Neighborhood opportunities and Discrimination

Urban Economics

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Agenda

Residential Location Patterns: US

Table 1
Poverty in cities and suburbs

Row		Center city resident	Suburban resident
1	All	0.1990	0.0753
2	Northeast	0.2089	0.0599
3	Midwest	0.1984	0.0565
4	South	0.1865	0.0744
5	West	0.1895	0.1031

Residential Location Patterns: Atlanta, Phoenix, Los Angeles

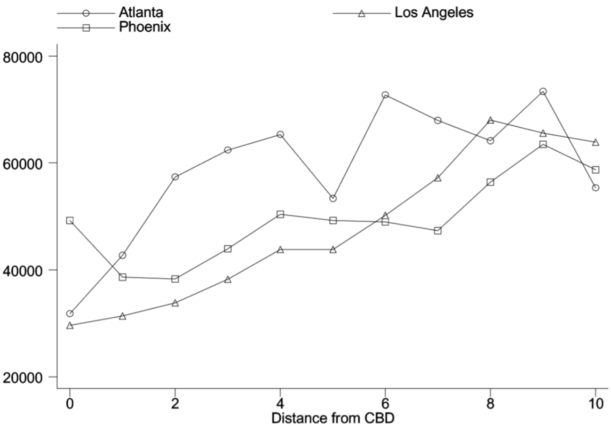


Fig. 2. Income and distance from the CBD in three new cities.

Residential Location Patterns: Europe

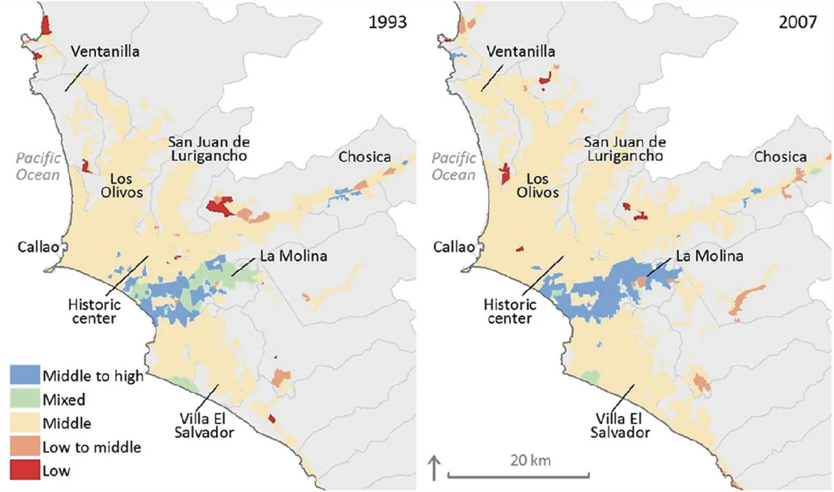
Table 1
Central-city vs. suburban incomes in France and the US

	Household income[a]	
Case	Central-city	Suburbs
Ile de France (Paris metro area)	124 000 Fr.[b]	106 000 Fr.
Province (other metro areas)	76 000 Fr.	84 000 Fr.
France (all metro areas)	84 000 Fr.	82 000 Fr.
Detroit (metro area)	$20 207	$40 084
U.S. (all metro areas)	$26 727	$26 314

[a] Household incomes are the 1990 average value in France and the 1989 median value in the U.S. The French data are from Nicot (1996), and the U.S. data are from the 1990 Census.
[b] The current franc-dollar exchange rate is approximately 6 francs per dollar.

