability insurance
  - patent granting
  - cancer screening

- In many cases, this examiner is effectively randomly assigned, *and* there is wide-range of differences (and discretion) in how likely they are to decide the outcome

- Key ingredients:
  1. random assignment of examiner
  2. discretion over a (typically binary) outcome
  3. heterogeneity in behavior

Consider two examples

- Example 1: Bail setting in Philadelphia (Stevenson (2018))

- After you're arrested, you can be:
  - held in jail
  - released on bail (you pay $)
  - released on recognizance (no bail $)

- This decision is made by one of six magistrates at preliminary hearings
  - The particular magistrate that a defendant faces depends on a rotating schedule
  - Hence, the randomness occurs from *the time of day*

Distortion of Justice: How the Inability to Pay Bail Affects Case Outcomes

Megan T. Stevenson*
George Mason University

This article uses a natural experiment to analyze whether incarceration during the pretrial period affects case outcomes. In Philadelphia, defendants randomly receive bail magistrates who differ widely in their propensity to set bail at affordable levels. Using magistrate leniency as an instrument, I find that pretrial detention leads to a 13% increase in the likelihood of being convicted, an effect largely explained by an increase in guilty pleas among defendants who otherwise would have been acquitted or had their charges dropped. I find also that pretrial detention leads to a 42% increase in the length of the incarceration sentence and a 41% increase in the amount of nonbail court fees owed. This latter finding contributes to a growing literature on fines-and-fees in criminal justice, and suggests that the use of money bail contributes to a "poverty-trap": those who are unable to pay bail wind up accruing more court debt. (*JEL* K14)

> I have had the "you can wait it out or take the deal and get out" conversation with way too many clients.
>
> —a public defender, Philadelphia

Consider two examples

- Example 1: Bail setting in Philadelphia (Stevenson (2018))

- After you're arrested, you can be:
  - held in jail
  - released on bail (you pay $)
  - released on recognizance (no bail $)

- This decision is made by one of six magistrates at preliminary hearings
  - The particular magistrate that a defendant faces depends on a rotating schedule
  - Hence, the randomness occurs from *the time of day*

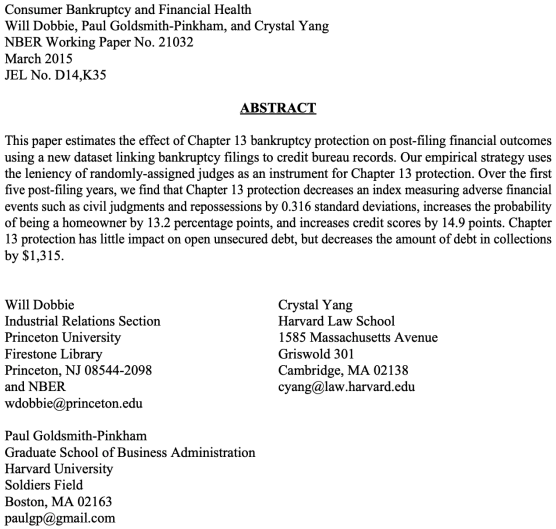
> Philadelphia employs six Arraignment Court Magistrates at a time, and one of the six will be on duty 24 hours a day, 7 days a week, including holidays. Each day is composed of three work shifts: graveyard (11:30 p.m.–7:30 a.m.), morning (7:30 a.m.–3:30 p.m.) and evening (3:30 p.m.–11:30 p.m.). Each magistrate will work for five days on a particular shift, take five days off, then do five days on the next shift, five days off, and so forth. For example, a magistrate may work the graveyard shift from January 1st to January 5th, have January 6th–10th off, then work the morning shift from January 11th–15th, have the 16th–20th off, do the evening shift from January 21st–25th, take the next five days off, and then start the cycle all over again.

Consider two examples

- Example 2: Bankruptcy judges in Chapter 13 (Dobbie et al.(2017))

- When you file for Chapter 13 bankruptcy, you file a repayment plan to be approved by a judge

- This decision is made by different judges staffed at a court on a given day
  - The assigned judge is done randomly by the court
  - Hence, the randomness occurs within *location-time* of filing



Consumer Bankruptcy and Financial Health
Will Dobbie, Paul Goldsmith-Pinkham, and Crystal Yang
NBER Working Paper No. 21032
March 2015
JEL No. D14,K35

ABSTRACT

This paper estimates the effect of Chapter 13 bankruptcy protection on post-filing financial outcomes using a new dataset linking bankruptcy filings to credit bureau records. Our empirical strategy uses the leniency of randomly-assigned judges as an instrument for Chapter 13 protection. Over the first five post-filing years, we find that Chapter 13 protection decreases an index measuring adverse financial events such as civil judgments and repossessions by 0.316 standard deviations, increases the probability of being a homeowner by 13.2 percentage points, and increases credit scores by 14.9 points. Chapter 13 protection has little impact on open unsecured debt, but decreases the amount of debt in collections by $1,315.

