Causal Inference under Interference: New Methods for Experimental Design and Observational Studies

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- The phenomenon is prevalent in the real world.

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- Public health intervention applies treatments at certain locations. "Herd immunity" dissipates in distance from these locations.
- Protest happens in one spot of a city. It affects bystander's political attitude in nearby areas.
- Both experiments and observational studies can be impacted by the presence of interference.

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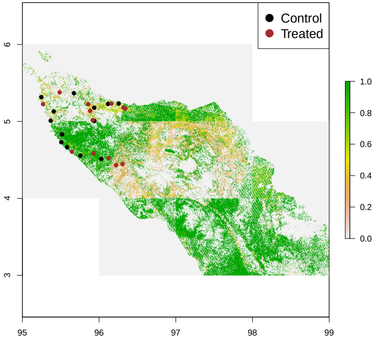
- Let's consider the following experiment:



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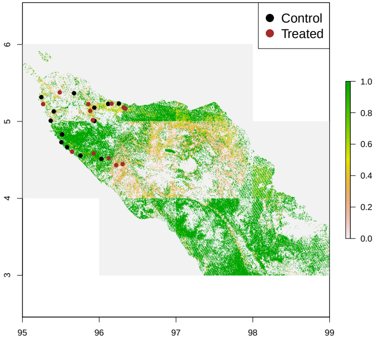

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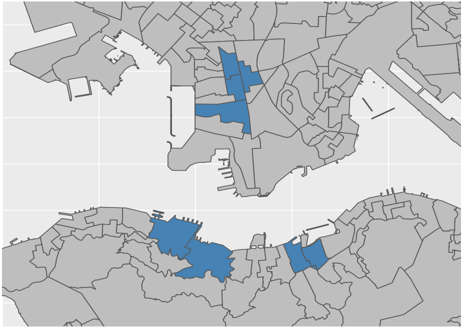

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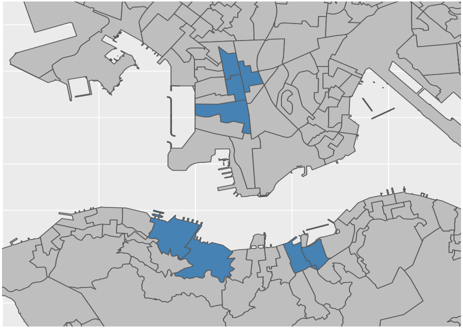

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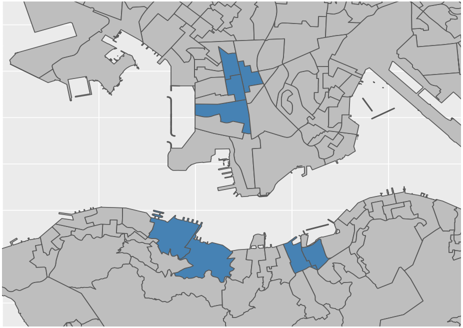

- In 2014, a protest later known as the Umbrella Movement broke out in Hong Kong.
- Protesters occupied metro stations in several constituencies of the city for 79 days.
- We know where the protest happened, and the vote share of the opposition in both the 2012 and 2016 elections.

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- What do the two examples have in common?
- The treatment is imposed on one observation but its impact is not limited to this observation.
- Why is it a problem?

Motivation

- Recall the classic Neyman-Rubin model:

$$Y_i = \begin{cases} Y_i(1) \text{ if } Z_i = 1 \\ Y_i(0) \text{ if } Z_i = 0 \end{cases},$$

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- The average treatment effect (ATE) is defined as $\tau = \frac{1}{N}\sum_{i=1}^{N} \tau_i$.
- If Z_i is randomly assigned, we can estimate the ATE via the group-mean difference estimator:

$$\hat{\tau} = \frac{1}{N_1}\sum_{i=1}^{N} Z_i Y_i - \frac{1}{N_0}\sum_{i=1}^{N}(1 - Z_i) Y_i$$

  where $N_1 = \sum_{i=1}^{N} Z_i$ and $N_0 = \sum_{i=1}^{N}(1 - Z_i)$.

