Interference, Spillovers and Dynamics

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Final day of Part 1

- Our discussions on identification in the potential outcomes framework have had several simplifying assumptions, which we will relax today.
  - Binary scalar treatment
  - Single time period (e.g. one treatment within the person)
  - SUTVA – Stable Unit Treatment Value Assignment

Extending PO into multi-valued treatments

- For simplicities' sake, we have focused on binary treatments so far. What if it's not?

- Let's start with a discrete, multi-valued treatment to start: $D_i \in \{0, 1, \ldots, d\}$. You can rescale, etc. How should we consider the effects from this treatment?

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- Let's start with a discrete, multi-valued treatment to start: $D_i \in \{0, 1, \ldots, d\}$. You can rescale, etc. How should we consider the effects from this treatment?
- This is straightforward! And all we need is the strong ignorability condition (the second term of SI is slightly more convoluted but intuitive)
- What if we have covariates? Or if we do linear regression here?

$$\begin{aligned} \tau_i(d, d') &= Y_i(d) - Y_i(d') \\ E(\tau_i(d, d')) &= E(Y_i(d) - Y_i(d')) \\ &= E(Y_i|D_i = d) - E(Y_i|D_i = d') \end{aligned}$$

Extending PO into multi-valued treatments - covariates and regression

- Recall that our different approaches impute counterfactuals. If we run a regression of

$$Y_i = \tau D_i + \gamma X_i + \epsilon_i,$$

we're asserting two things:

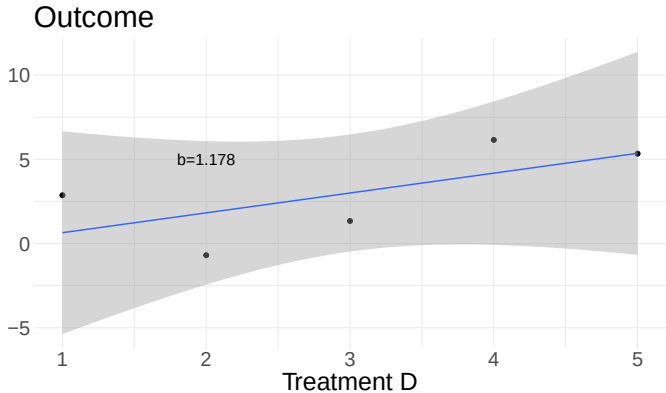
1. A linear approximation to the conditional expectation
2. A linear relationship between Y_i and D_i!

When might we do that?

- The choice of approximating the effect of D on Y wtih a particular functional form can come from many places:
  1. Data limitations: a linear model will be more accurately estimated (subject to the caveat that the functional form is right!)
  2. External validity: You may want to consider values of d that are not in the treatment set

Extending PO into multi-valued treatments - covariates and regression

- Let's make this concrete. Consider the following simulated data, where the effect is linear (just simulated such that $E(\tau_i(d, d') = d' - d$). (strict ignorability holds)



Extending PO into multi-valued treatments - covariates and regression

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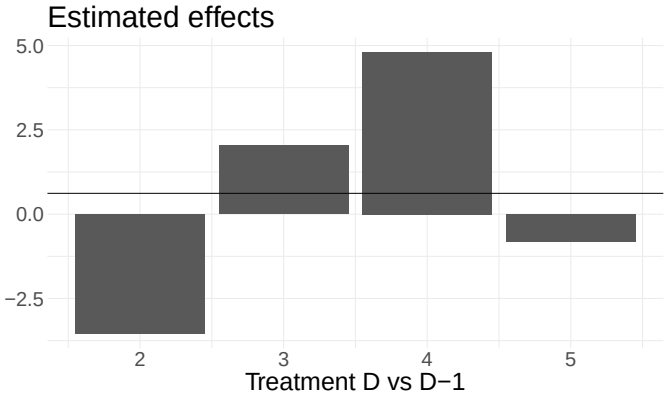
- Imposing model helps *a lot* compared to non-parametric form
  - But what do we do once we start thinking about controls?
  - But if we're wrong about the model?



