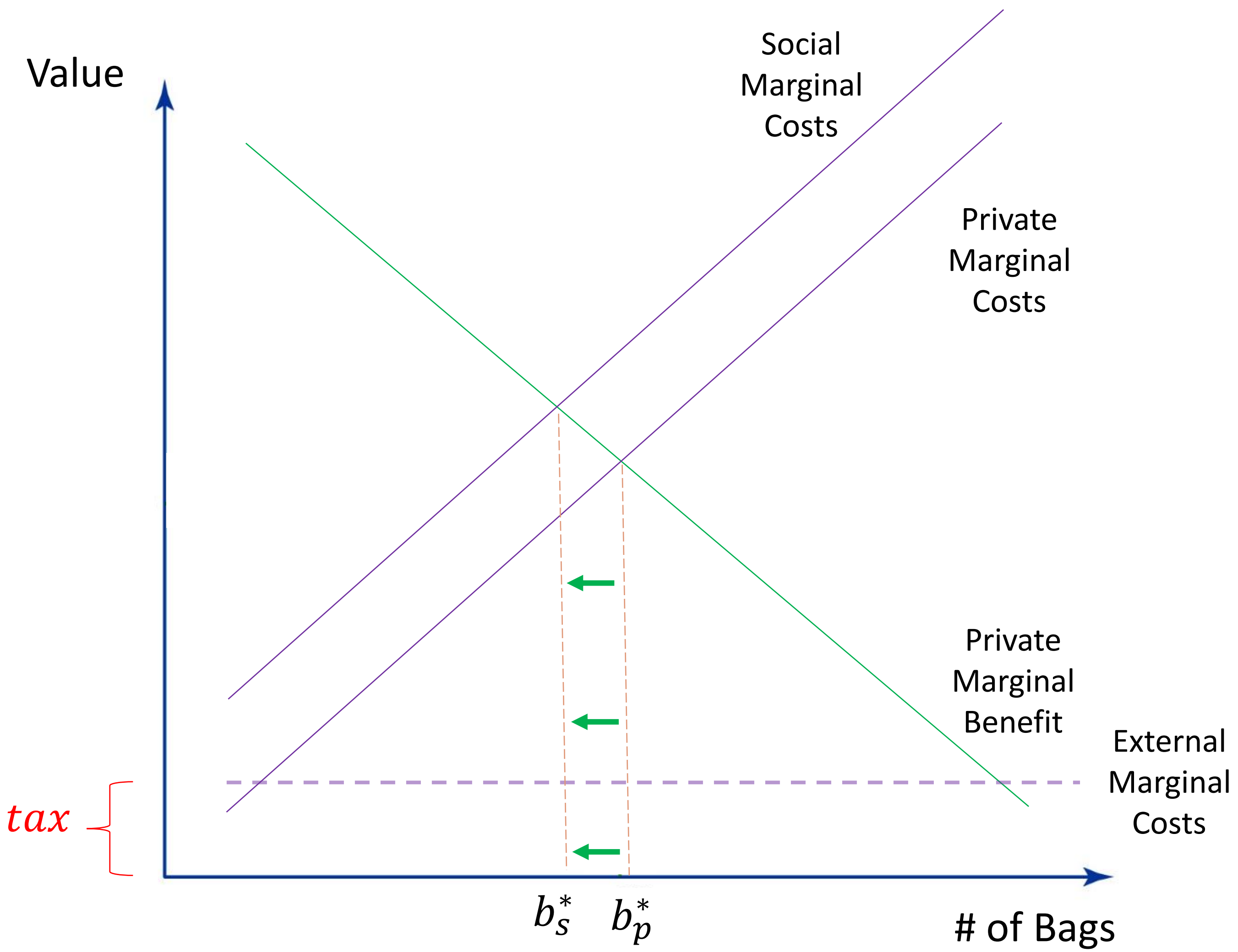
Lecture 3: Market Failures: Examples and in Practice