Residential Location Patterns: Lima

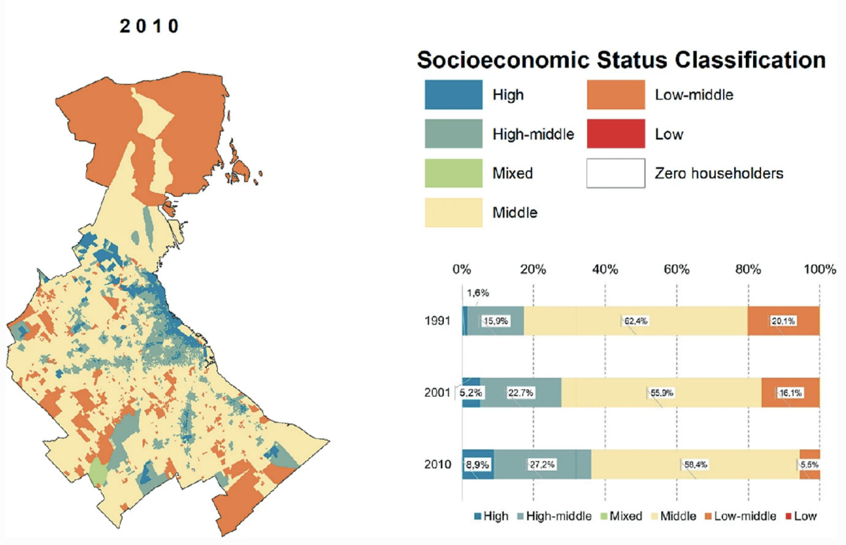
From: Changes in Spatial Inequality and Residential Segregation in Metropolitan Lima



(*Data source* INEI 2009; 2016)

Classification of neighbourhoods by socio-economic composition in Lima.

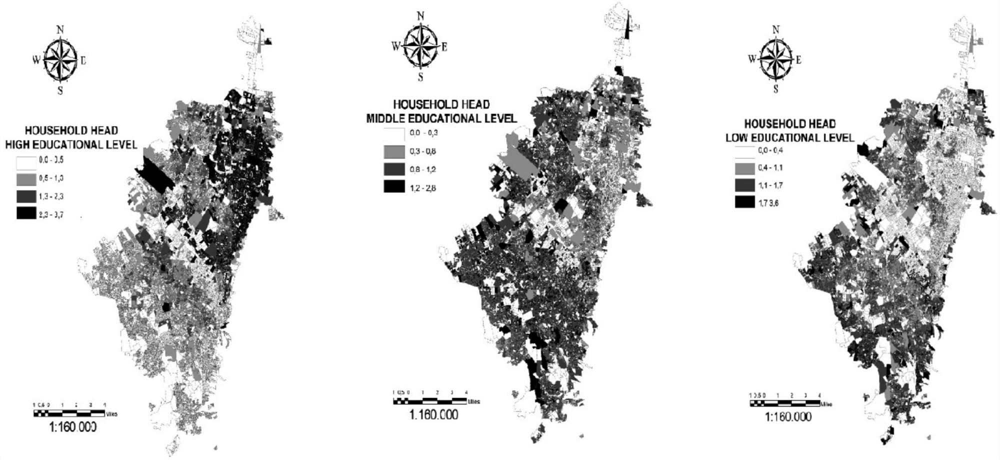
Residential Location Patterns: Buenos Aires



Source Population and housing censuses 1991, 2001, and 2010, INDEC, author's maps

Residential Location Patterns: Bogotá

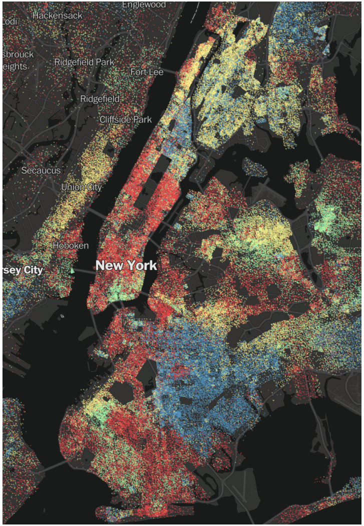
From: Socioeconomic Residential Segregation and Income Inequality in Bogotá: An Analysis Based on Census Data of 2005



Source Elaboration by the authors based on Population Census DANE (2005)

