Disease, Disparities, and Development: Evidence from Chagas Disease in Brazil

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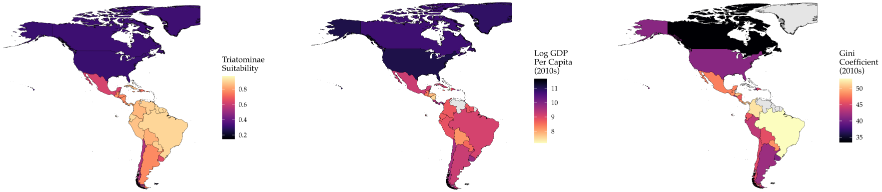
Chagas Disease, Income, and Inequality in the Americas



(a) Vector Suitability (b) Log GDP per Capita (c) Gini Coefficient

LatAm: Highly unequal region, large racial element to disparities

Chagas Disease: Only found in Latin America

- "Neglected disease of poor, rural, and forgotten populations" (Coura and Viñas, 2010; Houweling et al., 2016)

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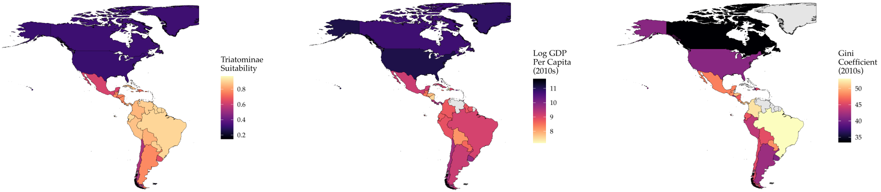

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→ What role has Chagas Disease played in disparities and underdevelopment in the region?

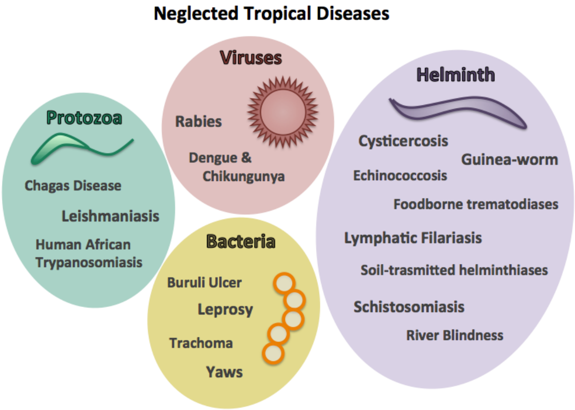
Global Correlations

	Log GDP per Capita		Gini Coefficient	
	(1)	(2)	(3)	(4)
Chagas Suitability $\times$ $\mathbf{1}$(Americas)	-0.02**	-0.03**	0.24***	0.25***
	(0.01)	(0.01)	(0.07)	(0.07)
Chagas Suitability	-0.00	-0.00	0.05*	0.04
	(0.01)	(0.01)	(0.03)	(0.03)
Continent FEs	Y	Y	Y	Y
Geography Controls	Y	Y	Y	Y
Disease Controls	N	Y	N	Y
Observations	207	207	155	155
Mean	8.91	8.91	37.16	37.16
SD	1.48	1.48	7.51	7.51
Adjusted R^2	0.524	0.525	0.588	0.596

Notes: The unit of observation is a country. Robust standard errors in parentheses. In columns (1) and (2), the outcome variable is the log of the average GDP (in $1,000s) per capita in 2019 from the World Bank Development Indicators. In columns (3) and (4), the outcome variable is the most recently reported Gini coefficient (scaled by 100) between 2010 and 2019 from the World Bank Development Indicators. Chagas Suitability is a 0 to 100 measure of the ecological suitability for Chagas vectors from Eberhard et al. (2020). $\mathbf{1}$(Americas) is an indicator variable equal to one if a country is in North or South America. Geography Controls include centroid longitude, centroid latitude, average rainfall, average temperature, elevation, area, and agricultural suitability. Disease Controls include malaria suitability and tsetse fly suitability. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Only in Americas: Chagas associated with ↓ income, ↑ inequality

Learning about Global Development More Broadly



NTDs: Like Chagas Disease, many mostly cause *morbidity* and have both acute and chronic phases

→ Learn about broader economic consequences of combating NTDs

- Policymakers' priorities are to combat *mortality* ▸ US funding

This Paper

Question: Does controlling (neglected tropical) disease have:

1. Short-run economic benefits?
2. Long-run benefits beyond individuals' labor market returns?

Setting: Brazil reduced Chagas Disease transmission (1984-89)

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- *Disease of poverty*: Vector lives in cracks in roof and walls—more likely in housing of non-white Brazilians

  → Reduce income inequality and racial disparities?