Prof. Parthum

Environmental Economics

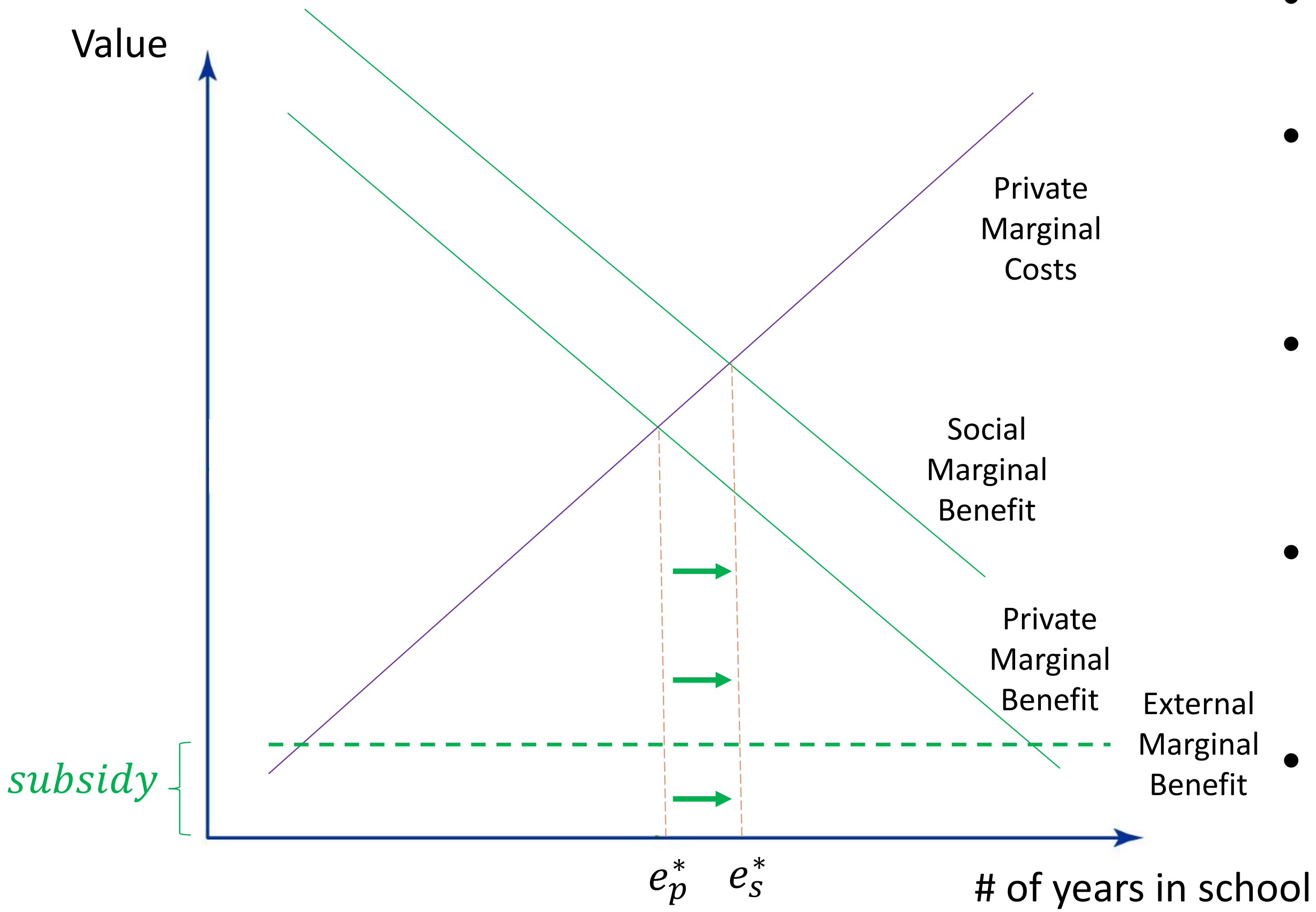
Econ 475

An Example:
The Market for Plastic Bags



- What's missing?
  - The external marginal cost
- Adding the external cost to the private recovers the (true) social marginal cost
- The socially optimal number of bags is *fewer* than the private. The private market *overprovides* bags
- What is a possible fix?
  - A *tax* equal to the external marginal cost

An Example:
The Market for College Education



- In education, the externality is a "good"
- The private market would *underprovide* public education
- A government *subsidy* can help correct this market failure
- In this context, is the external marginal benefit actually fixed (flat)?
- Try drawing an example of what you think external marginal benefits are

Market Failures

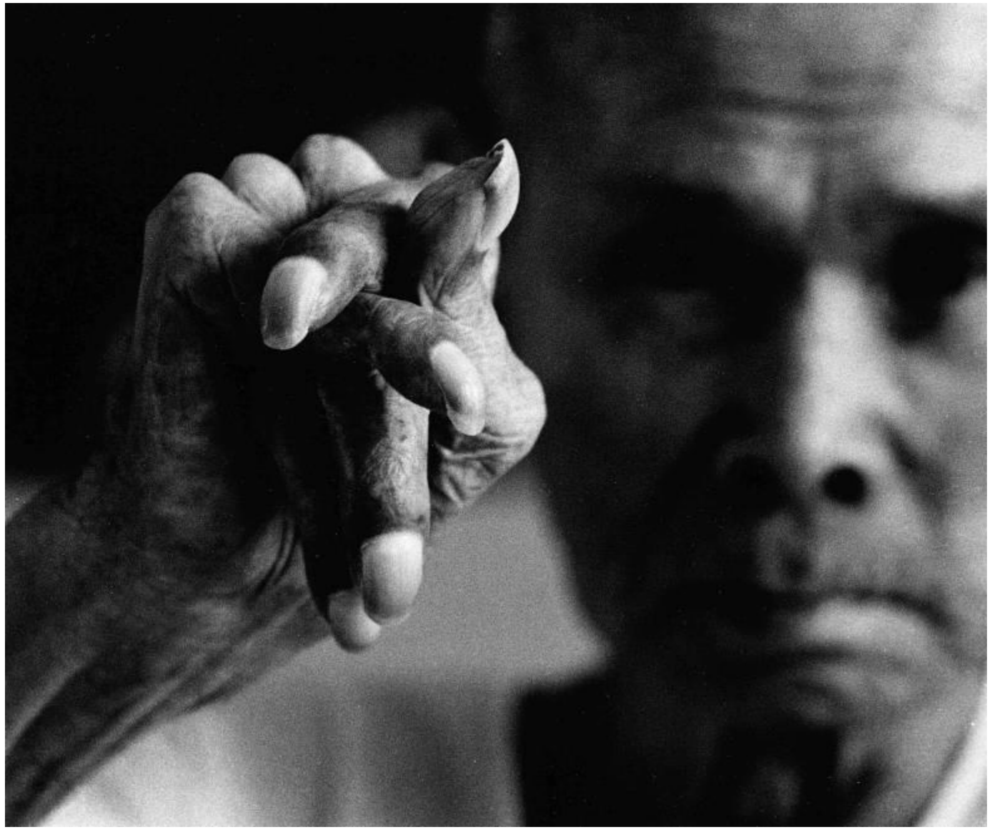
There are many different types or root causes of market failures. Think of some examples under each of these categories.

1. Information Asymmetries
   - One party in a transaction knows more than the other
2. Market Structure/Power
   - One party can influence the market equilibrium
3. Public Goods
   - Nonrival and nonexcludable
4. Externalities
   - Private actions have unintended effects

The Cuyahoga River Caught Fire at Least a Dozen Times, but No One Cared Until 1969