Will Dobbie
Industrial Relations Section
Princeton University
Firestone Library
Princeton, NJ 08544-2098
and NBER
wdobbie@princeton.edu

Crystal Yang
Harvard Law School
1585 Massachusetts Avenue
Griswold 301
Cambridge, MA 02138
cyang@law.harvard.edu

Paul Goldsmith-Pinkham
Graduate School of Business Administration
Harvard University
Soldiers Field
Boston, MA 02163
paulgp@gmail.com

Consider two examples

- Example 2: Bankruptcy judges in Chapter 13 (Dobbie et al.(2017))

- When you file for Chapter 13 bankruptcy, you file a repayment plan to be approved by a judge

- This decision is made by different judges staffed at a court on a given day
  - The assigned judge is done randomly by the court
  - Hence, the randomness occurs within *location-time* of filing

Bankruptcy judges are federal judges appointed to 14-year terms by the Court of Appeals in their judicial district. There are a total of 94 federal bankruptcy courts in the United States, including at least one bankruptcy court in each state, the District of Columbia, and Puerto Rico. Each bankruptcy court hears all cases originating from counties in its jurisdiction, and are often further divided into offices that hear all cases originating from a subset of counties in the court's jurisdiction. Bankruptcy judges often hear cases across multiple offices within their court, but only hear cases filed in their bankruptcy court. These cases are typically assigned to judges using a random number generator or a blind rotation system within each office.[8]

Notation

- Before we get to intuition, let's start with some notation
- We consider n individuals indexed by i, with two outcomes: D_i and Y_i
- Each individual is assigned to one of K examiners: $Q_i \in \{0, \ldots, K-1\}$
- Hence, very easy to consider the potential outcomes $D_i(q)$ for each of the potential exmainers, where we observe only one!
  - If $K = 2$, this is just our simple binary case.
  - With $K > 2$, it becomes more complicated
  - Note that there's no meaningful ordering to the K exmainers
- We can *also* consider the potential outcomes $Y_i(q)$. What are we ignoring?

Notation

- Before we get to intuition, let's start with some notation
- We consider n individuals indexed by i, with two outcomes: D_i and Y_i
- Each individual is assigned to one of K examiners: $Q_i \in \{0, \ldots, K-1\}$
- Hence, very easy to consider the potential outcomes $D_i(q)$ for each of the potential exmainers, where we observe only one!
  - If $K = 2$, this is just our simple binary case.
  - With $K > 2$, it becomes more complicated
  - Note that there's no meaningful ordering to the K exmainers
- We can *also* consider the potential outcomes $Y_i(q)$. What are we ignoring?
  - Potential impact of D_i on Y_i. When we do IV, we'll need to consider $Y_i(D_i(Q_i), Q_i)$, and then shut down the direct effect of Q_i in order to do IV

Intuition

- First consider the variation in D_i across the Q_i.
  - Conditional on some covariates W_i, we assume that strong ignorability holds
- If W_i is just a constant, then we can consider $\tau_{q,q'} = E(D_i|Q_i = q) - E(D_i|Q_i = q')$
  - This is the relative effect of judge q vs. q' on bail decisions or bankruptcy discharge
- We have an RCT, but with judges!
- Useful to also define $\mu_D(q) = E(D_i|Q_i = q)$
  - $\hat{\mu}_D(q)$ is the corresponding empirical estimate

What is this measure?