Extending PO into multi-valued treatments

- This is all a relatively familiar problem in econometrics
  - Substantial structural work testing different parametric forms
  - Non-parametric work dominates parametric work in terms of "reducing assumptions" but is extremely data hungry
  - Once the dimension of any control variables is high, restrictions on the models will be necessary, especially with multivalued treatments

- We'll discuss some of these *estimation* issues later, but key issue today is thinking about identification
  - Non-parametric version was feasible because strong ignorability and SUTVA across individuals

Extending PO into multi-valued treatments - multiple treatments

- Important note so far: D was an *ordered* multivalued treatment.
- What if now, $\mathbf{D}_i \in \{0,1\}^2$ – two binary treatments (you could encode this as a multivalued treatment, and then the ordering is likely meaningless)
- How should we model this? The most natural way is $Y_i(D_{i1}, D_{i2})$ – each treatment flexibly affecting the outcome

$Y_i(\mathbf{D_i})$	$D_{1i}=0$	$D_{1i}=1$
$D_{2i}=0$	$Y_i(0,0)$	$Y_i(1,0)$
$D_{2i}=1$	$Y_i(0,1)$	$Y_i(1,1)$

- What is our estimand now?
- Let $\tau(\mathbf{d}, \mathbf{d}') = E(Y_i(d_1, d_2) - Y_i(d_1', d_2'))$
- So many choices! What is the most relevant estimand? What exploits the most amount of data?
- Most important: *what is identifiable*?

Intuition building with multi-valued treatments

- What is an example of $\tau(\mathbf{d}, \mathbf{d}')$ that would not be identifiable, even if $\mathbf{d}$ is randomly assigned?

Intuition building with multi-valued treatments

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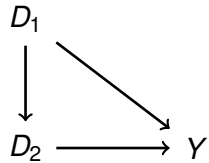
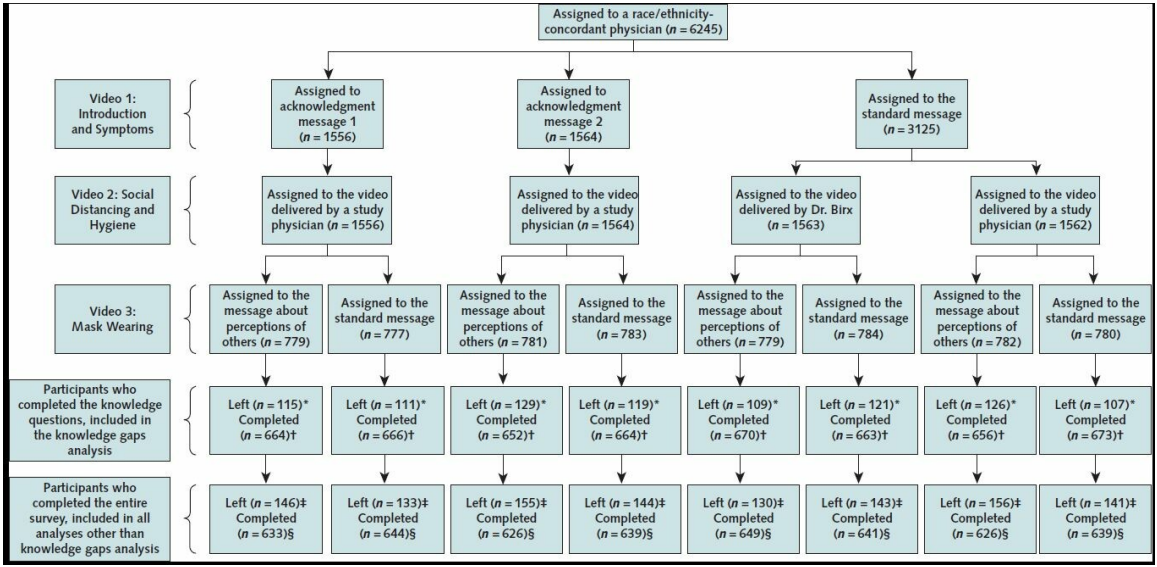
- What if D_{2i} is only given at times when D_{1i} is given?
  - Then, it's never possible to identify the effect relative to $Y_i(0,1)$
  - What does that imply about our other potential estimands?
  - E.g. if we just looked at the *marginal* estimands, where we were estimating the effect of one treatment, $E(Y_i(1, D_{i2}) - Y_i(0, D_{i2}))$ – this would integrate over joint distribution of treatments

- In this example, D_2 is no longer conditionally ignorable – the vector itself could be, but not individual components

- In most simple cases with multiple treatments, you'd randomize along multiple arms, and this problem is avoided

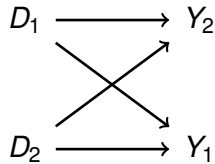


CONSORT diagram



Now, the SUTVA hits the fan

- In the discussion so far, the “interference” between treatments just comes from having multiple treatments to worry about
- What if treatments spill across units?
- Recall SUTVA: the potential outcomes of a unit do not vary with the treatment of other units
- When could this be violated?
  - *So many places*
- Why does this create an issue? Recall our discussion regarding marginal estimands – even with random assignment, our estimates of an effect will be contaminated by others’ treatment status



An overview of the types of issues caused by SUTVA not holding

- This type of problem is generally referred to as "interference."
  - It is challenging in a number of ways – for identification, estimation and inference
  - Today we'll focus on identification

- Want to touch on three versions of this problem:
  1. Social interactons and peer effects
  2. Spatial spillovers
  3. Economic interactions – budget constraints, etc.