Source: history.com

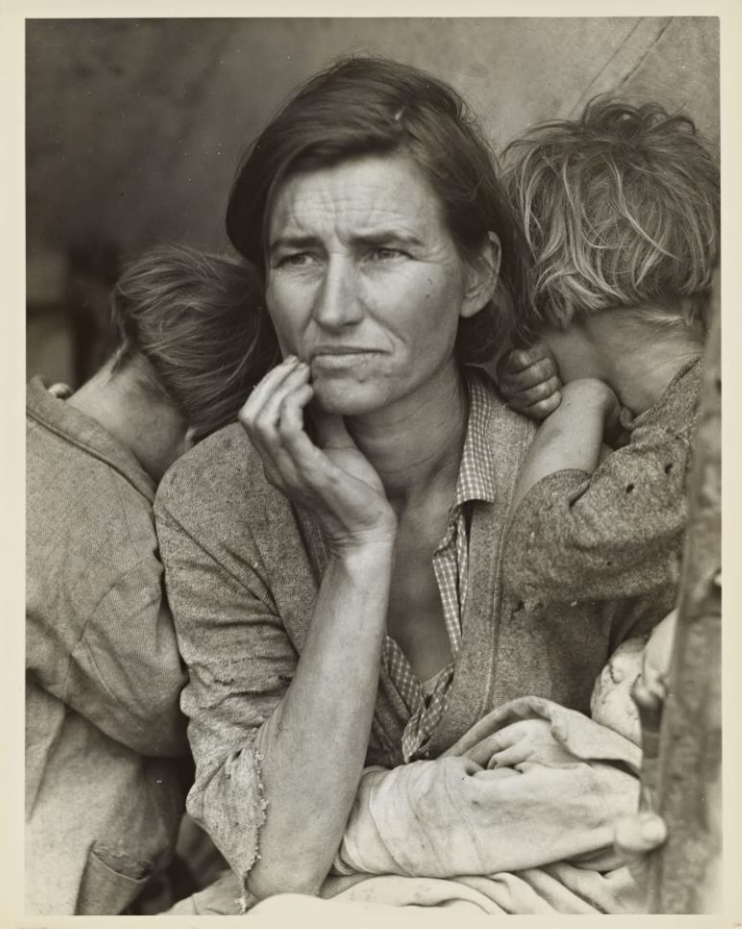
Minamata disease, 1956: the release of methylmercury in the industrial wastewater from a chemical factory owned by the Chisso Corporation



Source: [Blind Magazine]()

The Dust Bowl

As one "black blizzard" hit after another, harmful dust particles accumulated in people's lungs, causing hundreds of deaths and sickening thousands.



Source: history.com

Bag Leakage: The Effect of Disposable Carryout Bag Regulations on Unregulated Bags

By: Dr. Rebecca Taylor

Abstract: Leakage occurs when partial regulation of consumer products results in increased consumption of these products in unregulated domains. This article quantifies plastic leakage from the banning of plastic carryout bags. Using quasi-random policy variation in California, I find the elimination of 40 million pounds of plastic carryout bags is offset by a 12-million-pound increase in trash bag purchases—with small, medium, and tall trash bag sales increasing by 120%, 64%, and 6%, respectively. The results further reveal 12–22% of plastic carryout bags were reused as trash bags pre-regulation and show bag bans shift consumers towards fewer but heavier bags. With a substantial proportion of carryout bags already reused in a way that avoided the manufacture and purchase of another plastic bag, policy evaluations that ignore leakage effects overstate the regulation's welfare gains.

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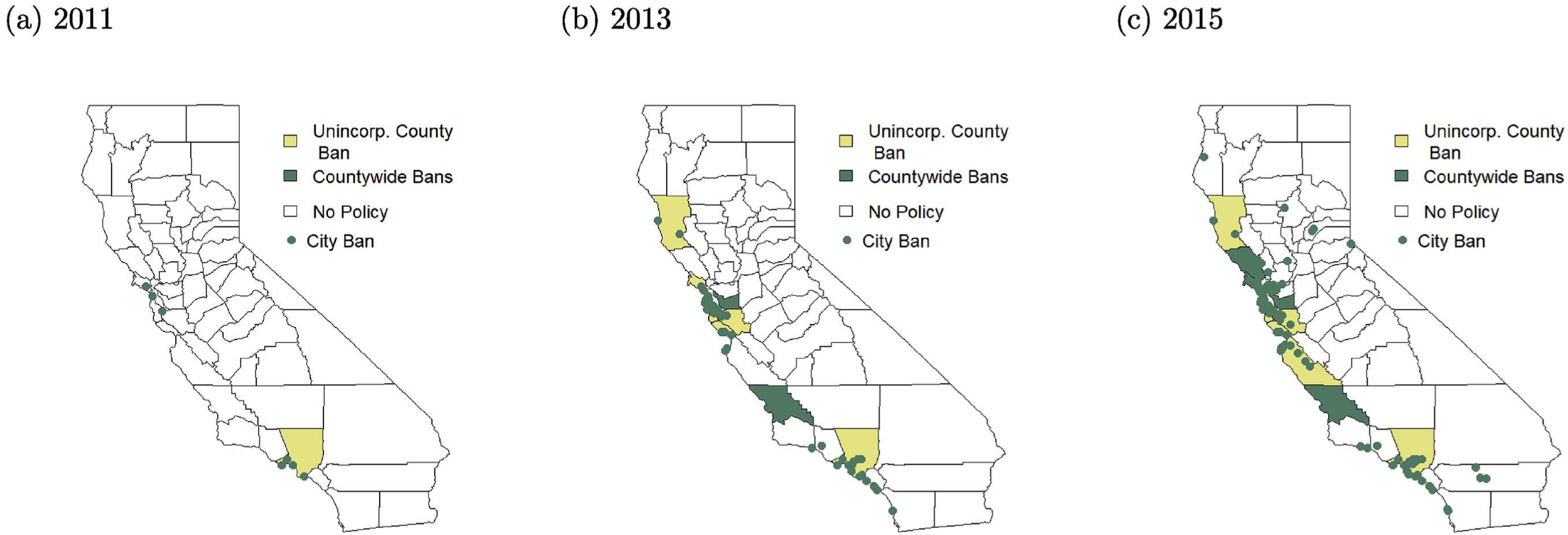

Note: The local governments of unincorporated counties and incorporated cities can pass ordinances to regulate disposable carryout bags. City-level policies are depicted with dark green circles. Unincorporated county policies are shaded in light yellow. Countywide policies—where all unincorporated areas and all cities in a county implement DCB regulations—are shaded in dark green.

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$$Y_{sjm}^{B} = \sum_{l=-12}^{12} \beta_l D_{l,jm} + \theta_{sj} + \delta_m + \epsilon_{sjm}$$

where Y_{sjm}^{B} is the outcome variable for store s in jurisdiction j and month-of-sample m with respect to bag product group B, θ_{sj} is a vector of store fixed effects, and δ_m is a vector of month-of-sample fixed effects. $D_{l,jm}$ is a dummy variable equaling one if jurisdiction j in month m implemented a DCB policy l months ago, with $l = 0$ denoting the month of implementation.

The β_l vector is the parameter of interest, as it traces out the differences in outcomes from before the DCB policies to after. I hypothesize that sales of trash bags deemed by customers to be substitutes for plastic carryout bags will increase. Thus, for any product group B that is a substitute for plastic carryout bags, I would expect the β_l coefficients in the post-policy period to be greater than zero.