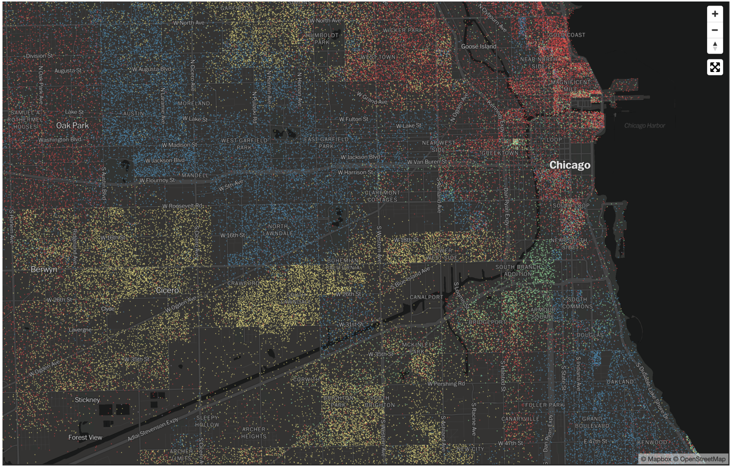
Location quotient for household leader by high, medium, and low education level in Bogotá, 2005.

Residential Location Patterns by Race: NYC



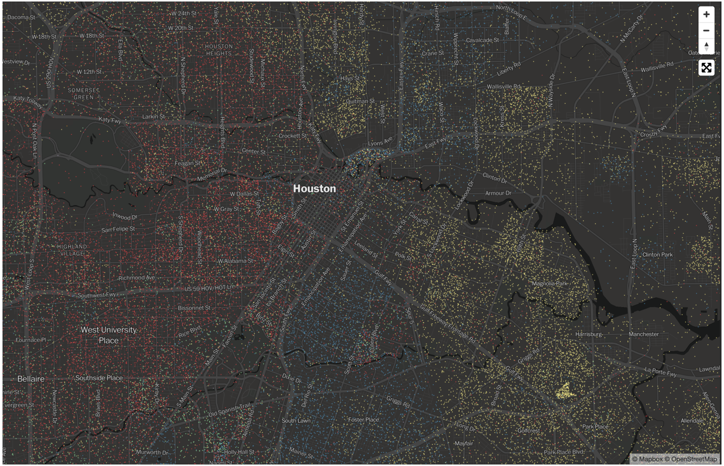
Source: https://www.washingtonpost.com/graphics/2018/national/segregation-us-cities/

Residential Location Patterns by Race: Chicago



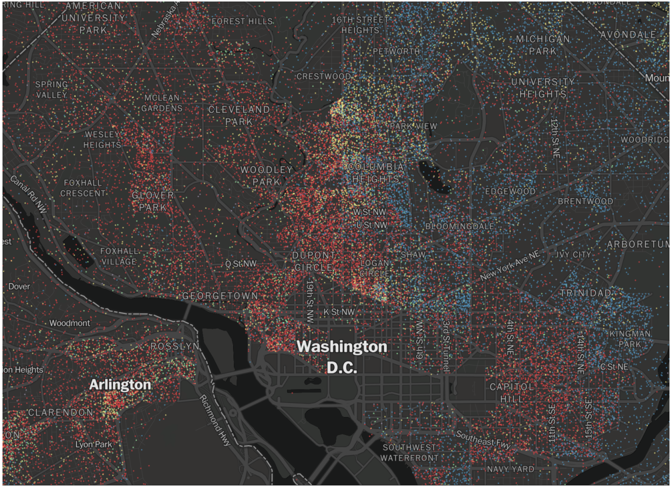
Source: https://www.washingtonpost.com/graphics/2018/national/segregation-us-cities/

Residential Location Patterns by Race: Houston



Source:
https://www.washingtonpost.com/graphics/2018/national/segregation-us-cities/

Residential Location Patterns by Race: Washington, DC



Source: https://www.washingtonpost.com/graphics/2018/national/segregation-us-cities/

Five Strongest Correlates of Upward Mobility

1. Segregation
   - Greater racial and income segregation associated with lower levels of mobility

Neighborhood Effects

- Recent literature has shown that the neighborhood where people live has important implications for individual outcomes.