Roadmap

1. **Using Vector Control (1984-89) to Define Treatment**
   - Munis: Never-infested (C) vs vector eliminated by 1989 (T)
2. **Short-Run Results**
   - Diff-in-diff: $\uparrow$ adult employment, maybe more for non-white
3. **Long-Run Results: Labor Market Outcomes**
   - Diff-in-diff: $\uparrow$ non-white incomes, $\downarrow$ income inequality
4. **Long-Run Results: Public Health Care System**
   - Triple-diff: $\downarrow$ circulatory disease hospital care vs other causes

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Chagas Disease Vector Control, 1984-89

(a) 1975-83 entomological surveys

- 36% of territory, 4.2% seroprevalence

Map source: Silveira (2011)

Chagas Disease Vector Control, 1984-89

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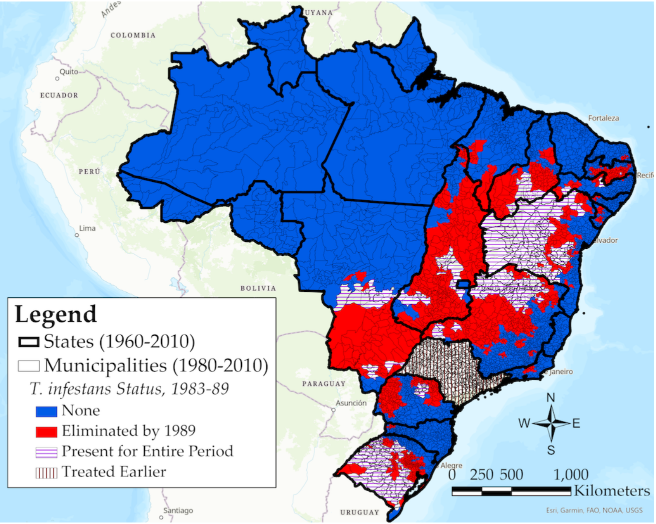
Map source: Silveira (2011)

(b) 1989 entomological survey

- Spraying → reduce vector's presence

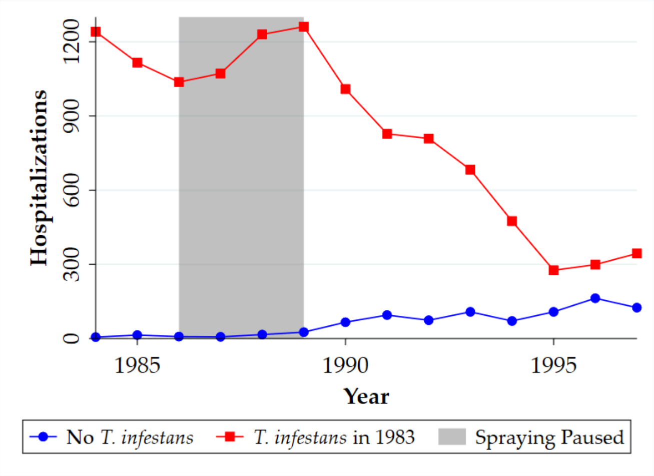
Map source: Coura & Dias (2009)

Defining Treatment and Control Groups



Municipalities: Treatment (eliminated by 1989) vs control (none)

Effect of Spraying on Chagas Disease



Hospitalizations for Acute Chagas Disease, 1984-97

Validation: Reduction in hospitalizations follows spraying

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Short-Run Analysis

Data: National Household Sample Surveys (PNAD)

- 11 of 18 years in 1982 to 1999 (missing '83-'85, '88, '91, '94, '96)
- Municipality not observable, only state reported

Dynamic diff-in-diff: Pre- and post-spraying comparisons across states with varying levels of *T. infestans* exposure

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$$y_{i,s,t} = \alpha_s + \gamma_t + \sum_{k \neq 1986} \tau_k \cdot \left(\mathbb{P}[Treat]_s \cdot \mathbf{1}[t = k] \right) + \mathbf{X}_i \beta + \epsilon_{i,s,t}$$

- Two-way FE: State s and year t
- $\mathbb{P}[Treat]_s$: Probability in 1980 census that individual of given sex and race in s resides in treatment municipality
- $\mathbf{1}[t = k]$: Indicator for observation from given year k
- $\mathbf{X}_i$: Age, age squared, racial category FE, female FE
- $\epsilon_{i,s,t}$: SE clustered by states (only 24 $\rightarrow$ wild cluster bootstrap CS)

Considerations for Short-Run Analysis

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Coefficients of interest τ_k: Measure difference in outcome in given year as probability of residing in treatment municipality goes from 0 to 1, relative to size of that difference in 1986

- Probability distribution: 25th percentile 0, 75th percentile 0.246

→ Frame results as moving from 25th to 75th percentile: Policy-relevant magnitudes are 1/4 of estimated coefficients

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Omitting 1986: Not actually final pre-treatment year (1983)

- 1986 only "lightly treated" $\rightarrow$ allows some assessment of pre-trends
- Conservative: Assume all municipalities treated in 1984

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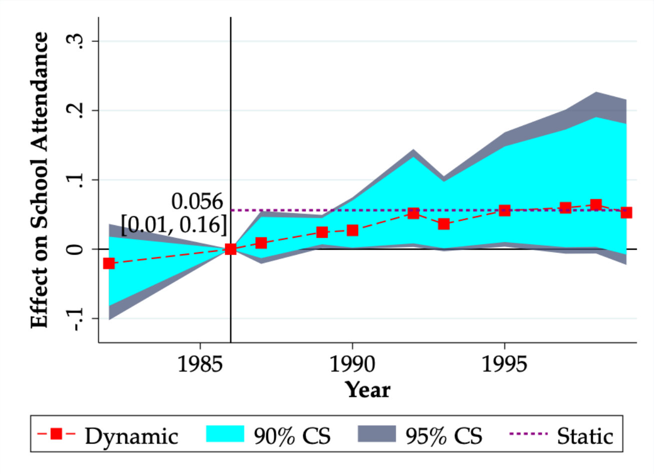
Static diff-in-diff: Interaction term is $\tau \cdot \left(\mathbb{P}[Treat]_s \cdot \mathbf{1}[t > 1986]\right)$