- In the context of bankruptcy or bail judges, $\mu_D(q)$ measures judge q's *leniency* – e.g. their average propensity

- We can literally estimate this with a simple linear regression of dummies! E.g. let $\mathcal{Q}_i$ be the set of dummies for Q_i:

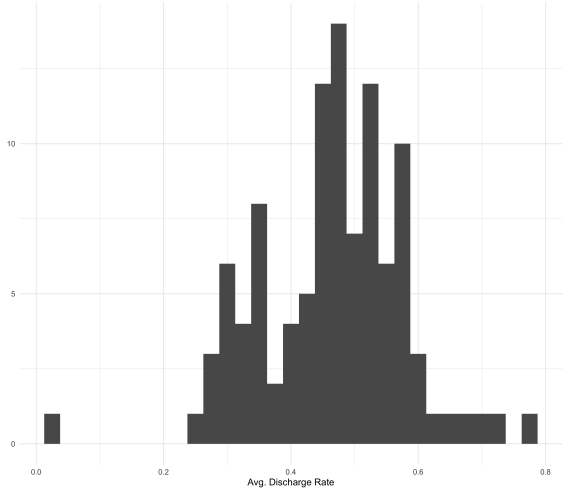
$$D_i = \mathcal{Q}_i \mu_D + u_i$$

- Note that given the simplicity of this measure, the μ_D are equivalent to the predicted values for D_i
  - E.g. $\hat{D}$ from the regression on $\mathcal{Q}_i$

- This predicted measure dovetails exactly into the measure that papers have historically used!

What's the instrument that people have used?

- In many papers, the measure that has been used is a "leniency" metric for judges: e.g. the $\hat{D}_i$
- This is the overall average leniency across judges in DGPY [note, not observation weighted – hence outliers]
- What's the problem with just the overall, in the DGPY context?
  - Judges are not random overall – location (and time) specific
- Importantly, the average judge characteristic may capture location specific features
  - Control for W_i



How to incorporate W_i?

- Now consider the simple linear regression with W_i:

$$D_i = \mathcal{Q}_i \mu_D + W_i \gamma + u_i$$

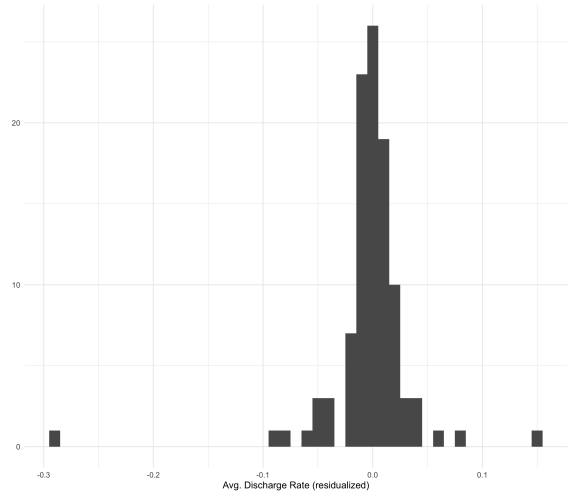
- Now, the μ_D are equivalent to the predicted values for D_i *after residualizing out for* W_i (which usually includes a constant)
  - E.g. $\hat{D}^{\perp}$ from the regression on $\mathcal{Q}_i^{\perp}$
  - Call this measure $Z_i = \hat{D}^{\perp}$ our leniency measure

- This captures the average variation in leniency within the average of a location
  - In the simple case where judges are nested within courts, this just captures variation across Q due to judge specific random variation
  - Mechanically, this would literally capture

$$\hat{D}^{\perp} = n^{-1} \left(\underbrace{\sum_i 1(Q_i = q) D_i}_{\text{judge mean}} - \underbrace{\sum_i 1(W_i = w) D_i}_{\text{location mean}} \right)$$

What's the instrument that people have used?

- Once you do this exercise with judge leniency, you get a much more recentered object
- This captures across location differences, and give true variation
- Point worth noting – due to the location effect, we can't estimate the "true" judge leniency – just relative leniency within an office

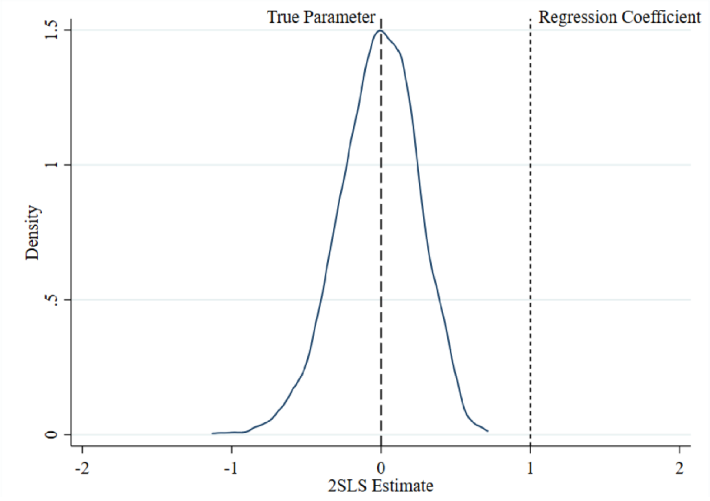


Important note: Leave-out

- In practice, individuals use the “leave-one-out” mean, rather than the actual average
- Why? Because if you include your own observation, that will be endogeneously correlated
- The “leave-one-out” leniency is the mechanical solution that deals with the many instrument problem