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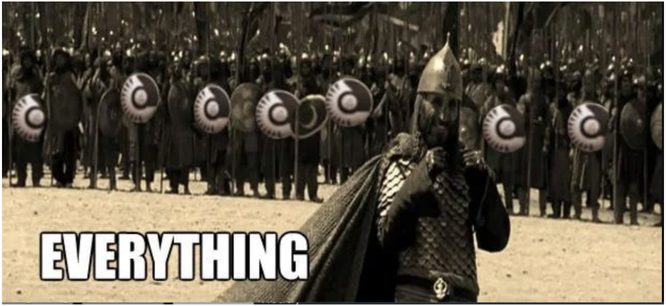

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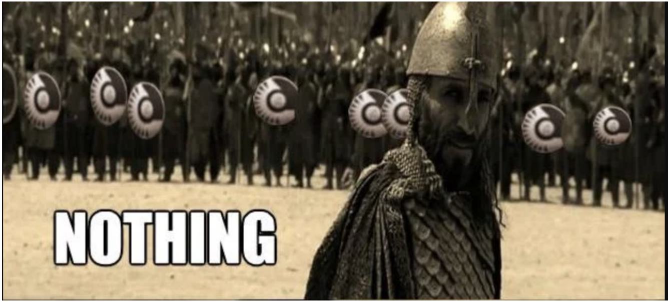

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- But this quantity is not well defined as its value is at random.

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- We can define

$$\tau_i = \mathbb{E}_{\mathbf{Z}_{-i}} [Y_i(1, \mathbf{Z}_{-i}) - Y_i(0, \mathbf{Z}_{-i})]$$

and the expected average treatment effect (EATE) as

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- This is the direct effect under interference.

Literature

- Consider a simple experiment with two subjects and Bernoulli assignment

Treatment status	Prob	Ye Wang	Xun Pang
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$$\tau_{Ye} = 0.5 * 4 + 0.5 * 5 = 4.5$$
$$\tau_{Xun} = 0.5 * 1 + 0.5 * 2 = 1.5$$

- Then,

$$\tau = \frac{1}{2}(4.5 + 1.5) = 3.$$

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- In other words, nothing but the interpretation changes.
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- But the indirect effect is more tricky.
- There are multiple ways to define and estimate the indirect effect in the literature.

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- g_i is called "level sets" (Athey et al. 2016).
- We can define the individual-level indirect effect as

$$\tau_i(Z_i) = Y_i(Z_i, g_i) - Y_i(Z_i, \tilde{g}_i)$$

and the expected average indirect effect as its average.

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- In this example, g_i takes three different values: 50%, 30%, and 0%.

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- Why don't students from other classes matter?
- How can we construct level sets in the monitoring station experiment?

Define the indirect effect in an agnostic way

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- This is the expected effect generated by the treatment status change of unit j on the outcome of unit i.

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- Therefore, we need to construct a "spillover mapping" μ to aggregate these quantities.
- μ is a linear mapping from $\mathcal{R}^N$ to $\mathcal{R}$ that captures the influence of a unit on the others.
- The form of μ is decided by the purpose of the study.

Define the indirect effect in an agnostic way

- For example, if we are interested in the spillover effect at distance d, we can construct an average "circle mean:"

$$\mu_j(d) = \frac{\sum_{i=1}^{N} \mathbf{1}\{d_{ij} = d\}\tau_{i;j}}{\sum_{i=1}^{N} \mathbf{1}\{d_{ij} = d\}}$$

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- We call this quantity the "average marginalized response" (AMR).