THE IMPACTS OF NEIGHBORHOODS ON INTERGENERATIONAL MOBILITY I: CHILDHOOD EXPOSURE EFFECTS*

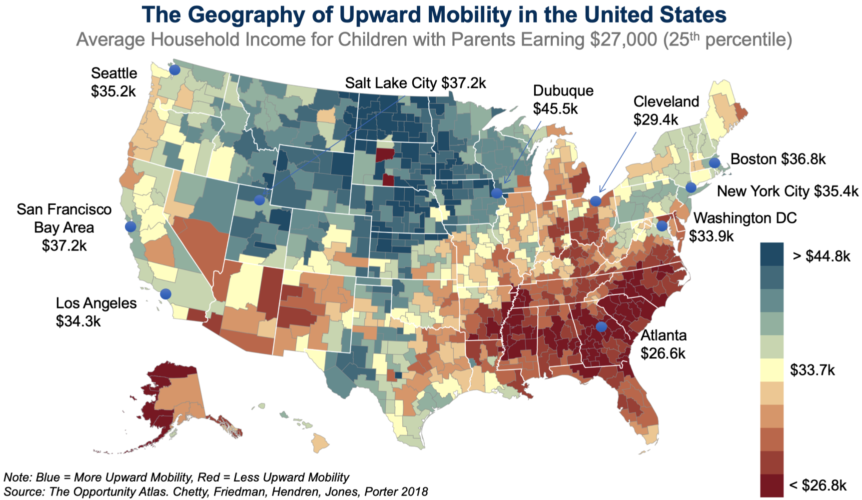
RAJ CHETTY AND NATHANIEL HENDREN

Agenda

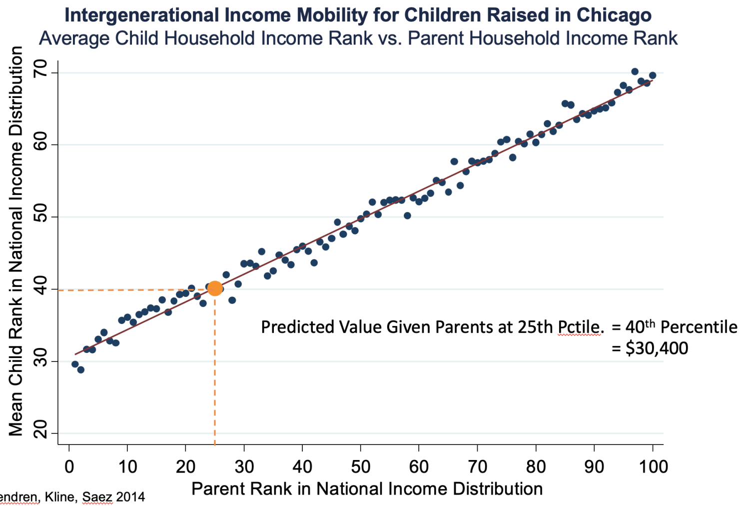
Intro

- How do children's chances of moving up vary across areas in the US?
- Are there some areas where kids do better than others? If so, what lessons can we learn from them?
- Recent studies have used big data to measure how upward mobility varies based on where children grow up
  - The Opportunity Atlas