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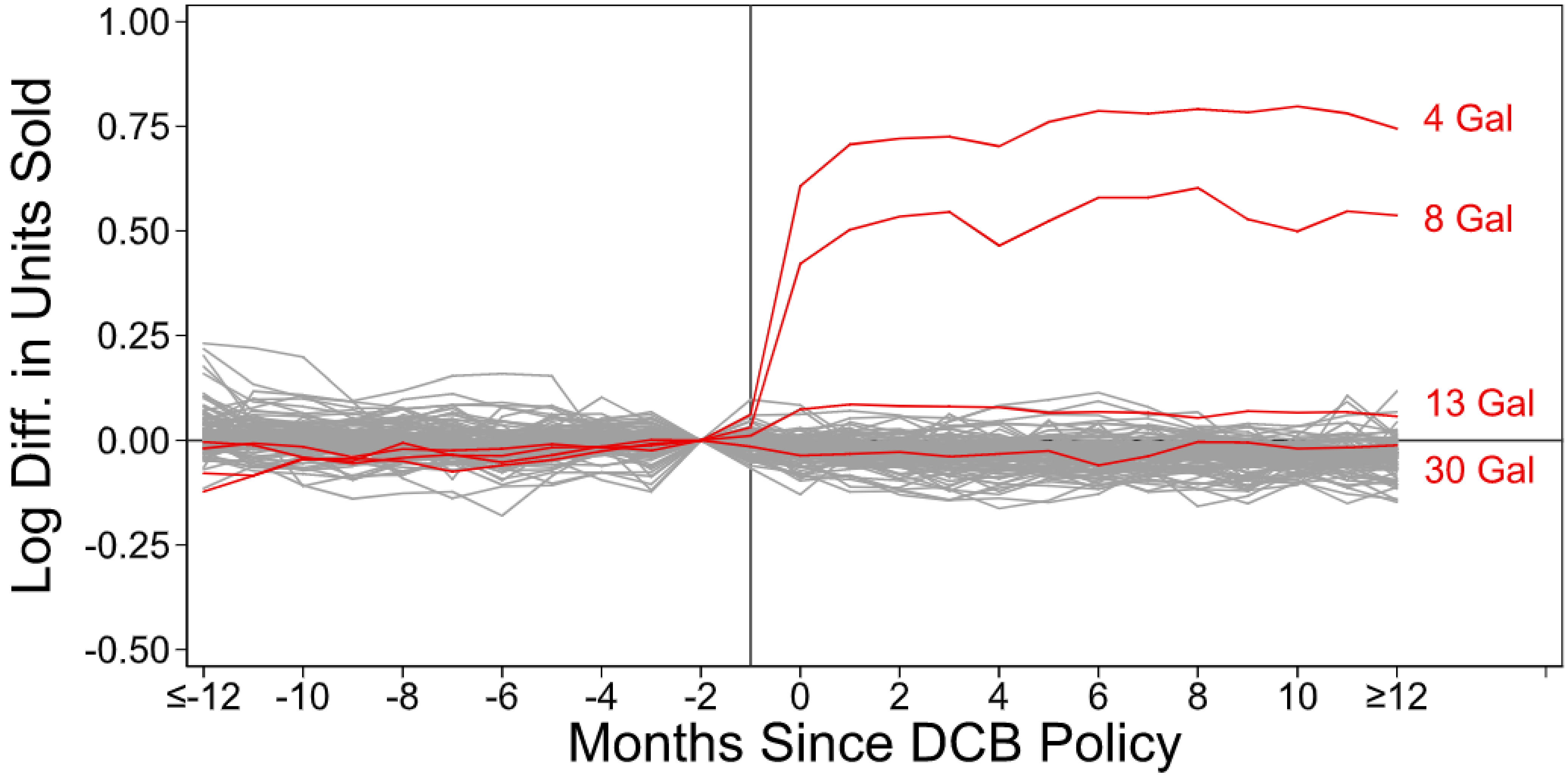

Downwind and out: The strategic dispersion of power plants and their pollution

By: Dr. John Morehouse and Dr. Ed Rubin

Abstract: In federalist systems, local governments can maximize local welfare by exporting locally produced negative externalities. We empirically substantiate this externality-export strategy for air pollution using historical power-plant siting, administrative borders, and prevailing wind directions. Using a simple, non-parametric test, we show that decision-makers disproportionately sited coal-fueled plants to reduce counties'/states' downwind pollution exposure. Natural-gas-fueled plants—lower polluters—did not follow this strategy. We then illustrate the extreme exportabilitiy of coal plants' pollution: within 6 hours, 50% of coal plants' emissions leave their source states—and 99% depart source counties. These results highlight how local strategic responses challenge federalist systems.

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$$p\text{-Value}(n_s) = \mathbf{P}(X \geq n_s;\, n = N_T,\, p = 0.5) = \sum_{x=n_s}^{N_T} \binom{N_T}{x} 0.5^{N_T}$$

We operationalize this test as an implementation of Fisher's Exact Test (Fisher, 1934; Fisher, 1935; Conover, 1971; Imbens and Rubin, 2015). Under a sharp (one-sided) null hypothesis of *no strategic siting to reduce downwind area*, the test statistic n_s (the number of plants for whom downwind area is less than upwind area) is distributed as a binomial distribution with size equal to the number of plants in the sample (N_T) and probability $p = 0.5$. Under this null, the expected share of plants whose downwind area is less than its upwind area is 50%. Consequently the p-value for a given test statistic is

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Figure 6: HYSPLIT trajectory and dispersion: Two example plants, January and July 2005