- All these problems are versions of violation of SUTVA
  - With a clean, well-identified experiment, many of these settings still work
  - However, to get the estimands we're interested in, we may have to substantially modify our traditional estimators or make strong assumptions

Networks

- Historical context: Manski (1993)
  - Paper focused on a linear-in-means structural equation

$$Y = \underbrace{\beta E(Y|g)}_{\text{endogeneous}} + \underbrace{\gamma_1 E(X|g)}_{\text{exogeneous}} + \gamma_2 X + \underbrace{\gamma_3 g}_{\text{contextual}} + u$$

- Peers were not well-defined but usually groups like classrooms, clubs, etc.

- This is a *structural* model. RF:

$$Y = \gamma_1/(1-\beta)E(X|g) + (\gamma_2/(1-\beta))X + (\gamma_3/(1-\beta))g + \tilde{u}$$

Identification of Endogenous Social Effects: The Reflection Problem

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First version received December 1991; *final version accepted December* 1992 (*Eds.*)

A variety of terms in common use connote endogenous social effects, wherein the propensity of an individual to behave in some way varies with the prevalence of that behaviour in some reference group containing the individual. These effects may, depending on the context, be called "social norms", "peer influences", "neighbourhood effects", "conformity", "imitation", "contagion", "epidemics", "bandwagons", "herd behaviour", "social interactions", or "interdependent preferences".

Mainstream economics has always been fundamentally concerned with a particular endogenous effect: how an individual's demand for a product varies with price, which is partly determined by aggregate demand in the relevant market. Economists have also

Why do such different perspectives persist? Why do the social sciences seem unable to converge to common conclusions about the channels through which society affects the individual? I believe that a large part of the answer is the difficulty of the identification problem. Empirical analysis of behaviour often cannot distinguish among competing hypotheses about the nature of social effects.

Large literature built off of this

- Manski (1993) spawned a huge literature, a lot of which focused on the linear-in-means model.
  - There's a literature that microfounds why you might use it

- An inherent issue, in my view, is that many empirical papers jumped to this construction immediately. They did not have a structural interpretation in mind, but used this setting as a way to test for effects.

- An innovation in this space was to start using network data to define the group structure
  - One key paper that moved to network version: Bramoullé et al. (2009)
  - Reframe Manski Linear-in-means model to

$$\begin{aligned} Y &= \beta AY + \gamma_1 AX + \gamma_2 X + \epsilon_i, \\ &= (I - \beta A)^{-1}\gamma_1 AX + (I - \beta A)^{-1}\gamma_2 X + (I - \beta A)^{-1}\epsilon_i \end{aligned}$$

  where A is an $n \times n$ matrix of individuals' connections. Again, structural, but now richer interactions

Design-based approach to peer effects

- Taking the point of view of evaluation, or design-baesd inference, setting up the empirical model in this way is somewhat confusing
- Instead, useful to think about the general form of social interactions that are identified in a potential outcomes framework
- Given n individuals, for person i, how much interference can we allow? What types?