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$$\mu_j(\mathbf{Y}; d) = \frac{\sum_{i=1}^{N} \mathbf{1}\{d_{ij} = d\} Y_i}{\sum_{i=1}^{N} \mathbf{1}\{d_{ij} = d\}}$$

and then apply the group-mean difference estimator:

$$\hat{\tau}(d) = \frac{1}{N_1} \sum_{i=1}^{N} Z_i \mu_i(\mathbf{Y}; d) - \frac{1}{N_0} \sum_{i=1}^{N} (1 - Z_i) \mu_i(\mathbf{Y}; d)$$

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- We show that the estimator is consistent and asymptotically normal.

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- The outcome and the treatment are measured at two different levels.
- For example, each pixel on the map vs. each potential location for the monitoring stations.
- In this case,

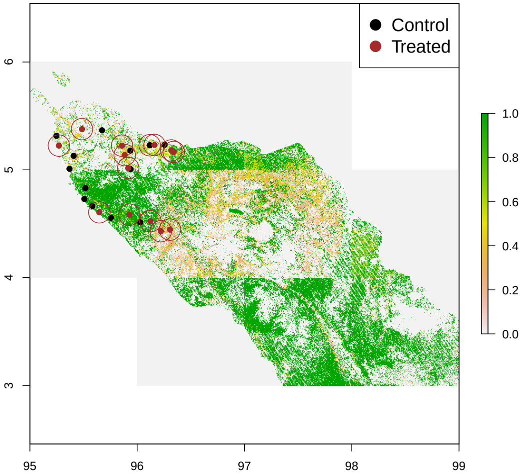
$$\tau_{ix} = \mathbb{E}_{\mathbf{Z}_{-i}}\left[Y_x(1, \mathbf{Z}_{-i}) - Y_x(0, \mathbf{Z}_{-i})\right]$$

and

$$\mu_i(d) = \frac{\sum_{x \in \mathcal{X}} \mathbf{1}\{d_{ix} = d\}\tau_{ix}}{\sum_{x \in \mathcal{X}} \mathbf{1}\{d_{ix} = d\}}$$

Application: monitoring stations in Indonesia

- To estimate the effect at distance d, we draw circles with radius d around each treated village.



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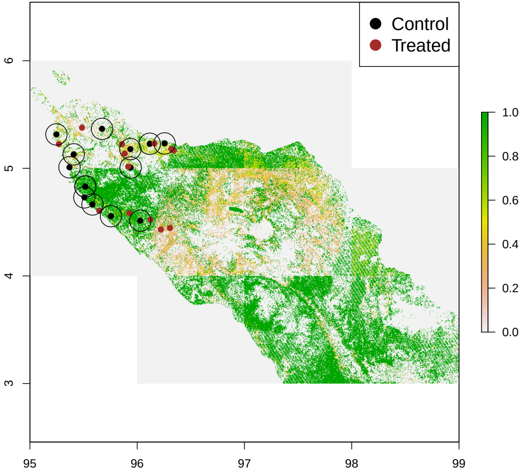
- Then, we do the same for each village in the control group.



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- At each distance d, we have

$$\hat{\tau}(d) = \frac{1}{14}\sum_{i=1}^{14} Z_i \mu_i(\mathbf{Y}; d) - \frac{1}{14}\sum_{i=1}^{14}(1 - Z_i)\mu_i(\mathbf{Y}; d)$$

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- Variance estimate of $\hat{\tau}(d)$ can be obtained via the spatial HAC variance estimator (Conley, 1999) in regression analysis.
- We can also rely on Fisher's permutation test for statistical inference.