(a) Plant 1378, January 2005 **(b)** Plant 1378, July 2005

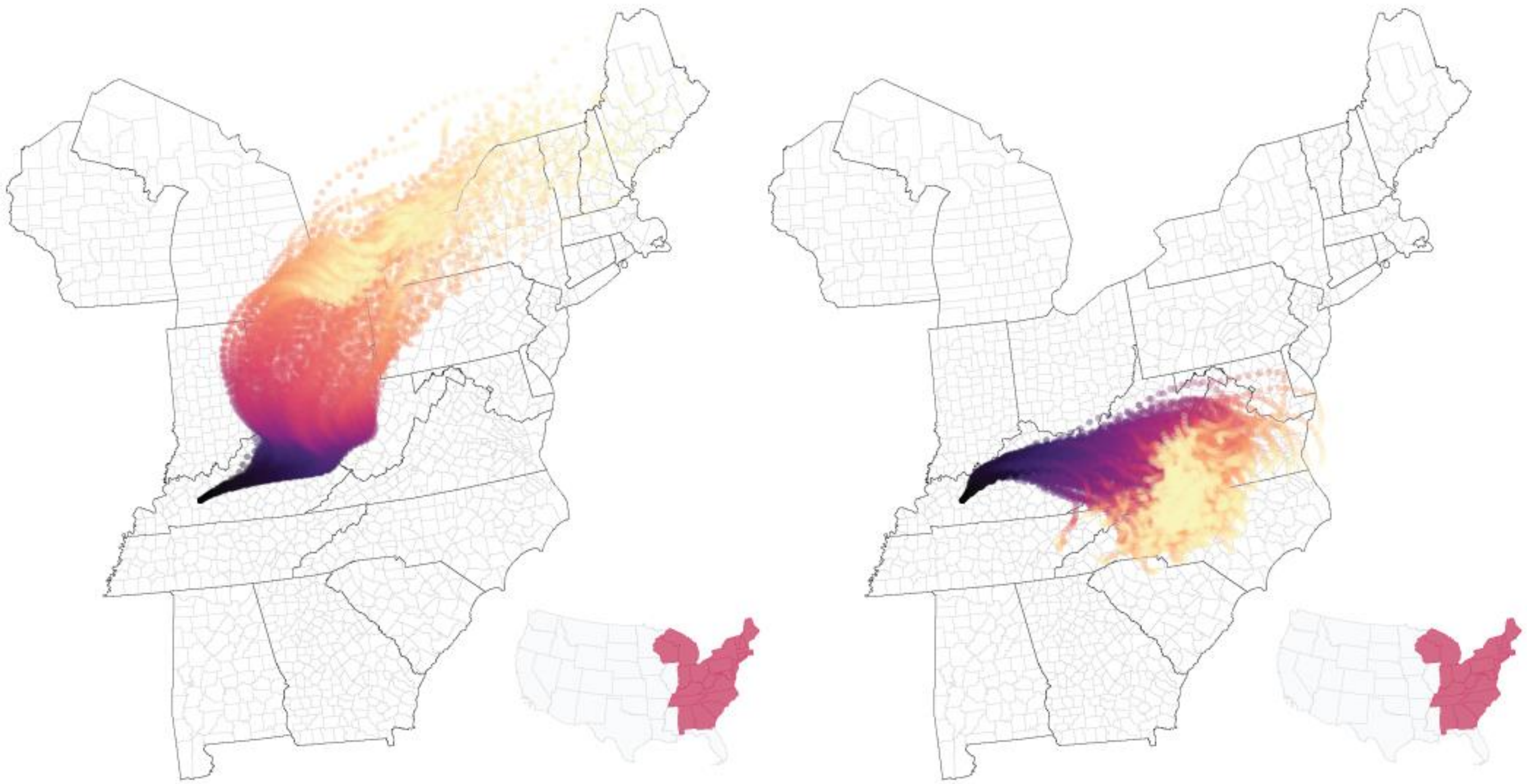

Table 1: Testing strategic siting: Upwind *vs.* downwind areas for coal and natural gas plants

	(1) **Coal-fueled plants**	(2) **Natural-gas-fueled plants**
Panel A: Siting strategically within **county**		
Count	514	1,254
Count *strategic*	292	620
Percent *strategic*	56.81%	49.44%
Fisher's exact test of H_0: In-**county** downwind area ≤ upwind area		
Under H_o: E[Percent strategic: County] = 50%		
P-value	0.0012	0.6641
Panel B: Siting strategically within **state**		
Count	514	1,254
Count *strategic*	277	574
Percent *strategic*	53.89%	45.77%
Fisher's exact test of H_0: In-**state** downwind area ≤ upwind area		
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P-value	0.0426	0.9987
Panel C: Siting strategically within **both county *and* state**		
Count	514	1,254
Count *strategic*	179	314
Percent *strategic*	34.82%	25.04%
Fisher's exact test of H_0: Downwind area ≤ upwind area in **county and state**		
Under H_o: E[Percent strategic: County ∧ State] = 25%		
P-value	<0.0001	0.4978

We define a plant's location as "strategic" if the downwind area *within its home county (or state)* is less than its upwind area *within its home county (or state)*. We calculate *downwind* and *upwind* areas based upon 90-degree right triangles with a vertex at the plant pointing up- or down-wind based upon the locally prevailing wind direction. Figure 2 illustrates this calculation. *Sources:* eGRID (2018) and authors' calculations.

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Should congested cities reduce their speed limits? Evidence from São Paulo, Brazil

By: Dr. Amanda Ang et al.