$$Y_i(D_1, D_2, \ldots, D_n)$$

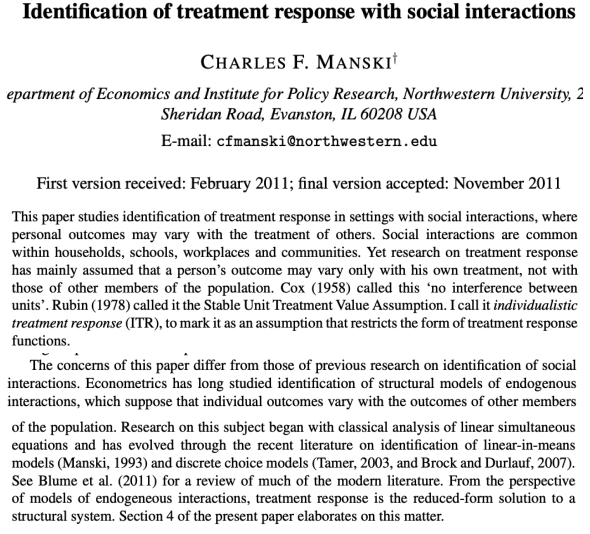
is far more extreme than

$$Y_i(D_i, A\mathbf{D}_n).$$

- This is a very active literature

Networks

- There is no “one solution” in this setting
- Certain restrictions need to be made to identify some estimands
- Manski (2013) is a very nice discussion of this in a *very* high-level way
  - Warning: this paper can make you feel overwhelmed
  - It is fine to make restrictive assumptions to identify effects!
- Key point to identify in this literature – are you attempting to estimate the *spillover* effect, or are you attempting to identify individual ATE in the presence of spillovers?



Identification of treatment response with social interactions

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First version received: February 2011; final version accepted: November 2011

This paper studies identification of treatment response in settings with social interactions, where personal outcomes may vary with the treatment of others. Social interactions are common within households, schools, workplaces and communities. Yet research on treatment response has mainly assumed that a person's outcome may vary only with his own treatment, not with those of other members of the population. Cox (1958) called this 'no interference between units'. Rubin (1978) called it the Stable Unit Treatment Value Assumption. I call it *individualistic treatment response* (ITR), to mark it as an assumption that restricts the form of treatment response functions.

The concerns of this paper differ from those of previous research on identification of social interactions. Econometrics has long studied identification of structural models of endogenous interactions, which suppose that individual outcomes vary with the outcomes of other members of the population. Research on this subject began with classical analysis of linear simultaneous equations and has evolved through the recent literature on identification of linear-in-means models (Manski, 1993) and discrete choice models (Tamer, 2003, and Brock and Durlauf, 2007). See Blume et al. (2011) for a review of much of the modern literature. From the perspective of models of endogeneous interactions, treatment response is the reduced-form solution to a structural system. Section 4 of the present paper elaborates on this matter.

Two papers in this space - Aronow and Samii (2017)

- Aronow and Samii (2017) provide a framework for thinking about estimation and identification under general forms of interference

- A&S use design-based inference, and consider the following generalized mapping.
  - For any generalized vector of interventions, $\mathbf{D}_n$, there's an experimental design which assigns probabilities over this (this is familiar!)
  - There is then an *exposure* mapping $f(\mathbf{D}_n, \theta_i)$ from these vectors to a treatment, which includes traits of an indiviudal, θ_i (e.g. their network location) and the treatment vector, and maps it to an exposure outcome.

- This exposure mapping does two things:
  - Makes restrictions on types of interactions (e.g. who can affect you and what type of effect it is)
    - To make this concrete – is it the sum of your connected individuals in your network? Any exposure at all? Does it matter who in your network exposes you?
  - Maps the experimental design to a propensity score of the exposure treatment

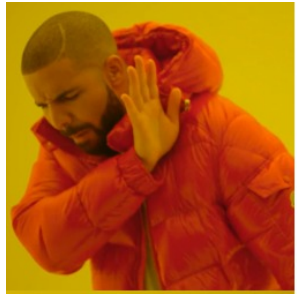
- This allows the use of IPW estimators, which are unbiased (but variance of estimator is conservative)

Two papers in this space - Athey, Eckles, and Athey (2018)

- This is a paper about null hypothesis tests under networks
- Key feature that this paper adds: testing specific types of analysis by creating “artificial” experiments
- This approach is less conservative, but more focused on testing

My views on social interactions summed up

- Already very hard to do research considering spillovers
- Make sure to not ignore the difficult identification challenges and assumptions that you'll need to make
- If you need a model, that's great!



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Manski (1993)

Identification of treatment response with social interactions

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Manski (2013)

Spatial analysis

- Unsurprisingly, geographic proximity
- Spatial literature has sat in the same literature as social interactions
  - Distance on a network graph can be viewed as a similar distance metric to geographic (or economic) distance
- Similar A matrix, and consequentially similar structural models are propose
- The Aronow and Samii setting allows for this as well – nothing deeply different here relative to networks, except that distance is potentially more continuous / complex

Economic spillovers, budget constraints, and GE

- Consider the following simple experiment – I give one half of people in the economy checks for $2000 dollars.
  - I then study the impact of these checks on their consumption
  - Why might the effects be different than if I had run this experiment on a small share of individuals?