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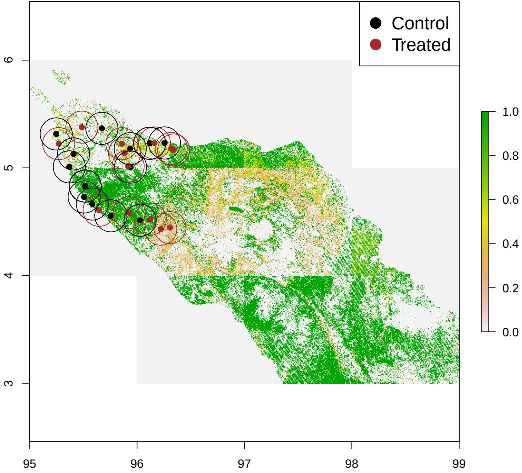
- We can estimate $\tau(d)$ for various values of d:



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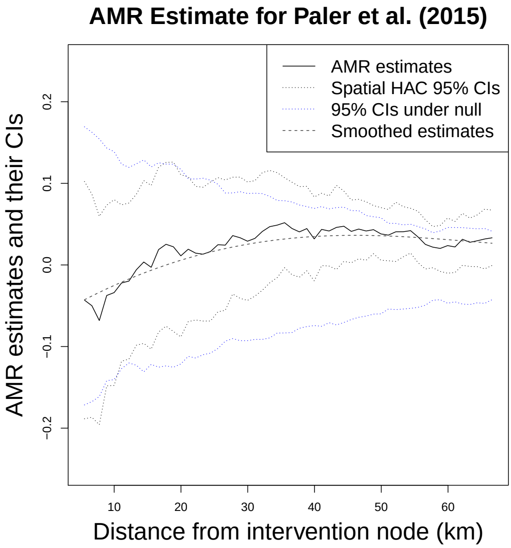

Estimate spillover effects in spatial experiments

- To estimate the direct effect, just calculate the group-mean difference.

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- The estimands are similarly defined (but more complicated).

Estimate spillover effects in observational studies

$$
\begin{aligned}
&\tau_{it;j}(\mathbf{z}^{s:t}, \tilde{\mathbf{z}}^{s:t}) \\
=&\mathbb{E}_{\mathbf{Z}^{1:T} \setminus \mathbf{Z}_j^{s:t}} \left[Y_{it}(\mathbf{z}^{s:t}; \mathbf{Z}^{1:T} \setminus \mathbf{Z}_j^{s:t}) - Y_{it}(\tilde{\mathbf{z}}^{s:t}; \mathbf{Z}^{1:T} \setminus \mathbf{Z}_j^{s:t}) \right] \\
&\mu_j(\tau_{(1:N,t);j}(\mathbf{z}^{s:t}, \tilde{\mathbf{z}}^{s:t}); d) = \frac{\sum_{i=1}^N \mathbf{1}\{d_{ij} = d\} \tau_{it;j}(\mathbf{z}^{s:t}, \tilde{\mathbf{z}}^{s:t})}{\sum_{i=1}^N \mathbf{1}\{d_{ij} = d\}} \\
&\tau_t(\mathbf{z}^{s:t}, \tilde{\mathbf{z}}^{s:t}; d) = \frac{1}{N} \sum_{j=1}^N \mu_j(\tau_{(1:N,t);j}(\mathbf{z}^{s:t}, \tilde{\mathbf{z}}^{s:t}); d) \\
&\tau(\mathbf{z}^{s:t}, \tilde{\mathbf{z}}^{s:t}; d) = \frac{1}{t-s+1} \sum_{t'=s}^{t} \tau_{t'}(\mathbf{z}^{s:t}, \tilde{\mathbf{z}}^{s:t}; d).
\end{aligned}
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Estimate spillover effects in observational studies