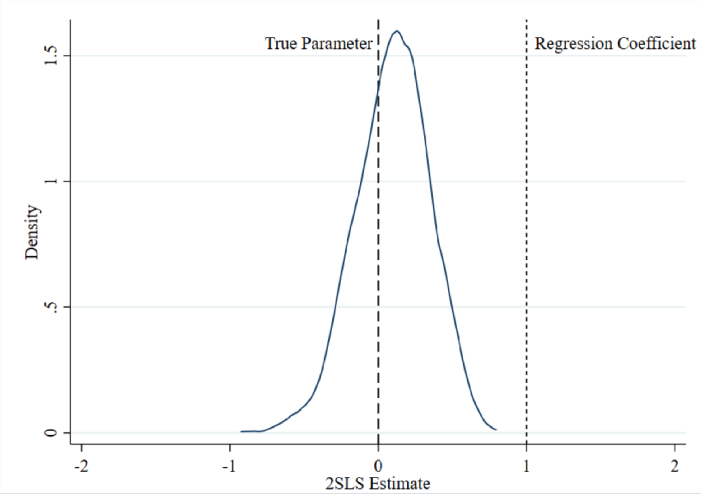
Recall Many IV bias is pernicious

Monte Carlo: $Y_i = \varepsilon_i$, $D_i = \Pi Z_{i1} + \eta_i$: IV with one Z_{i1}



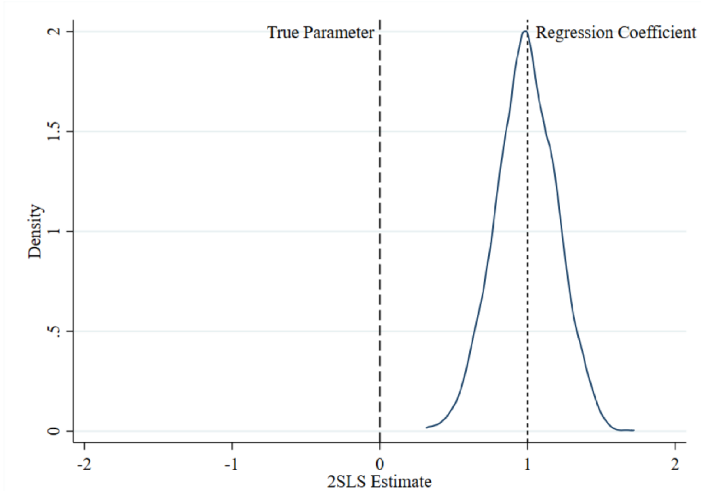
Recall Many IV bias is pernicious

Monte Carlo: $Y_i = \varepsilon_i$, $D_i = \Pi Z_{i1} + \eta_i$: IV with ten Z_{ij}



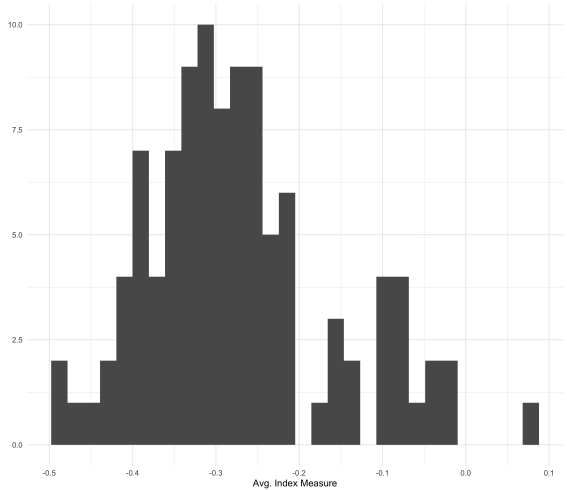
Recall Many IV bias is pernicious

Monte Carlo: $Y_i = \varepsilon_i$, $D_i = \Pi Z_{i1} + \eta_i$: IV with 100 Z_{ij}