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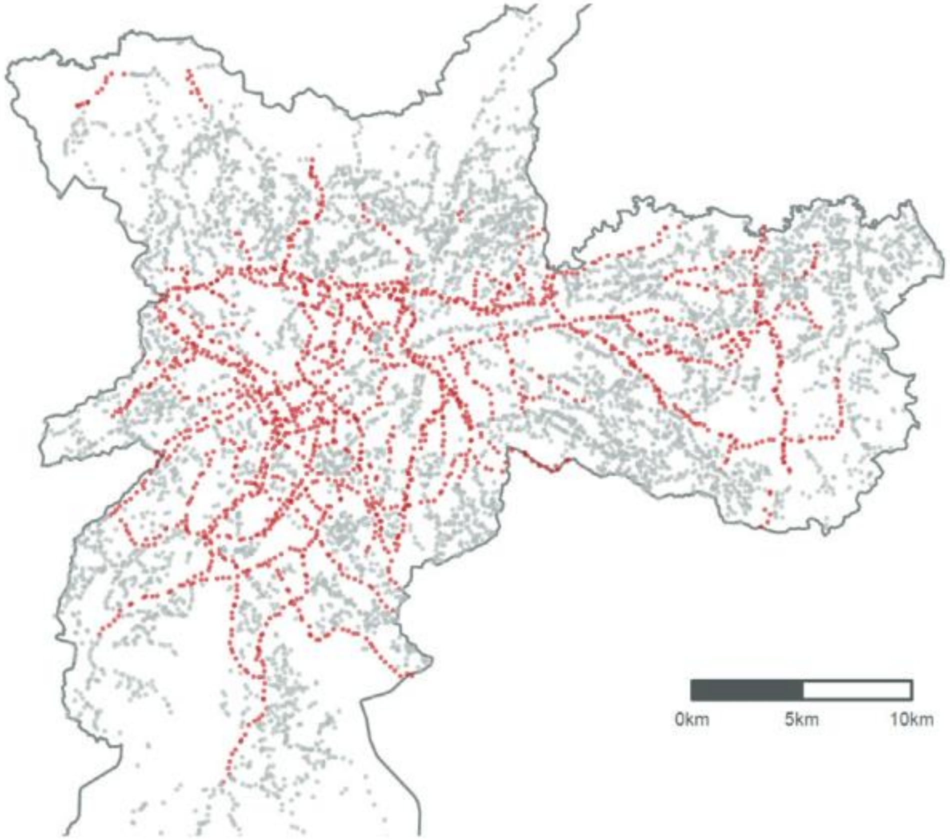
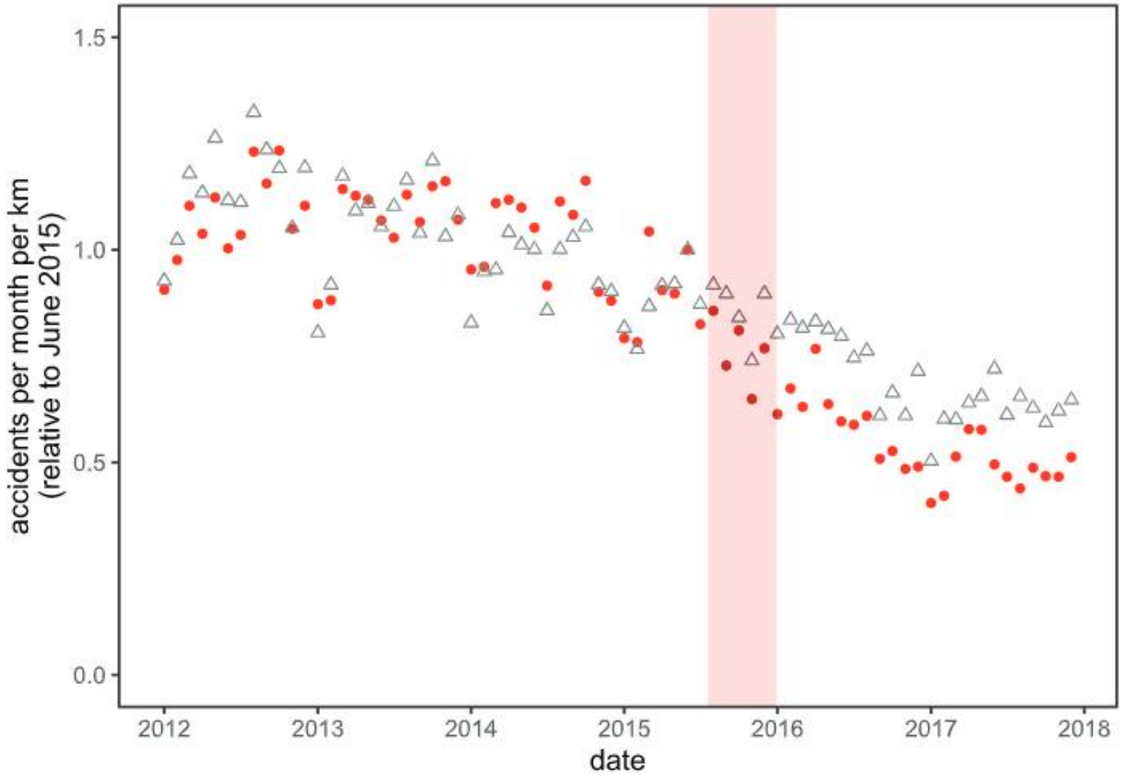
A: All treated and all control segments





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We use the following Poisson event study model[29] of accident counts to estimate of the impact of changing the speed limit:

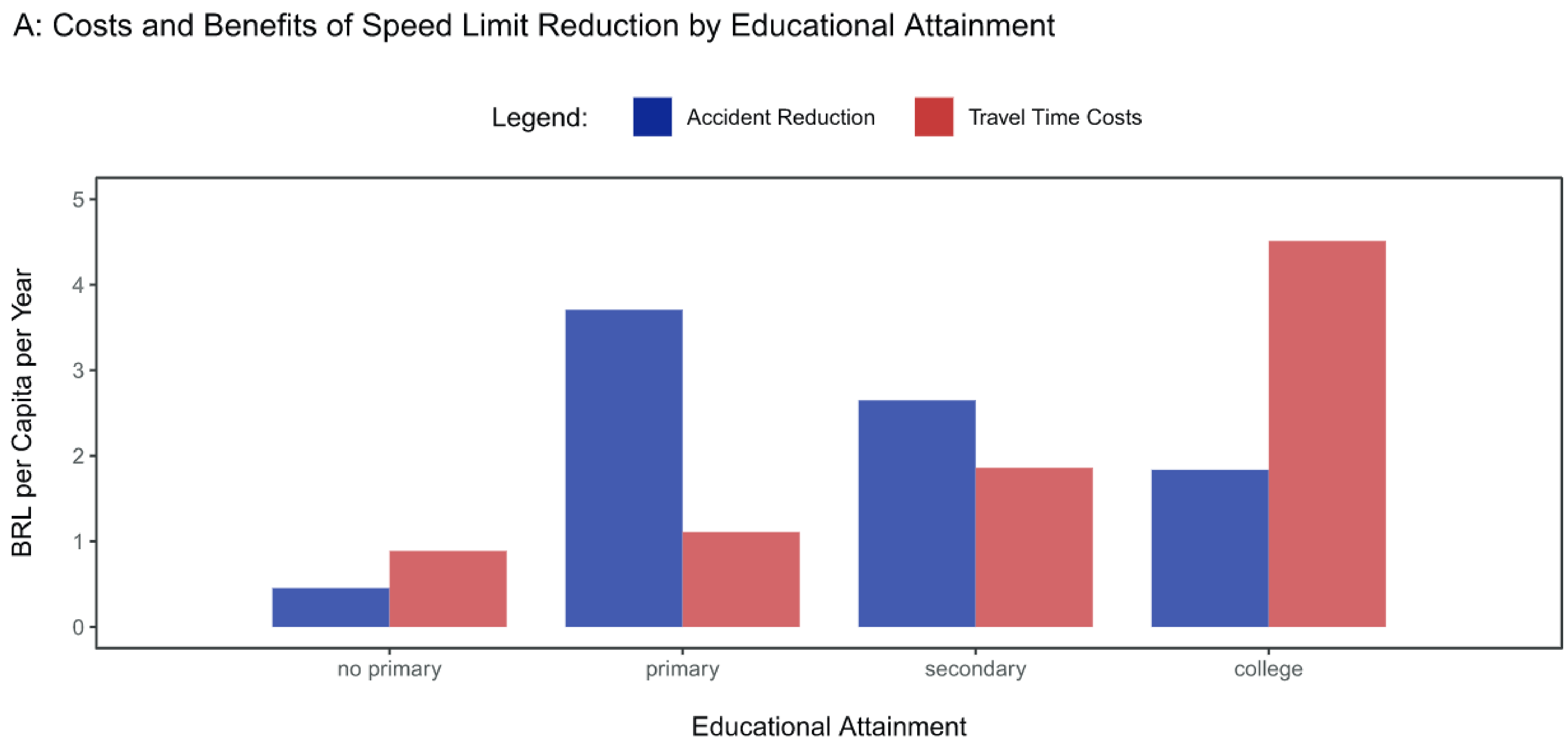
$$\log\left(E\left(y_{it}\right)\right) = \alpha_i + \beta X_t + \left(\sum_{q=1}^{6} \gamma_q D_{it}^{q}\right) + \zeta C_{it} + \eta C_{it} SLR_{it} \qquad (1)$$