- For identification and estimation, we need to assume "sequential ignorability:"

$$\mathbf{Z}_t \perp Y_{it}(\mathbf{Z}_t, \mathbf{Z}^T \setminus \mathbf{Z}_t) | \mathbf{Z}^{t-1}, \mathbf{Y}^{t-1}, \mathbf{X}^t$$
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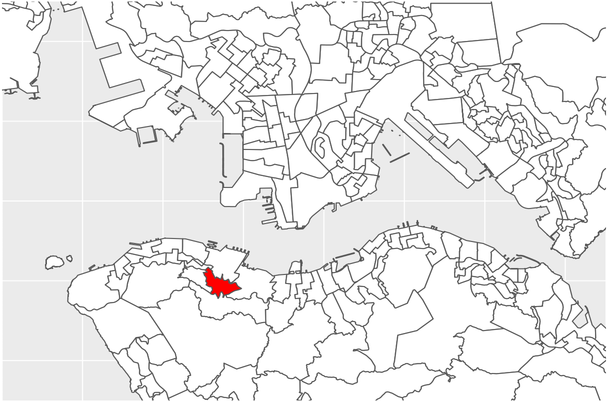
- Then, we can rely on the inverse probability of treatment weighting (IPTW) estimators for estimation.

Estimate spillover effects in observational studies

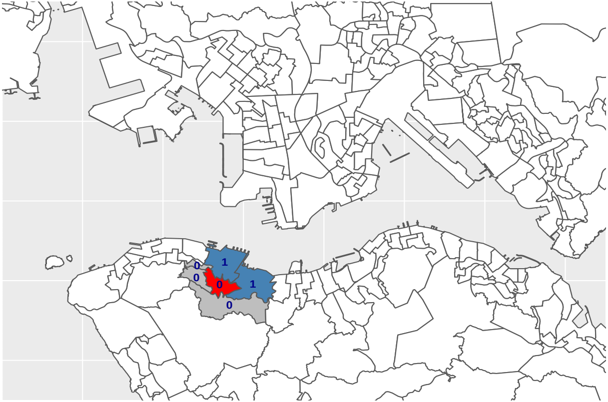
- Conventional approaches for dealing with spillover try to explain the outcome of each observation:



Estimate spillover effects in observational studies

- Conventional approaches for dealing with spillover try to explain the outcome of each observation:

$$Y_{it} = \delta_1 Z_{it} + \delta_2 \sum_{j \in \mathcal{N}_i} Z_{jt} + \beta \mathbf{X}_{it} + \varepsilon_{it}$$



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- How can we ensure that the model specification is correct?
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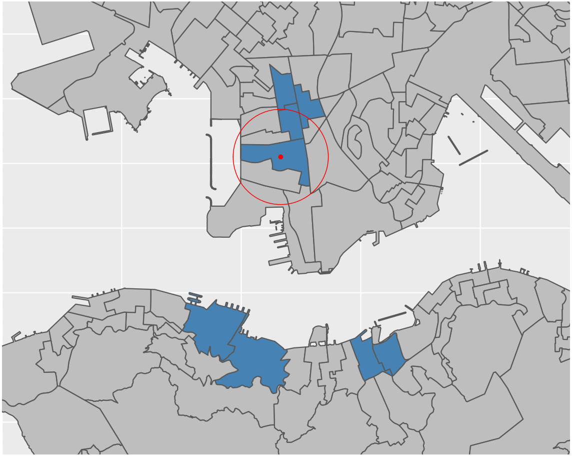
- What if the effect is not additive but interactive?
- The design-based approach asks a different question: what is the influence generated by the treatment status of unit i?
- How to measure the influence is up to the researcher's choice.

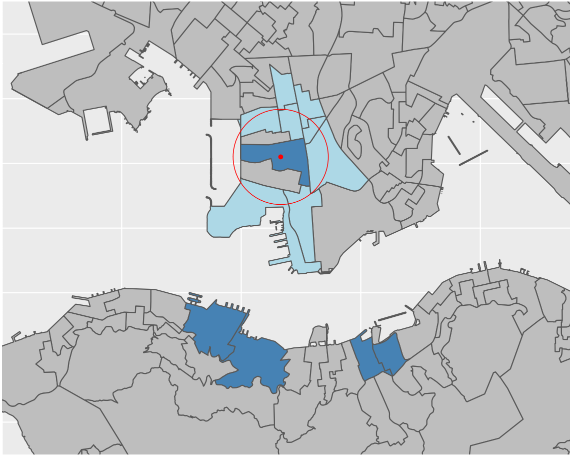
Estimate spillover effects in observational studies