- The economic spillovers coming through budget constraints are hugely important, but also deeply challenging
  - This class is not the best place for them
  - Instead, I will briefly touch on two examples that try to deal with this

Example 1: Chodorow-Reich (2019)

- Use cross-region incidence of fiscal stimulus to identify multipliers on local employment
- How can we use this to inform what we care about, e.g. a large *national* stimulus?
  - Aside: how could we term what the different estimands are?
  - $E(Y(1) - Y(0))$ is not quite right for regional effect, but estimand of interest is clearly $E(Y_{US}(1, \ldots, 1) - Y_{US}(0, \ldots, 0))$
- Using economic theory, make the case that cross-region evidence bounds the estimand of interest from below.

Geographic Cross-Sectional Fiscal Spending Multipliers: What Have We Learned?†

By Gabriel Chodorow-Reich*

A geographic cross-sectional fiscal spending multiplier measures the effect of an increase in spending in one region in a monetary union. The past several years have witnessed a wave of new research on such multipliers. By definition, estimation uses variation in fiscal policy across distinct geographic areas in the same calendar period. This approach has a number of advantages, most notably the potential for much greater variation in policy across space than over time and variation more plausibly exogenous with respect to the no-intervention paths of outcome variables. At the same time, cross-sectional multipliers differ in important dimensions from the national government spending multiplier to which they are often compared. Recognition of these differences has led to pessimism regarding whether cross-sectional multipliers provide any guidance for the effects of other types of policies.[1]

In this paper, I assess what we have learned from this research wave. I find the retreat regarding the literature's informativeness for other interventions to be premature. Drawing on theoretical explorations, I argue that the typical empirical cross-sectional multiplier study provides a rough lower bound for a particular, policy-relevant type of national multiplier, the closed economy, no-monetary-policy-response, deficit-financed multiplier. The lower bound reflects the high openness of local regions, while the "rough" accounts for the small effects of outside financing common in cross-sectional studies. I then review empirical estimates and find a cross-study mean of about 1.8. Putting these two elements together, cross-sectional studies imply a lower bound on the appropriate national multiplier of roughly 1.7.

Example 2: Sraer & Thesmar (2020)

- Use cross-firm experiment to influence the allocation of credit
- Some firms got lots more credit! Some did not
- How to aggregate up this affect? E.g. the policy effect is estimated by differencing the impact of the change on those who were more directly exposed vs. not – however, this doesn't tell us about the aggregate impact on the economy
- The paper argues, using ecnoomic theory, that these issues can be safely ignored under certain assumptions

How to Use Natural Experiments to Estimate Misallocation *

David Sraer
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November 20, 2020

Abstract

We propose a method to estimate the effect of firm policies (e.g., bankruptcy laws or subsidized credit) on allocative efficiency using (quasi-) experimental evidence. Our approach takes general equilibrium effects into account and requires neither a structural estimation nor a precise assumption on how the experiment affects firms. Our aggregation formula relies on treatment effects of the policy on the distribution of output-to-capital ratios, which are easily estimated in (quasi-) experimental data. We show that this method is valid as long as the true data-generating process belongs to a large class of commonly-used models in macro-finance. Finally, we apply this method to the French banking deregulation episode of the mid-1980s and find that this reform led to an increase in aggregate TFP of 2.7%.

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- How to aggregate up this affect? E.g. the policy effect is estimated by differencing the impact of the change on those who were more directly exposed vs. not – however, this doesn't tell us about the aggregate impact on the economy
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A similar external validity issue arises when the econometrician endeavors to measure how aggregate efficiency would change if the policy was extended to all firms in the economy. All the econometrician can do is measure the effect of the policy change when the policy is not at scale. However, scaling up the experiment will result in changes in equilibrium conditions. For instance, it may lead to a wage increase, and firms may respond differently to the policy treatment when the labor market is tighter. There again, nothing guarantees that the estimated treatment effects in the real data can be used in a counterfactual exercise where general equilibrium conditions have changed.

While these two obstacles are real, our paper provides a broad set of conditions under which they can be safely ignored. Section 3 shows that under broad conditions, applicable in most macroeconomic models with heterogeneous firms, the distribution of MRPKs is independent of general equilibrium conditions. This is our main Theorem 1. As we show, this invariance relies crucially on two key assumptions about technology and frictions. First, the sources of distortions (financing frictions and constraints, tax

Final thought: Dynamics

- How to think about treatments staggered *in time*?
- We have a vector of outcomes over time – what effects can we identify treatments on? What restrictions do we need to make?
- Consider the mRNA Covid Vaccine trials – what assumptions do we need to identify the effect of just one dose?