where y_{it} is the number of accidents on segment i during month t, α_i is a segment fixed effect that captures the time-invariant component of accidents on each segment. X_t is a vector of time-varying controls that are measured at the city level. It includes controls for secular changes in driver behavior across the time series with a linear time trend and two covariates that capture aggregate changes in driving behavior during the period: (1) the log of fuel sales in the State of São Paulo and (2) the log of the total number of speed monitoring cameras in São Paulo. The variable D_{it}^{q} is an indicator for the number of q quarters relative to i's initial treatment ($q = 1$ is the quarter of initial treatment). We cap observations on all segments at the sixth relative quarter, which ensures that all segments have the same time in treatment. The sixth quarter is our longest-term estimate and reflects our best estimate of the longer-run effect of the policy. Standard errors are clustered at the road level (202 clusters).

The primary coefficients of interest are the γ_q terms, which measure changes in the number of accidents on a treated segment in each of the quarters following the treatment of segment (i).[30] C_{it} is an indicator for the presence of a speed monitoring camera on segment i during month t, such that ζ estimates the change in accidents on road segments where camera-based enforcement is initiated during the study period.[31] The interaction term $C_{it} \cdot SLR_{it}$ is an indicator for whether the speed limit reduction policy occurred on a segment that also received camera-based enforcement, such that the coefficient η measures the interaction between the speed limit reduction and the onset of camera-based speed enforcement. Because the policy was partially reversed in January 2017, we restrict our sample to December 2016 to isolate the effects of the speed limit reduction, though we estimate and discuss an extended version of this model that includes the post-reversal period in Appendix B.

Should congested cities reduce their speed limits? Evidence from São Paulo, Brazil

By: Dr. Amanda Ang et al.



Air Pollution from Agricultural Fires Increases Hypertension Risk

By: Hemant Pullabhotla and Dr. Mateus Souza

Abstract: In many parts of the developing world, farmers widely use deliberate fires to burn vegetation and clear land to plant crops. These agricultural fires, however, are known to be associated with health costs due to increased air pollution. We contribute to underpinning the associated health cost estimates by studying the effects of these fires on hypertension risk. Despite being one of the leading causes of mortality globally, there is little direct evidence on how hypertension risk changes with exposure to pollution from agricultural fires. To overcome common data and empirical challenges in this setting, we match blood pressure readings from nearly 784,000 individuals across India with satellite data on 1.2 million agricultural fires, wind direction realizations, and local ambient air pollution. We find that the incidence of hypertension increases by 1.8% for each standard deviation increase in the number of upwind fires observed one day before the blood pressure readings. We find that the impact is stronger among older males, smokers, individuals that were already on blood pressure medication, and individuals belonging to socially marginalized groups. Our estimates suggest that agricultural fires in India lead to hypertension-related additional mortality, associated with USD 9 billion annually in costs.