The Opportunity Atlas

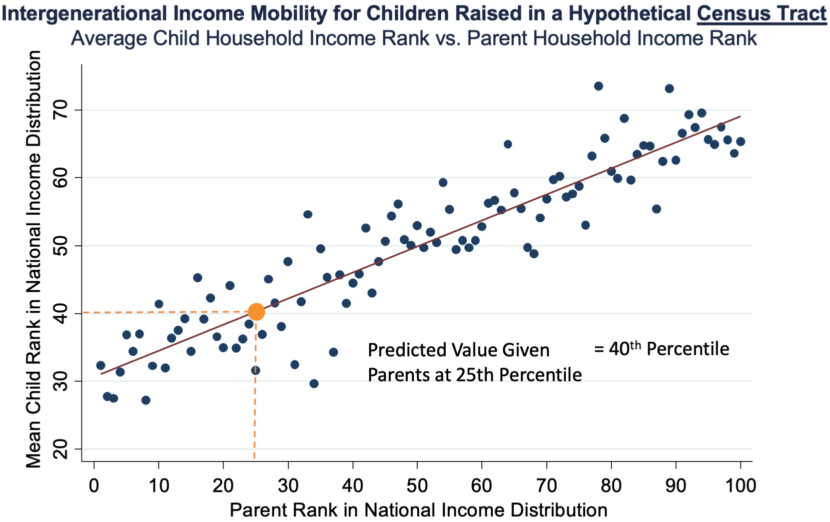


The Opportunity Atlas



Source: Chetty, Hendren, Kline, Saez 2014

The Opportunity Atlas



The Opportunity Atlas
Data Sources and Sample Definitions

- Data sources: Anonymized Census data (2000, 2010, ACS) covering U.S. population linked to federal income tax returns from 1989-2015
- Link children to parents based on dependent claiming on tax returns
- Target sample: Children in 1978-83 birth cohorts who were born in the U.S. or are authorized immigrants who came to the U.S. in childhood
- Analysis sample: 20.5 million children, 96% coverage rate of target sample

The Opportunity Atlas

Measuring Parents' and Children's Incomes in Tax Data

- Parents' household incomes: average income reported on Form 1040 tax return from 1994-2000
- Children's incomes measured from tax returns in 2014-15 (ages 31-37)
- Focus on percentile ranks in national distribution:
  - Rank children relative to others born in the same year and parents relative to other parents

The Opportunity Atlas
Causal Effects of Neighborhoods vs. Sorting

- Two very different explanations for variation in children's outcomes across areas:

The Opportunity Atlas

Causal Effects of Neighborhoods vs. Sorting

- Two very different explanations for variation in children's outcomes across areas:
  1. Sorting: different people live in different places
  2. Causal effects: places have a causal effect on upward mobility for a given person

The Opportunity Atlas

Identifying Causal Effects of Neighborhoods

- Objective is to determine how much a child's potential outcomes would improve on average if he were to grow up in an area where the permanent residents' outcomes are 1 percentile point higher.

The Opportunity Atlas

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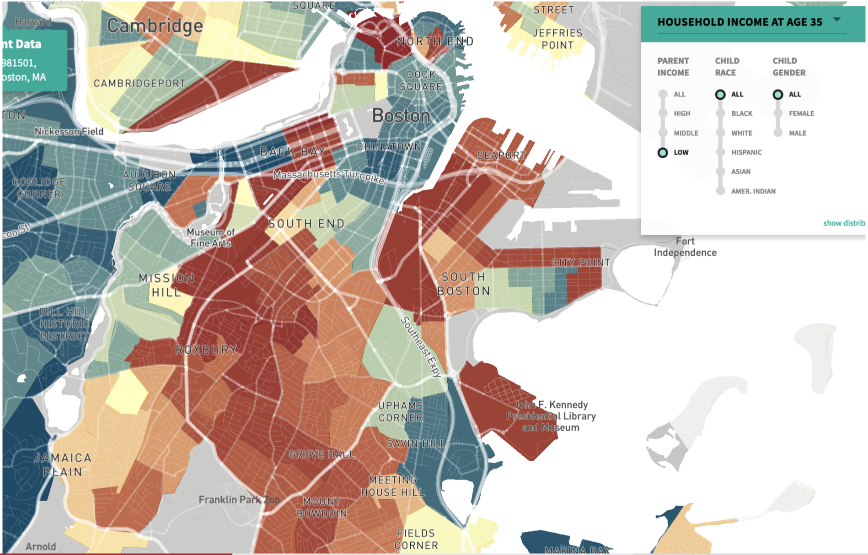
- Ideal experiment?