- In practice, we just draw a circle with radius d around constituency i,



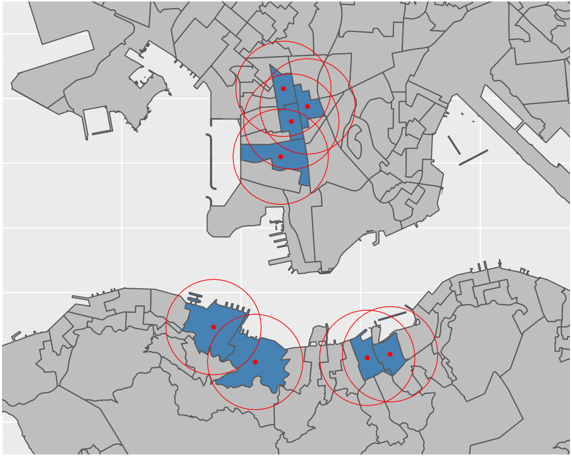
Estimate spillover effects in observational studies

- and calculate the average of the opposition's vote shares over the passed constituencies.



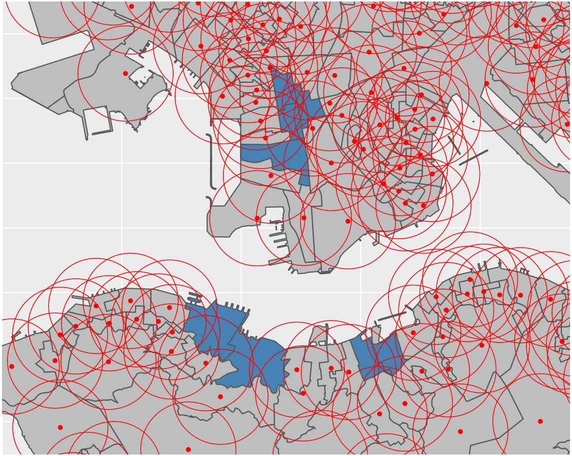
Estimate spillover effects in observational studies

- It is easy to calculate $\mu_i(\mathbf{Y_t}; d)$ for both treated constituencies



Estimate spillover effects in observational studies

- and untreated constituencies (trypophobia warning!).



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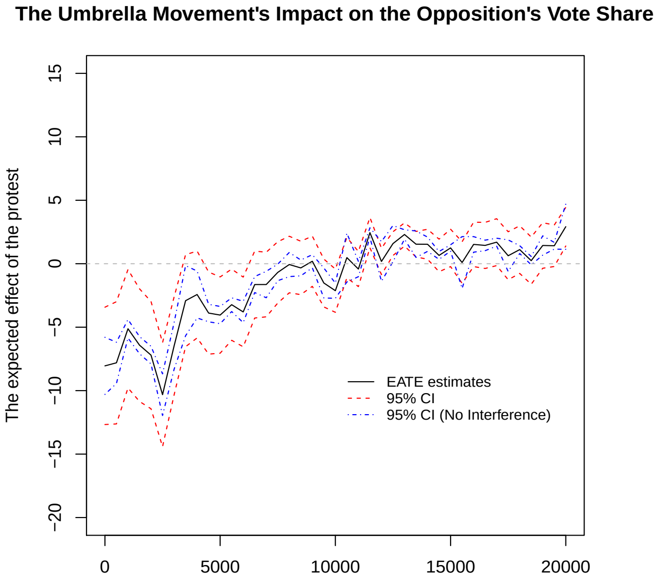
- Inference and asymptotic results are similar as in Aronow, Samii and Wang (2021).

Application: protest in Hong Kong



Application: reform in New York State

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- The dataset has a staggered adoption structure with 917 units, 25 periods, and 26 different types of history.