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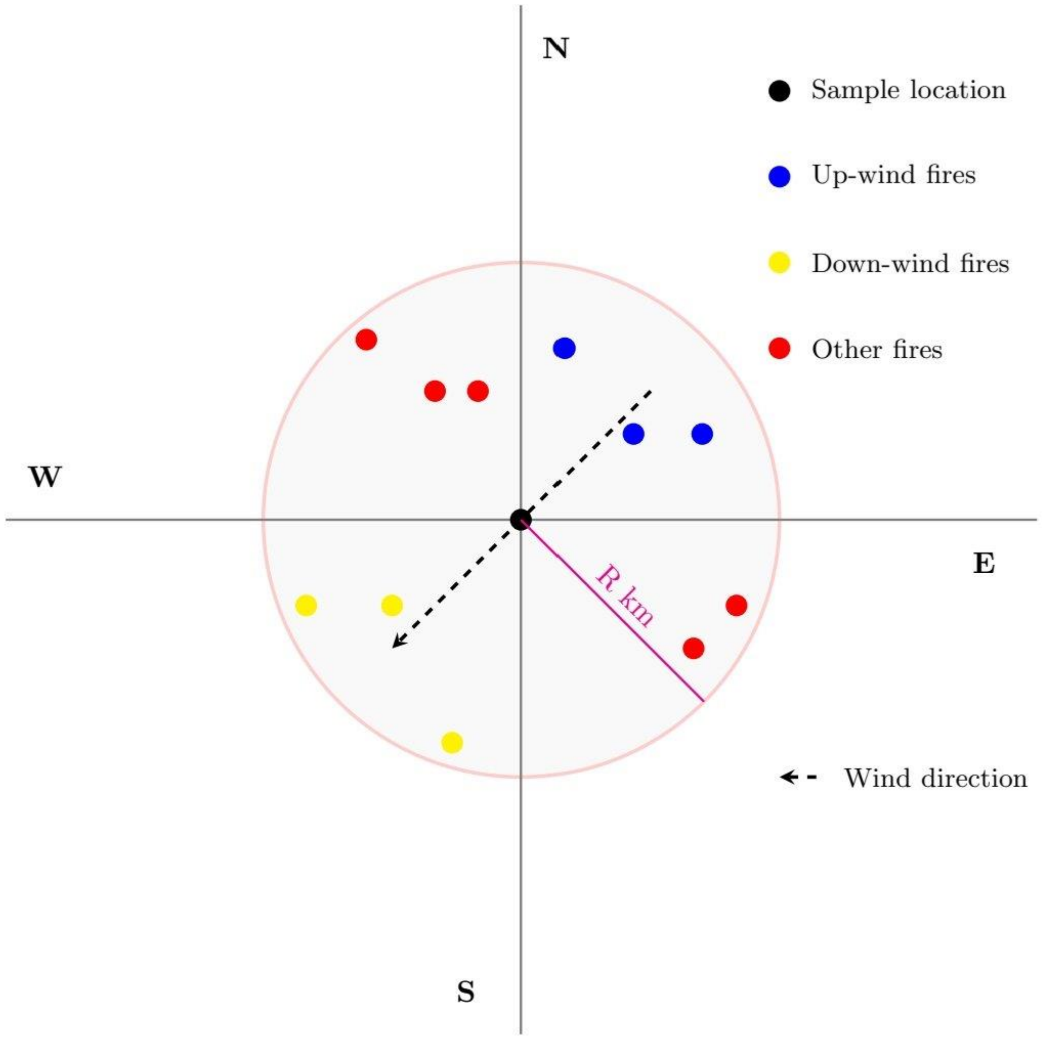

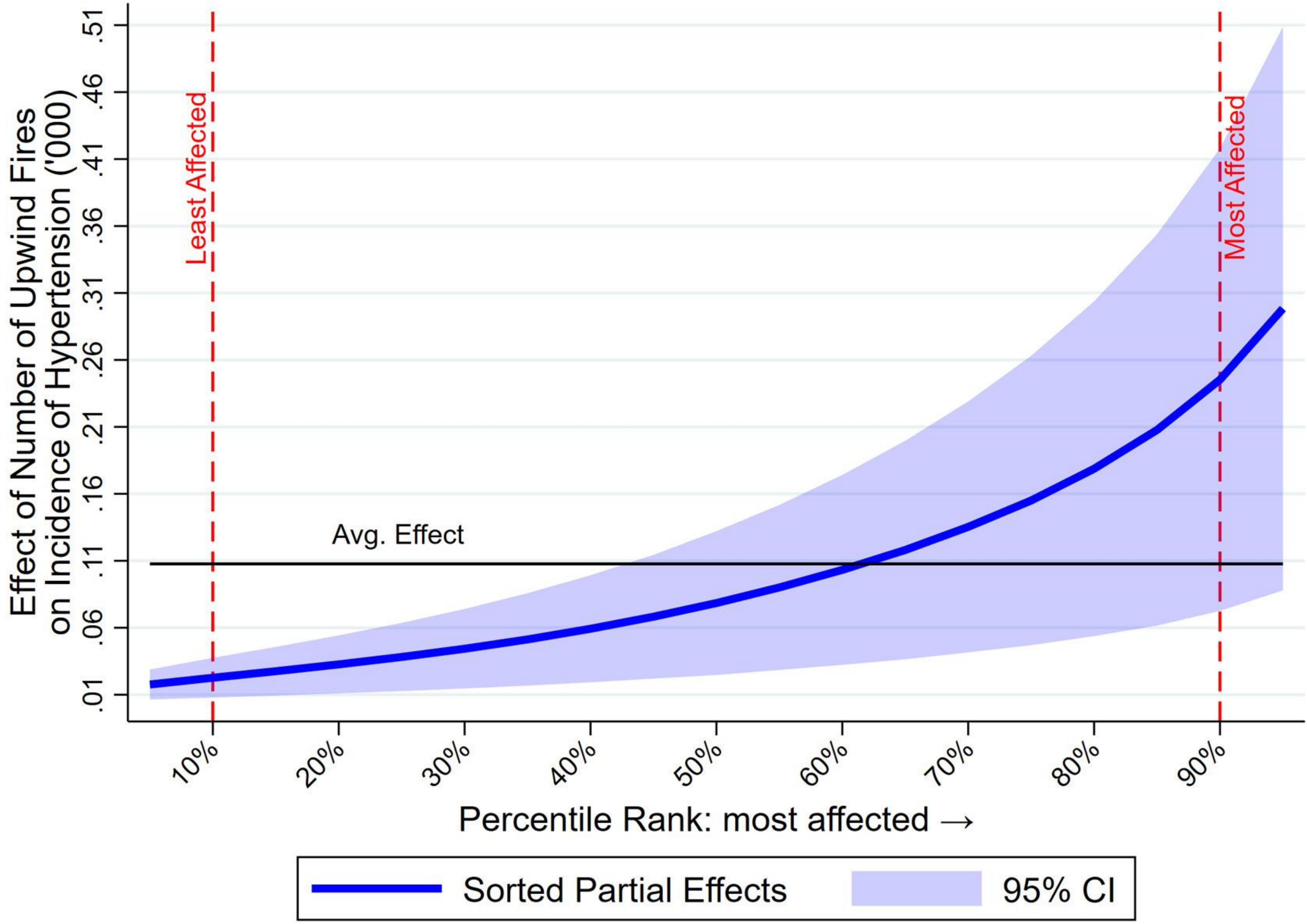

Next Week

- Tuesday, Sept 6th is on a "Monday" Schedule
  - Wrapping up Module 1 with a class on positive versus normative notions in applied economics

- Wednesday, Sept 7th is a regularly scheduled class
  - Beginning Module 2 with econometrics and treatment effects

- Saturday, Sept 10th your first reflection post is due