- Complementary: More precise but imposes single value across years

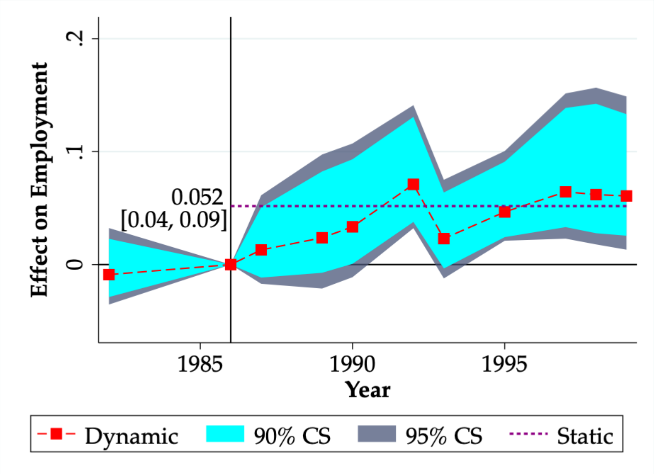
Short-Run Effects on Children's Attendance (Ages 8-18)



Notes: Regression uses 836,331 observations. Static estimate magnitude is displayed next to the dotted purple line with 90-percent wild bootstrap confidence set below it in brackets. In the pre-treatment year, 74.7 percent of children attended school.

Attendance effect for p25 → p75: ↑ 1.4 p.p. (1.9%), but noisy

Short-Run Effects on Adults' Employment (Ages 35-50)



Notes: Regression uses 1,198,480 observations. Static estimate magnitude is displayed next to the dotted purple line with 90-percent wild bootstrap confidence set below it in brackets. In the pre-treatment year, 67.0 percent of adults were employed.

Employment effect for p25 $\rightarrow$ p75: $\uparrow$ 1.3 p.p. (1.9%), precise

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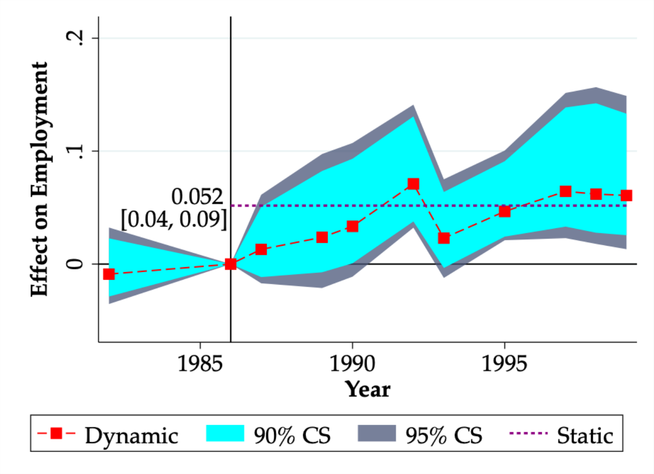

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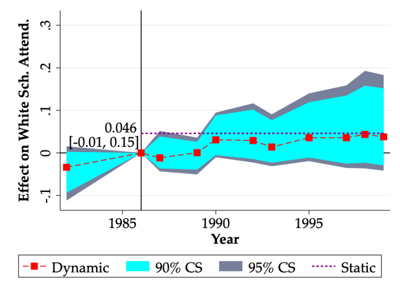
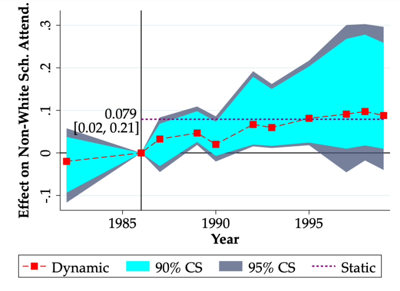
→ **Effects on adults *already in the labor force!***

Differential Short-Run Effects by Race? Attendance



(a) White, Ages 8-18



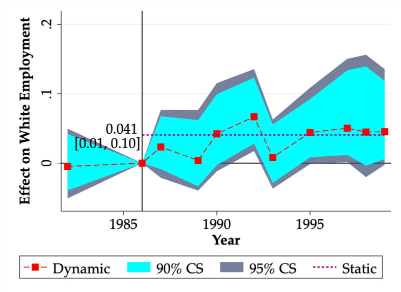
(b) Non-White, Ages 8-18

Notes: Regressions use 389,414 observations in (a) and 446,017 in (b). Static estimates magnitudes are displayed next to the dotted purple lines with 90-percent wild bootstrap confidence set below them in brackets. In the pre-treatment year, 77.7 percent of white and 74.1 percent of non-white children attended school.