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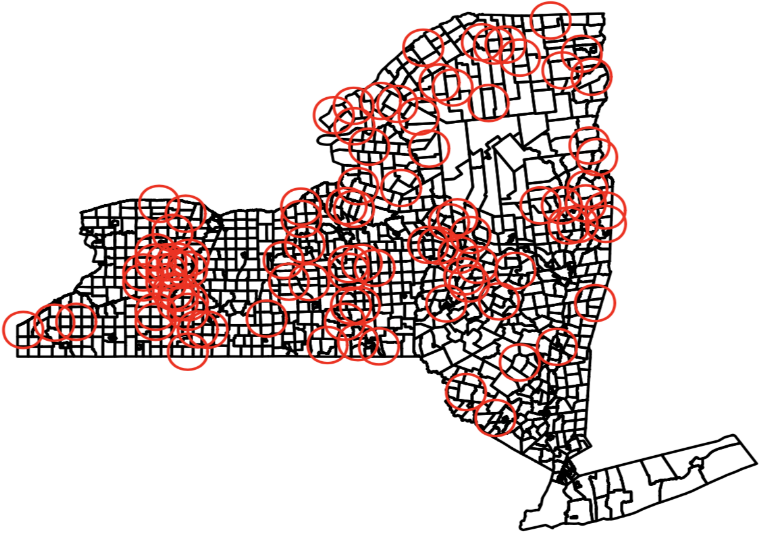

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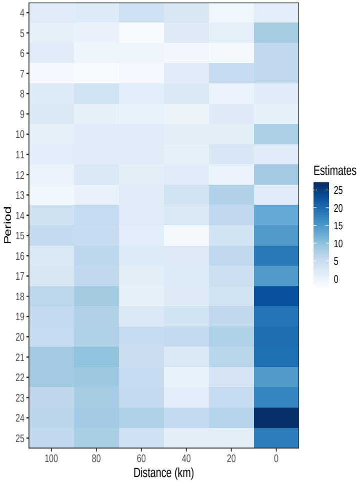

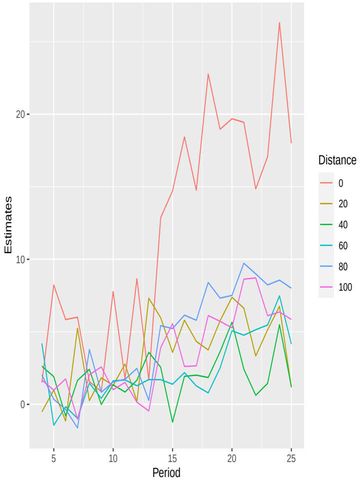

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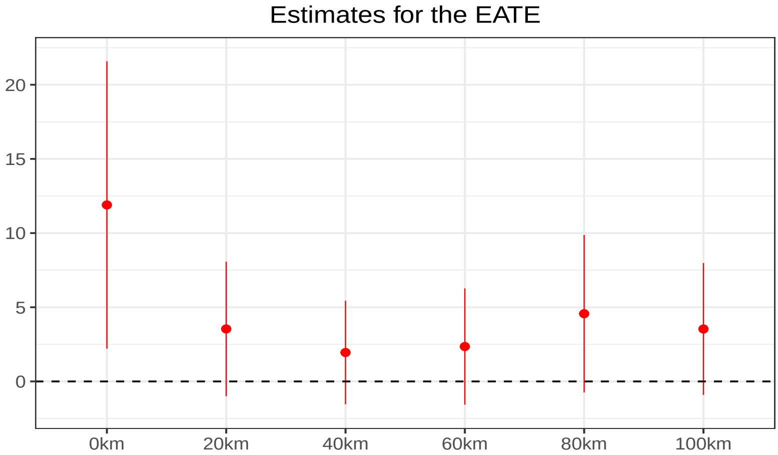

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- No causality without at least a hypothetical experiment.
- Whenever trying to identify a causal relationship from a dataset, you should think about the experiment that generates it.
- It focus on determinants of the treatment assignment rather the outcome.
- This perspective provides a clearer connection between theory and empirics and permits researchers to discuss causality in a more rigorous manner.