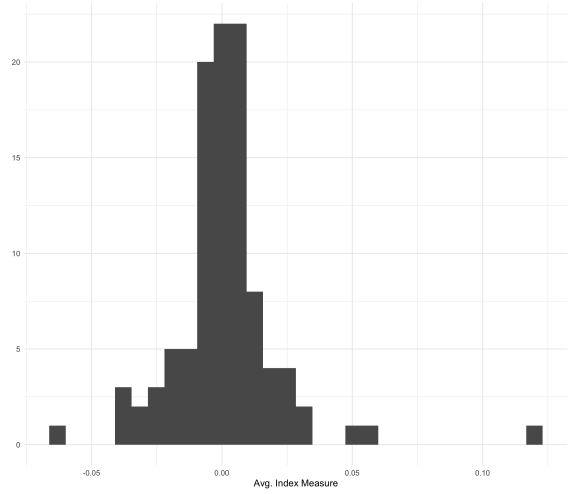
Nothing stops us from doing the same with outcomes!

- We can do the same exercise with our outcome measures!
- Nothing changes in this setting – we have both location and judge effects, and we can see differences if we don't account for them
- Location variation absorbs a large chunk, but we still see variation in our outcome caused by variation in judges



Nothing stops us from doing the same with outcomes!

- We can do the same exercise with our outcome measures!
- Nothing changes in this setting – we have both location and judge effects, and we can see differences if we don't account for them
- Location variation absorbs a large chunk, but we still see variation in our outcome caused by variation in judges



Thinking about instrumental variables

- The reason that this is approach is used, of course, is to use the variation in $\mu_D(q)$ to identify the effect of D on Y

- So what do we need? Recall
  1. relevance – e.g. we need that our instrument is predictive of D
  2. exclusion – e.g. we need that the examiner *only* has an effect on Y e.g. potential outcomes $Y_i(D_i(Q_i), Q_i) = Y_i(D_i(Q_i))$
  3. monotonicity – e.g. we need an ordering in the effects

- The last two are the challenging part, and not inherently testable. Let's discuss the issues, but first, how is this done in practice?

How is this done in practice, usually?

- In many papers (including my own, historically), the *leniency* measure Z_i has been used, rather than the dummies for judges

- Why? A number of reasons:
  - Faster – one time calculation vs. over identifed 2sls
  - Visualization – plotting the reduced form and first stage against leniency is very intuitive
  - Worry about first stage power is easier in the just identified case

- But note the following equivalency from our GMM/2SLS estimator:

$$\hat{\beta}_{2SLS} = \frac{D'\mathcal{Q}(\mathcal{Q}'\mathcal{Q})^{-1}\mathcal{Q}'Y}{D'\underbrace{\mathcal{Q}(\mathcal{Q}'\mathcal{Q})^{-1}\mathcal{Q}'}_{P_{\mathcal{Q}}}D} = \frac{\hat{D}'Y}{\hat{D}'D} \tag{1}$$

- Using many instruments and using the predicted first stage as your instrument are the same thing (adding controls just adds residualization)

So which is it?

- Our variation is really the random assignment of K judges. Collapsing to a predicted first stage doesn't change this, and if anything masks the experiement
- The estimator is exactly the same (or close, once we deal with some estimation issues)
  - If this were the right approach, we could do this in every overidentified setting!
- The issue is with inference – e.g. how much uncertainty is there in the underlying projections?
  - Consider the real line with the $\mu_D(q)$ and $\hat{\mu}_D(q)$. Focusing on the estimated leniency measure ignores potential important underlying variation in the first stage estimates
- In some cases, using the overidentified approach and the leniency just-identified approach give very similar standard errors – this is due to relative precision in estimates in the μ, and is not guaranteed!

An important reason to like leniency measures

- The biggest reason why researchers used the leniency measure is it gave a natural way to deal with the "own-observation" problem

- Note that $\hat{\mu}_D(q)$ includes indivudal *i*'s observation in the estimation procedure, scaled by n^{-1}
  - This term is endogeneous!

- Leniency measures that researchers construct use a "leave-one-out" mean to account for this, and instead measure an individual's leniency exposure as a judge's leniency *excluding* own observation

- This variable was easily plugged into 2SLS and avoided the bias

- But... this approach just approximates jackknife IV!
  - This own observation issue is exactly the bias that came up from overidentified IV in finite samples
  - When the number of judges is large-ish within a court, this can be an issue

Use (U)JIVE!