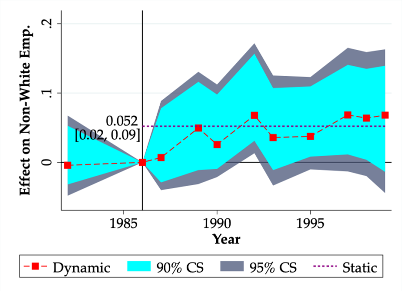
Attendance effects for p25 → p75: ↑ 1.1 p.p. (1.5%) for white, ↑ 1.9 p.p. (2.6%) for non-white

→ Probably some small attendance effect, maybe larger for non-white

Differential Short-Run Effects by Race? Employment



(a) White, Ages 35-50



(b) Non-White, Ages 35-50

Notes: Regressions use 648,336 observations in (a) and 550,144 in (b). Static estimates magnitudes are displayed next to the dotted purple lines with 90-percent wild bootstrap confidence set below them in brackets. In the pre-treatment year, 67.2 percent of white and 66.9 percent of non-white adults were employed.

Employment effects for p25 → p75: ↑ 1.0 p.p. (1.5%) for white, ↑ 1.3 p.p. (1.9%) for non-white

→ Small but clear employment effect, maybe larger for non-white

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Long-Run Analysis: Labor Market Outcomes

Data: IPUMS 10-percent sample of 2010 census

- 2/3 of sample lives in municipality of birth
- For movers, can only observe state of birth

Dynamic diff-in-diff: Comparing pre- and post-spraying cohorts across probabilities of having been born in treatment municipality

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- Two-way FE: Municipality m, birth cohort c
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- Same as before: Individual-level controls, cluster SE by state

Considerations for Long-Run Analysis

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$\rightarrow$ Frame results as moving from 50th (0) to 75th percentile (0.247): Policy-relevant magnitudes are 1/4 of estimated coefficients

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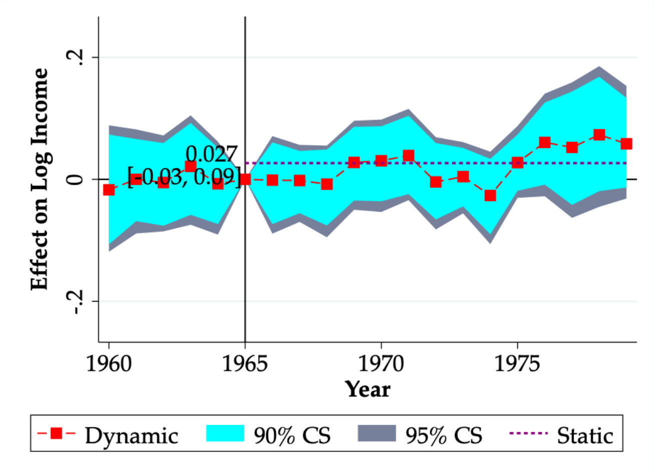
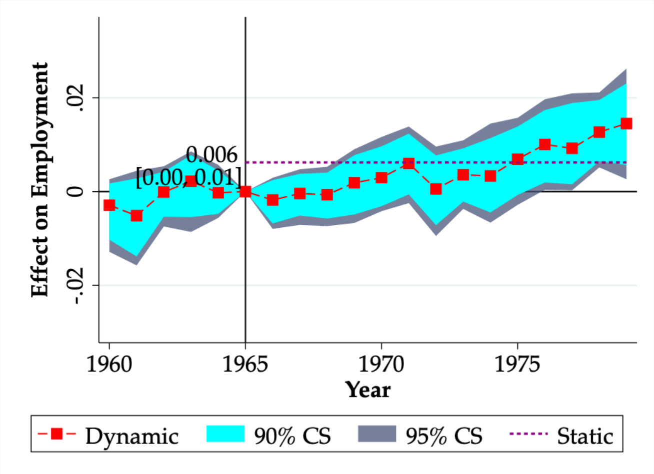
Long-Run Effects on Log Monthly Income



Notes: Regression uses 3,872,397 observations. Static estimate magnitude is displayed next to the dotted purple line with 90-percent wild bootstrap confidence set below it in brackets. For pre-treatment cohorts, mean log monthly income was 6.242.

Income effect for p50 → p75: ↑ 0.7%, but noisy

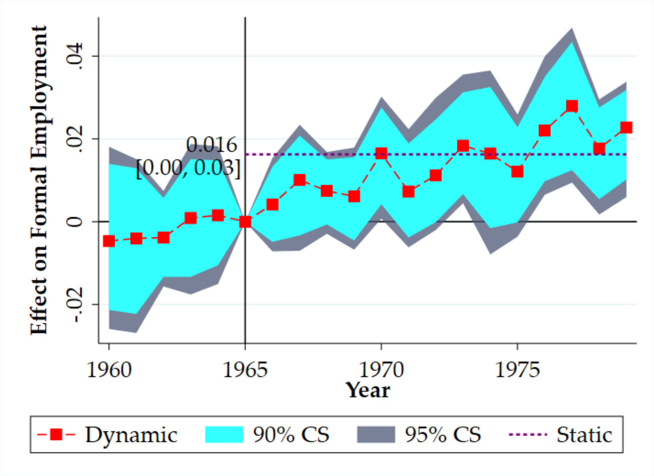
Long-Run Effects on Employment (Conditional on LFP)



Notes: Regression uses 4,075,538 observations. Static estimate magnitude is displayed next to the dotted purple line with 90-percent wild bootstrap confidence set below it in brackets. For pre-treatment cohorts, 91.2 percent of those in the labor force were employed.