What's wrong with strict exogeneity?

- Let's consider a simple case:

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Y_{i1} &= \mu + \alpha_i + \xi_1 + \varepsilon_{i1} \\
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- It implies "parallel trends:"

$$\mathrm{E}\left[Y_{i2}(\mathbf{0}^{1:2}) - Y_{i1}(\mathbf{0}^{1:2}) | Z_{i2} = 1\right] =$$
$$\mathrm{E}\left[Y_{i2}(\mathbf{0}^{1:2}) - Y_{i1}(\mathbf{0}^{1:2}) | Z_{i2} = 0\right].$$

- The unknown propensity score for unit i is denoted as p_i, $p_i = P(Z_{i2} = 1 | \alpha_i)$.

What's wrong with strict exogeneity?

- The classic DID estimator takes the following form:

$$\hat{\tau}_{DID} = \frac{1}{N_1} \sum_{i=1}^{N} Z_{i2}(Y_{i2} - Y_{i1}) - \frac{1}{N_0} \sum_{i=1}^{N} (1 - Z_{i2})(Y_{i2} - Y_{i1})$$

What's wrong with strict exogeneity?

- We can show that,

$$\begin{aligned}
&\hat{\tau}_{DID} \\
&\to \frac{\mathrm{E}\left[p_i \mathbb{E}_{\mathbf{Z}^{1:2}\setminus Z_{i2}} g_i(1, \mathbf{Z}^{1:2} \setminus Z_{i2})\right]}{\mathrm{E}\, p_i} \\
&\quad - \frac{\mathrm{E}\left[(1-p_i) \mathbb{E}_{\mathbf{Z}^{1:2}\setminus Z_{i2}} g_i(0, \mathbf{Z}^{1:2} \setminus Z_{i2})\right]}{\mathrm{E}\,(1-p_i)} \\
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- It is **inconsistent** for the EATE and has **no substantive interpretation**.
- A similar problem has been identified by Aronow and Samii (2016) for cross-sectional analysis.

What's wrong with strict exogeneity?

- Without interference, $g_i(0, \mathbf{Z}^{1:2} \setminus Z_{i2}) = 0$ and $g_i(1, \mathbf{Z}^{1:2} \setminus Z_{i2}) = \tau_i$

$$\hat{\tau}_{DID} \rightarrow \frac{\mathrm{E}\,[p_i \tau_i]}{\mathrm{E}\,p_i} = PATT.$$

- Intuitively, the control group has been contaminated by spatial interference.

Bias of the DID estimator

$$
\begin{aligned}
Bias_{DID} &= \tau_{DID} - \tau_2(1,0;0) \\
&= \frac{\mathrm{Cov}\left[p_i, \mathbb{E}_{\mathbf{Z}^{1:2} \setminus Z_{i2}} g_i(1, \mathbf{Z}^{1:2} \setminus Z_{i2})\right]}{\mathrm{E}\, p_i} \\
&\quad + \frac{\mathrm{Cov}\left[p_i, \mathbb{E}_{\mathbf{Z}^{1:2} \setminus Z_{i2}} g_i(0, \mathbf{Z}^{1:2} \setminus Z_{i2})\right]}{\mathrm{E}\,(1 - p_i)}
\end{aligned}
$$

- The direction of the bias depends on the correlation between p_i and the marginalized effects.

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- Which scenario is more possible depends on your judgment.
- The cost of ignoring fixed effects is lower than you thought in the spatial setting.
- When units are small in size and contiguous to each other, we can approximate the influence of unit fixed effects via controlling a smooth function of geographical coordinates of units.

Conclusion

Thank you!