The Opportunity Atlas
Identifying Causal Effects of Neighborhoods

- Ideal experiment?
- they approximate this experiment using a quasi-experimental design
- Study 3 million families who move across Census tracts in observational data
- Key idea: exploit variation in age of child when family moves to identify causal effects of environment

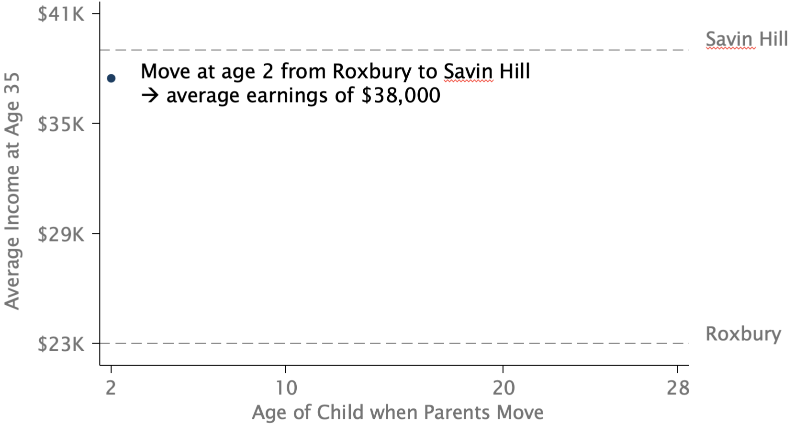
The Opportunity Atlas

Identifying Causal Effects of Neighborhoods



The Opportunity Atlas

Identifying Causal Effects of Neighborhoods



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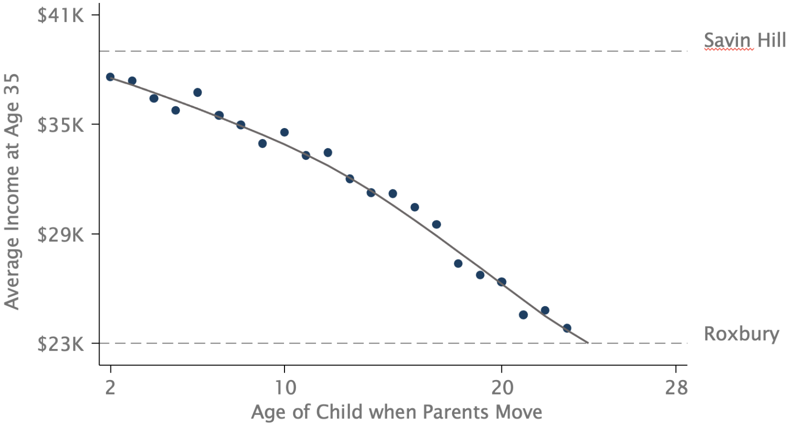
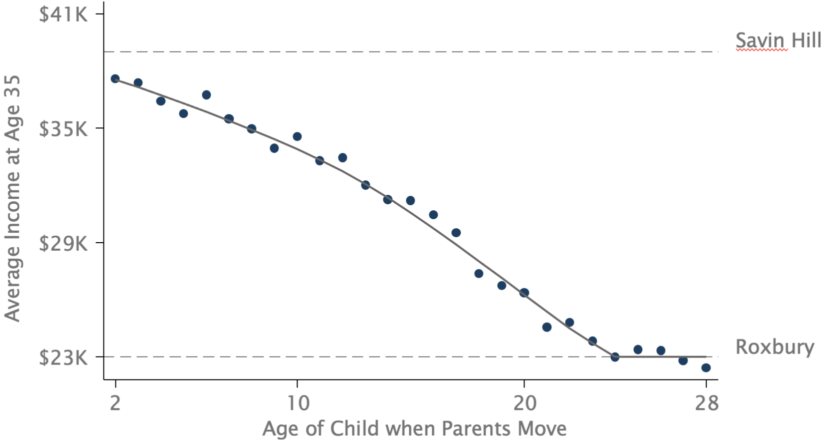
Identifying Causal Effects of Neighborhoods



The Opportunity Atlas

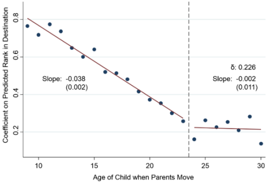
Identifying Causal Effects of Neighborhoods