Employment effect for p50 → p75: ↑ 0.1 p.p. (0.2% or 1.7%)

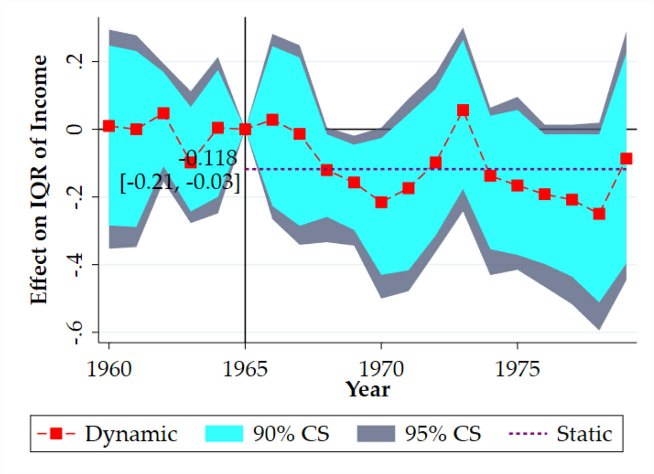
Long-Run Effects on Formal Employment



Notes: Regression uses 3,011,605 observations. Static estimate magnitude is displayed next to the dotted purple line with 90-percent wild bootstrap confidence set below it in brackets. For pre-treatment cohorts, 46.8 percent of those employed were in formal jobs.

Formal employment effect for p50 → p75: ↑ 0.4 p.p. (0.8%)

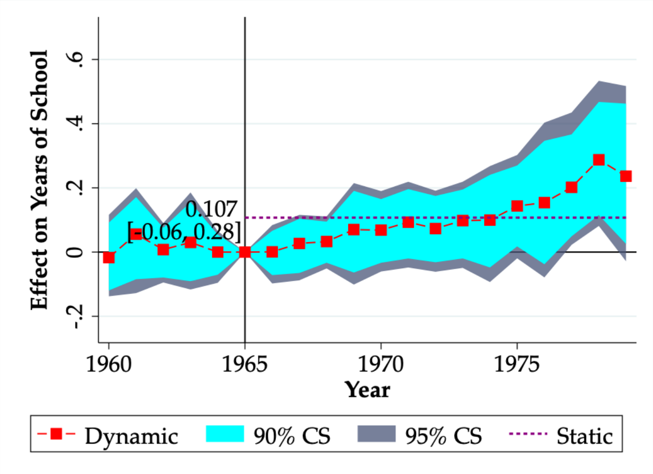
Long-Run Effects on Income Inequality



Notes: Observations are municipalities of birth. Regression uses 63,232 observations. Static estimate magnitude is displayed next to the dotted purple line with 90-percent wild bootstrap confidence set below it in brackets. For pre-treatment cohorts, the interquartile range of log income was 3.47.

Income IQR effect for p50 → p75: ↓ 0.03 log points (0.8%)

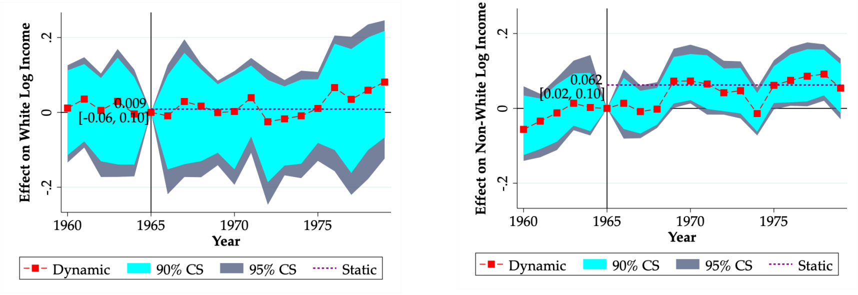
Long-Run Channel: Educational Attainment?



Notes: Regression uses 5,299,001 observations. Static estimate magnitude is displayed next to the dotted purple line with 90-percent wild bootstrap confidence set below it in brackets. For pre-treatment cohorts, mean years of schooling was 6.02.

Schooling effect for p50 → p75: ↑ 0.03 years (0.4%), but noisy

Reducing Long-Run Racial Disparities: Income



(a) White

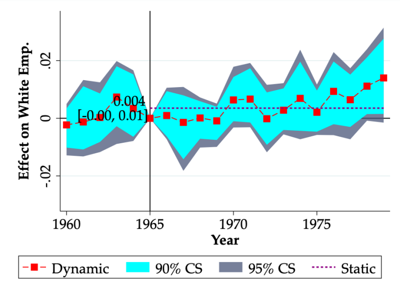
(b) Non-White

Notes: Regressions use 1,964,673 observations in (a) and 1,907,724 in (b). Static estimates magnitudes are displayed next to the dotted purple lines with 90-percent wild bootstrap confidence set below them in brackets. For pre-treatment cohorts, mean log monthly incomes were 6.58 for whites and 5.86 for non-whites.