- Given the leniency approach is exactly solved with a known technique, there is no great reason to use leniency directly
- It is more transparent to use the judge variation directly
- If you want to do graphical visualization, just use the first stage coefficients!
- A key issue raised in Kolesar (2013) is the issue of many controls (e.g. many fixed effects) which creates the same problem as many instruments under certain settings
  - Provides a solution, through UJIVE. See his website for code.

How to test these exclusion and monotonoicity?

- Testing exclusion is always challenging
- However, like standard treatment designs and RCTs, we can test for balance
- Use the predicted first stage (e.g. the propensity score) and test of excluded covariates across p-scores $\hat{\mu}_D(q)$
- See Aronow and Miller for discusion on balance tests

Table 1
Descriptive Statistics and Randomization Balance

	All Credit Users		Judge Sample		
	Full Sample	Bankruptcy Filers	Harsh Judge	Lenient Judge	p-value
Panel A: Judge Leniency	(1)	(2)	(3)	(4)	(5)
Judge Leniency	-	-	-0.013	0.012	0.000
Panel B: Baseline Characteristics					
Age	48.549	43.699	44.843	44.863	0.229
Homeowner	0.470	0.520	0.668	0.643	0.175
Panel C: Baseline Financial Events					
Delinquency	0.148	0.413	0.681	0.675	0.962
Collection	0.137	0.296	0.460	0.467	0.897
Charge-off	0.065	0.188	0.308	0.310	0.630
Bankruptcy	0.010	0.007	0.046	0.048	0.318
Judgment	0.009	0.034	0.067	0.060	0.403
Foreclosure	0.003	0.010	0.055	0.048	0.632
Lien	0.004	0.011	0.021	0.021	0.445
Repossession	0.003	0.012	0.022	0.020	0.491

How to test these exclusion and monotonoicity?

- Testing (and believing) monotonicity is also challenging

EXAMPLE 2 (Administrative Screening):[5] Suppose applicants for a social program are screened by two officials. The two officials are likely to have different admission rates, even if the stated admission criteria are identical. Since the identity of the official is probably immaterial to the response, it seems plausible that Condition 1 is satisfied. The instrument is binary so Condition 3 is trivially satisfied. However, Condition 2 requires that if official A accepts applicants with probability $P(0)$, and official B accepts people with probability $P(1) > P(0)$, official B must accept *any* applicant who would have been accepted by official A. This is unlikely to hold if admission is based on a number of criteria. Therefore, in this example we *cannot* use Theorem 1 to identify a local average treatment effect nonparametrically despite the presence of an instrument satisfying Condition 1.

How to test these exclusion and monotonoicity?

- Testing (and believing) monotonicity is also challenging
- Kitagawa (2015) and Frandsen et al. (2019) discuss ways to test this for binary outcomes
- Current limitation is finite sample approximation
- However, provide useful intuititon for tests

Judging Judge Fixed Effects
Brigham R. Frandsen, Lars J. Lefgren, and Emily C. Leslie
NBER Working Paper No. 25528
February 2019
JEL No. C26,K14

ABSTRACT

We propose a test for the identifying assumptions invoked in designs based on random assignment to one of many "judges." We show that standard identifying assumptions imply that the conditional expectation of the outcome given judge assignment is a continuous function with bounded slope of the judge propensity to treat. The implication leads to a two-part test that generalizes the Sargan-Hansen overidentification test and assesses whether implied treatment effects across the range of judge propensities are possible given the domain of the outcome. We show the asymptotic validity of the testing procedure, demonstrate its finite-sample performance in simulations, and apply the test in an empirical setting examining the effects of pre-trial release on defendant outcomes in Miami. When the assumptions are not satisfied, we propose a weaker average monotonicity assumption under which IV still converges to a proper weighted average of treatment effects.