The Opportunity Atlas

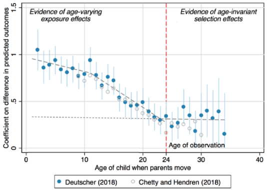
Identifying Causal Effects of Neighborhoods

United States



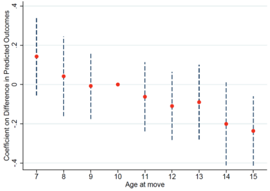
Source: Chetty and Hendren (QJE 2018)

Australia



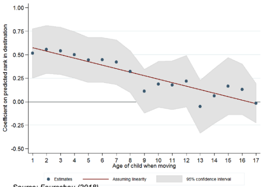
Source: Deutscher (2018)

Montreal, Canada



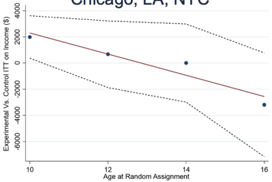
Source: Laliberté (2018)

Denmark



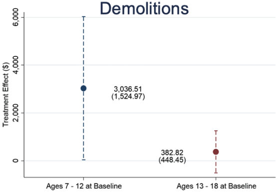
Source: Faurschou (2018)

MTO: Baltimore, Boston, Chicago, LA, NYC



Source: Chetty, Hendren, Katz (AER 2016)

Chicago Public Housing Demolitions



Source: Chyn (AER 2018)

The Opportunity Atlas

Identifying Causal Effects of Neighborhoods

- Key assumption: timing of moves to a better/worse area unrelated to other determinants of child's outcomes
- This assumption might not hold for two reasons:

The Opportunity Atlas

Identifying Causal Effects of Neighborhoods

- Key assumption: timing of moves to a better/worse area unrelated to other determinants of child's outcomes
- This assumption might not hold for two reasons:
  1. Parents who move to good areas when their children are young might be different from those who move later
  2. Moving may be related to other factors (e.g., change in parents' job, income) that affect children directly

Neighborhood Effects

- We have shown that the neighborhood where people live has important implications for outcomes.
- Observational data make it difficult to disentangle the multiple factors involved in the residential location choice.
  - Disparities in income, differences in information about neighborhood attributes
  - Housing/neighborhood preferences that also affect residential sorting behavior
  - Discrimination
  - etc.

Agenda

Intro

- I'm going to discuss Chetty et al (2014) AER

The Effects of Exposure to Better Neighborhoods on Children: New Evidence from the Moving to Opportunity Experiment†

By Raj Chetty, Nathaniel Hendren, and Lawrence F. Katz*

- Disclaimer: these slides are based on their slides

Motivation

- Substantial disparities in economic outcomes across low vs. high poverty neighborhoods [e.g., Wilson 1987, Jencks and Mayer 1990, Cutler and Glaeser 1997]
- These disparities motivated the HUD Moving to Opportunity (MTO) experiment in the mid 1990's
- Offered a randomly selected subset of families living in high-poverty housing projects housing vouchers to move to lower-poverty areas
- Large literature on MTO has found significant effects on adult health and subjective well-being
- But these studies have consistently found that the MTO treatments had no impact on earnings or employment rates of adults and older youth [e.g. Katz, Kling, and Liebman 2001, Oreopoulous 2003, Sanbonmatsu et al. 2011]

Revisit MTO

- They revisit the MTO experiment and focus on its impacts on children who were young when their families moved to better neighborhoods
- Re-analysis motivated by a companion paper that presents quasi-experimental evidence on neighborhood effects [Chetty and Hendren 2015]
- Key finding: childhood exposure effects
- Every year in a better area during childhood $\rightarrow$ better outcomes in adulthood
- Implies that gains from moving to a better area are larger for children who move when young

Revisit MTO

- In light of this evidence on childhood exposure effects, they returned to MTO data to examine treatment effects on young children
- Link MTO data to tax data to analyze effects of MTO treatments on children's outcomes in adulthood
- Children they study were not old enough to observe outcomes in adulthood at the time of the MTO Final Impacts Evaluation (which used data up to 2008)