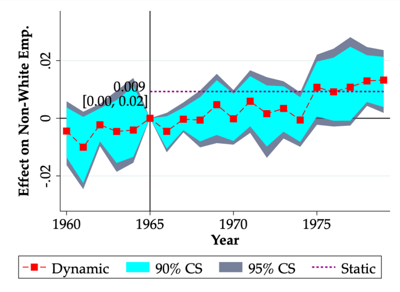
Income effects for p50 → p75: ↑ 0.2% for whites, ↑ 1.5% and precise for non-whites

→ **Triple-diff:** 1.6% larger increase for non-white men (90% wild cluster bootstrap CS: -0.004, 0.029)

Reducing Long-Run Racial Disparities: Employment



(a) White



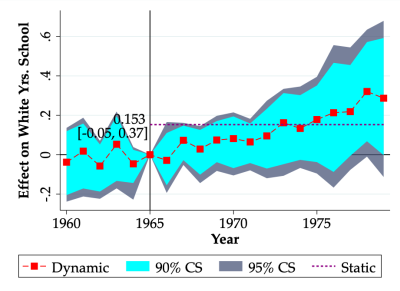
(b) Non-White

Notes: Regressions use 2,044,592 observations in (a) and 2,030,946 in (b). Static estimates magnitudes are displayed next to the dotted purple lines with 90-percent wild bootstrap confidence set below them in brackets. For pre-treatment cohorts, 93.6 percent of whites in the labor force and 88.5 percent of non-whites in the labor force were employed.

Employment effects for p50 $\rightarrow$ p75: $\uparrow$ 0.1 p.p. for whites (0.1% or 1.5%), $\uparrow$ 0.2 p.p. for non-whites (0.3% or 2.0%)

Triple-diff: Not different for non-whites (same with formal emp.)

Long-Run Channel: Educational Attainment?



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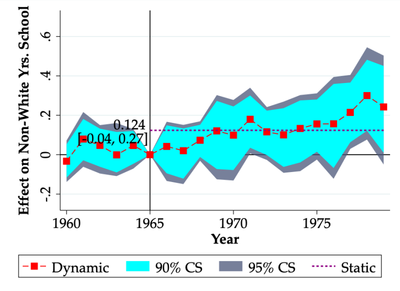


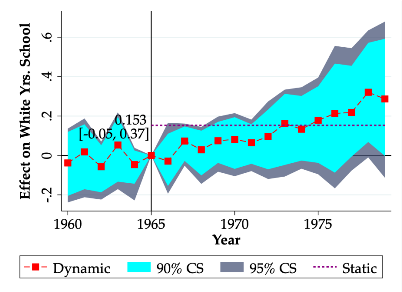
(b) Non-White

Notes: Regressions use 2,568,352 observations in (a) and 2,730,649 in (b). Static estimates magnitudes are displayed next to the dotted purple lines with 90-percent wild bootstrap confidence set below them in brackets. For pre-treatment cohorts, mean years of schooling were 6.91 for whites and 5.10 years for non-whites.

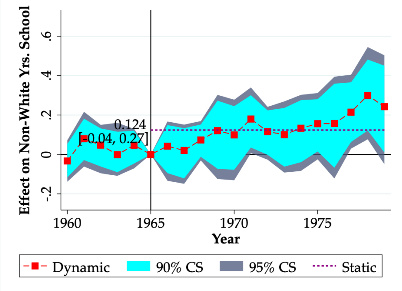
Basically equally small schooling effects: 0.04 years for white (0.5%), 0.03 years for non-white (0.6%)

→ Maybe some small effect on schooling for each group

Long-Run Channel: Educational Attainment?



(a) White



(b) Non-White

Notes: Regressions use 2,568,352 observations in (a) and 2,730,649 in (b). Static estimates magnitudes are displayed next to the dotted purple lines with 90-percent wild bootstrap confidence set below them in brackets. For pre-treatment cohorts, mean years of schooling were 6.91 for whites and 5.10 years for non-whites.

Explaining non-white income gains: Psacharopoulos and Patrinos (2018) Mincer return 15.7% $\cdot$ 0.03 years = 0.5% of 1.5% increase

$\rightarrow$ Suggests role for cardiovascular effects in adulthood

Roadmap

1. **Using Vector Control (1984-89) to Define Treatment**
   - Munis: Never-infested (C) vs vector eliminated by 1989 (T)

2. **Short-Run Results**
   - Diff-in-diff: $\uparrow$ adult employment, maybe more for non-white

3. **Long-Run Results: Labor Market Outcomes**
   - Diff-in-diff: $\uparrow$ non-white incomes, $\downarrow$ income inequality

4. **Long-Run Results: Public Health Care System**
   - Triple-diff: $\downarrow$ circulatory disease hospital care vs other causes

Examining Public Health Care System Outcomes

Unified Health Service (SUS): World's largest government-run health care system

- Consumes 4% of GDP, universal coverage but mostly used by poor
- Circulatory diseases: Since 2010, accounted for more than one-tenth of hospitalizations paid for by SUS (850,000/year), one-fifth of spending on hospital care (BRL 1.5 billion/year, or 0.1% of GDP)

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→ **Solution:** Triple-differences approach comparing outcomes due to circulatory disease (affected by chronic Chagas Disease) to those due to all other causes (not affected by chronic Chagas Disease)

Long-Run Analysis: Public Health Care System

Data: Hospitalizations, person-days in hospital, and spending on hospital care covered by SUS for 1984-2019 (SIH/SUS)