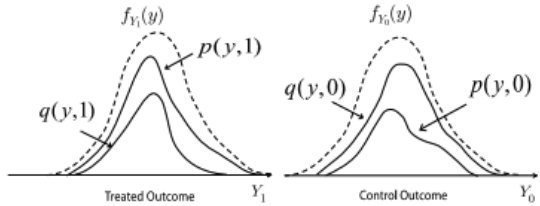
Brigham R. Frandsen
Department of Economics
Brigham Young University
Provo, UT 84602
frandsen@byu.edu

Lars J. Lefgren
Department of Economics
Brigham Young University
130 Faculty Office Building
Provo, UT 84602
and NBER
lars_lefgren@byu.edu

Emily C. Leslie
Department of Economics
Brigham Young University
435 Crabtree Technology Building
Provo, UT 84602
emily.c.leslie@gmail.com

Kitagawa (2015) result

- Consider binary endogeneous treatment D and a discrete instrument Z.
- Joint *testable* assumption: instrument is valid and monotonicity. Why?
  - Consider
    $p(y,d) = Pr(Y = y, D = d|Z = 1)$,
    $q(y,d) = Pr(Y = y, D = d|Z = 0)$
  - Let P and B be the probability over sets
  - Imbens and Rubin (1997) show
    $P(B,1) - Q(B,1) = Pr(Y_1 \in B, D_1 > D_0)$
    $P(B,0) - Q(B,0) = Pr(Y_0 \in B, D_1 > D_0)$

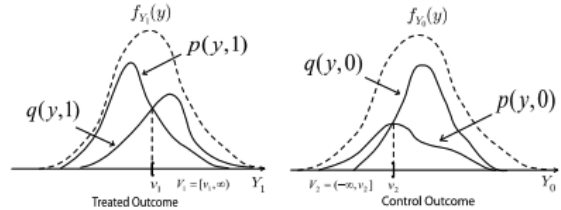


- Testable implication:

$$P(B,1) - Q(B,1) \geq 0$$
$$P(B,0) - Q(B,0) \geq 0$$

Kitagawa (2015) result

- Consider binary endogeneous treatment D and a discrete instrument Z.
- Joint *testable* assumption: instrument is valid and monotonicity. Why?
  - Consider
    $p(y,d) = Pr(Y = y, D = d|Z = 1)$,
    $q(y,d) = Pr(Y = y, D = d|Z = 0)$
  - Let P and B be the probability over sets
  - Imbens and Rubin (1997) show
    $P(B,1) - Q(B,1) = Pr(Y_1 \in B, D_1 > D_0)$
    $P(B,0) - Q(B,0) = Pr(Y_0 \in B, D_1 > D_0)$



- Testable implication:

$$P(B,1) - Q(B,1) \geq 0$$
$$P(B,0) - Q(B,0) \geq 0$$

Extension to Examiner designs (Frandsen et al.)

- Now, with multiple judges, we don't know the "true" z values
- Need to instead consider how this moves with noise
- Mapping from judges to "leniency" and then use Kitagawa test
- Package available: `testjfe` in Stata

What to do if test fails? (Frandsen et al.)

- If monotonicity fails in the strict sense, can we rescue the result?
-

THEOREM 3: *Suppose random assignment holds and define*

$$\omega_i \coloneqq \sum_{j=1}^{J} \lambda_j (p_j - p)\left[D_i(j) - \bar{D}_i\right],$$

and

$$\gamma_{ij} \coloneqq \gamma_{ij}(0)\left[1 - D_i(j)\right] + \gamma_{ij}(1) D_i(j),$$

where

$$p \coloneqq \sum_{j=1}^{J} \lambda_j p_j.$$

Then,

$$\beta_{2SLS} = \frac{E\left[\omega_i\left(\bar{Y}_i(1) - \bar{Y}_i(0)\right)\right]}{E[\omega_i]} + \frac{E\left[\sum_{j=1}^{J} \lambda_j (p_j - p)\gamma_{ij}\right]}{\sum_{j=1}^{J} \lambda_j (p_j - p)^2}.$$

CONDITION 2 (Average monotonicity): $\omega_i = \sum_{j=1}^{J} \lambda_j (p_j - p)\left[D_i(j) - \bar{D}_i\right] \geq 0$ *almost surely.*

CONDITION 3 (Average exclusion): $E\left[\sum_{j=1}^{J} \lambda_j (p_j - p)\gamma_{ij}\right] = 0.$

Brief aside on inference

- The many instruments field is still working on heteroskedasticty and complicated inference settings
- This stuff is challenging!
- However, given that the random assignment is effectively a set of judges to a given individual, robust standard errors seems most appropriate
- However, the norm (requested by referees, etc.) appears to be judge clustering
  - Not totally clear why

Further Readings

- Cunningham (2021) chapter 7.8.2
- Judging Judge Fixed Effects (2020)
- Mueller-Smith (2015)
- Dobbie and Song (2015)