Dynamic triple-diff: Comparing "pre" and "post" years across states with varying levels of *T. infestans* exposure and across disease categories (circulatory vs non-circulatory)

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$$
\begin{aligned}
y_{s,t,d} &= \alpha_{s,t} + \gamma_{t,d} + \delta_{s,d} \\
&\quad + \sum_{k \neq 1999} \tau_k \cdot \left(\mathbb{P}[Treat]_s \cdot \mathbf{1}[t = k] \cdot \mathbf{1}[d = circ] \right) + \epsilon_{s,t,d}
\end{aligned}
$$

- FE: Interactions for state s, year t, and disease category d
- $\mathbb{P}[Treat]_s$: Probability in 1980 census that individual in s resides in treatment municipality
- $\mathbf{1}[d = circ]$: Indicator for d being circulatory disease
- Same as before: Cluster SE by state

Considerations for Long-Run Analysis

$$y_{s,t,d} = \alpha_{s,t} + \gamma_{t,d} + \delta_{s,d} + \sum_{k \neq 1999} \tau_k \cdot \left(\mathbb{P}[Treat]_s \cdot \mathbf{1}[t=k] \cdot \mathbf{1}[d=circ] \right) + \epsilon_{s,t,d}$$

Coefficients of interest τ_k: Measure difference in outcome for circulatory diseases in given year as probability of residing in treatment municipality goes from 0 to 1, relative to size of that difference for 1999 (10 years after spraying finished) for non-circulatory diseases

→ Frame results as moving from 25th (0) to 75th percentile (0.263): Policy-relevant magnitudes are 1/4 of estimated coefficients

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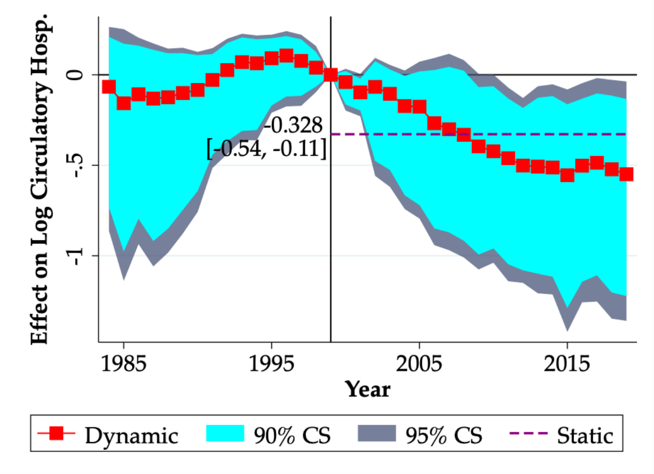
Static triple-diff: $\tau \cdot \left(\mathbb{P}[Treat]_s \cdot \mathbf{1}[t > 1999] \cdot \mathbf{1}[d = circ]\right)$

- Complementary: More precise but imposes single value across years

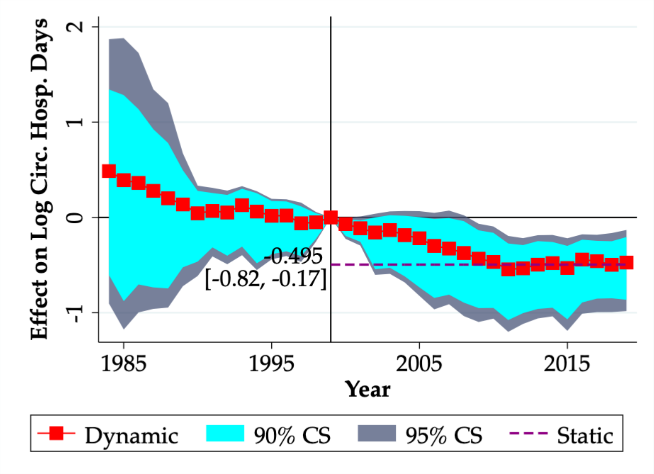
Long-Run Effects on Log Circ. Disease Hospitalizations



Notes: Regression uses 1,512 state-year-disease category observations. Static estimate magnitude is displayed next to the dotted purple line with 90-percent wild bootstrap confidence set below it in brackets. For pre-treatment years, mean log hospitalizations was 9.94 for circulatory diseases and 12.4 for non-circulatory diseases.

Hospitalization effect for p25 $\rightarrow$ p75: $\downarrow$ 8.5%

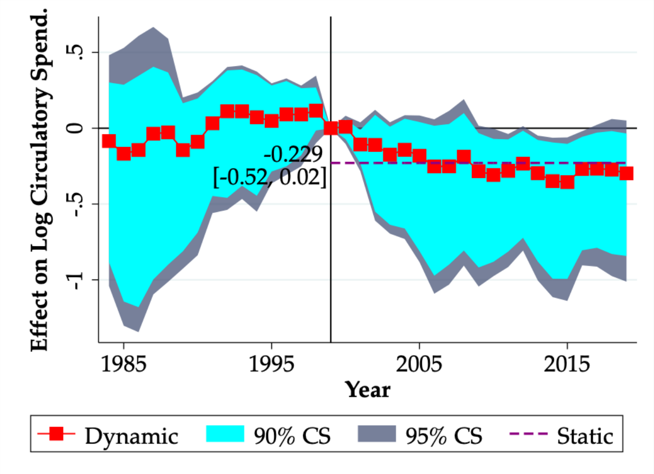
Long-Run Effects on Log Circ. Disease Hospital Days



Notes: Regression uses 1,512 state-year-disease category observations. Static estimate magnitude is displayed next to the dotted purple line with 90-percent wild bootstrap confidence set below it in brackets. For pre-treatment years, mean log person-days in the hospital was 11.9 for circulatory diseases and 14.2 for non-circulatory diseases.

Hospital days effect for p25 $\rightarrow$ p75: $\downarrow$ 13.0%

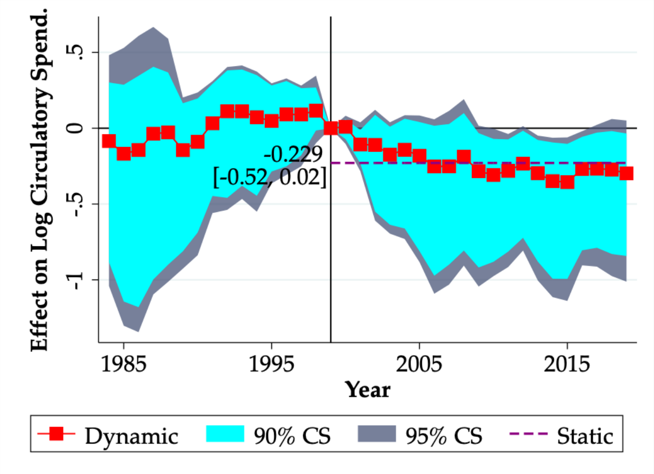
Long-Run Effects on Log Circ. Disease Hospital Spending



Notes: Regression uses 1,512 state-year-disease category observations. Static estimate magnitude is displayed next to the dotted purple line with 90-percent wild bootstrap confidence set below it in brackets. For pre-treatment years, mean log spending was 12.2 for circulatory diseases and 18.1 for non-circulatory diseases.

Hospital spending effect for p25 → p75: ↓ 6.0%, but slightly noisy

Long-Run Effects on Log Circ. Disease Hospital Spending



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Back of the envelope: SUS spends 0.1% of GDP on cardiac hospital care per year $\Longrightarrow$ $\downarrow$ 6% saved 0.006% of GDP per year

Conclusion

Impacts of Chagas Disease control in Brazil:

- Short run: Increased adults' employment
- Long run: Increased incomes for non-white Brazilians, reducing income inequality; reduced burden of circulatory disease on public health care system, helping to improve country's fiscal health

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Role of disease in development: More than just schooling!

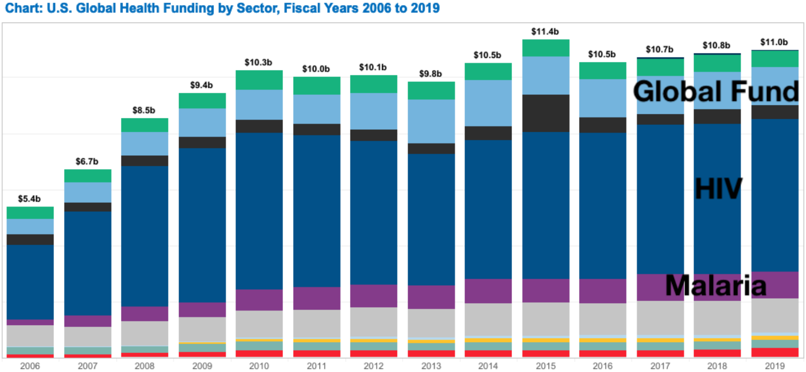
- Schooling's benefits heavily discounted b/c not realized for years
- Benefit side of ledger could be understated, so some disease control programs that should be undertaken might not be

→ We should look disease control's benefits in other domains relevant for development, like those above

Roadmap

5. **Appendix Slides**

Appendix: Diseases in US Global Health Funds, 2006-19



Source: Kaiser Family Foundation (2021)

High mortality: HIV ($5.4B), malaria ($0.9B), Global Fund ($1.4B)

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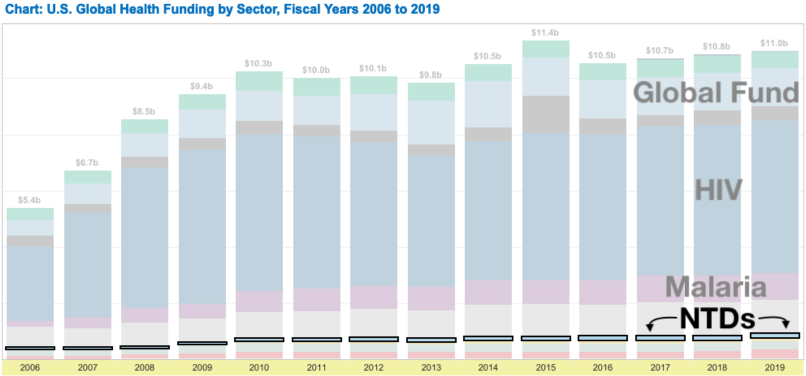

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High mortality: HIV ($5.4B), malaria ($0.9B), Global Fund ($1.4B)

High morbidity: NTDs ($0.1B for all 20 diseases)

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