Moving to Opportunity Experiment

- HUD Moving to Opportunity Experiment implemented from 1994-1998
- 4,600 families at 5 sites: Baltimore, Boston, Chicago, LA, New York
- Families randomly assigned to one of three groups:
  - Experimental: housing vouchers restricted to low-poverty ($< 10\%$) Census tracts
  - Section 8: conventional housing vouchers, no restrictions
  - Control: public housing in high-poverty (50% at baseline) areas

Moving to Opportunity Experiment

Data

- MTO data obtained from HUD
  - 4,604 households and 15,892 individuals
  - Primary focus: 8,603 children born in or before 1991
- Link MTO data to federal income tax returns from 1996-2012
  - Approximately 85% of children matched
  - Match rates do not differ significantly across treatment groups
  - Baseline covariates balanced across treatment groups in matched data

Estimation I

- Estimation 1:

$$y_i = \alpha + \beta_E Exp_i + \beta_S S8_i + \delta s_i + \epsilon_i \quad (1)$$

- Experimental take-up: 48% for young children, 40% for older children
- Section 8 take-up: 65.8% for young children, 55% for older children

Estimation II

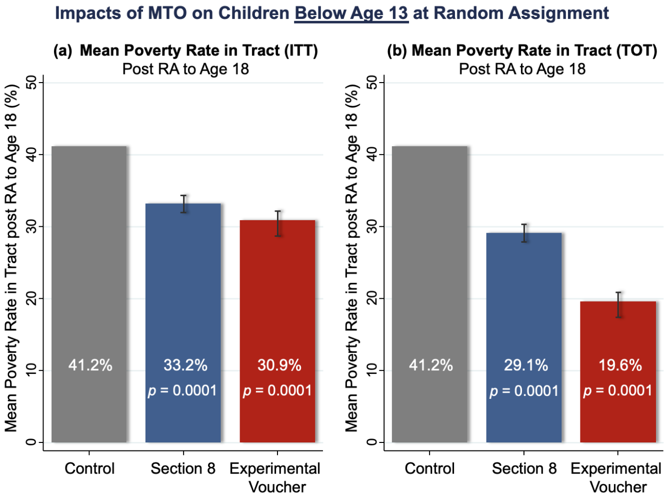
- Experimental take-up: 48% for young children, 40% for older children
- Section 8 take-up: 65.8% for young children, 55% for older children

$$y_i = \alpha + \beta_E TakeExp_i + \beta_S TakeS8_i + \delta s_i + \epsilon_i \tag{2}$$

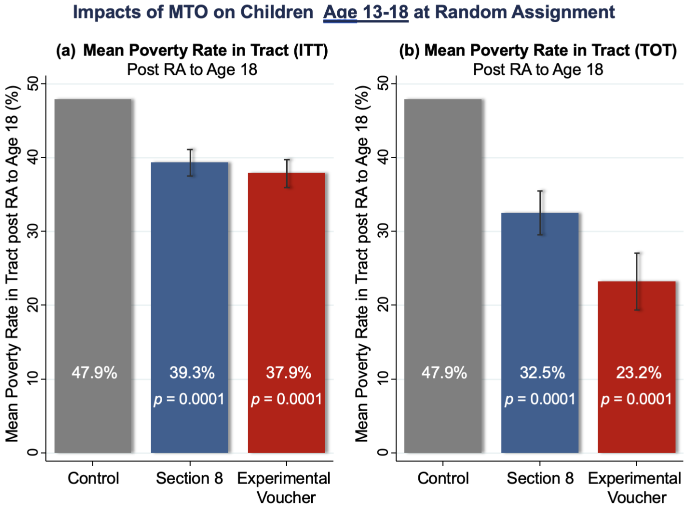
Results I: Where they live

- "First degree" effects of MTO experiment on poverty rates
- Measure mean poverty rates from random assignment to age 18 at tract level using Census data
- Use poverty rates as an index of nbhd. quality, but note that MTO treatments naturally changed many other features of neighborhoods too

Results I: Where they live



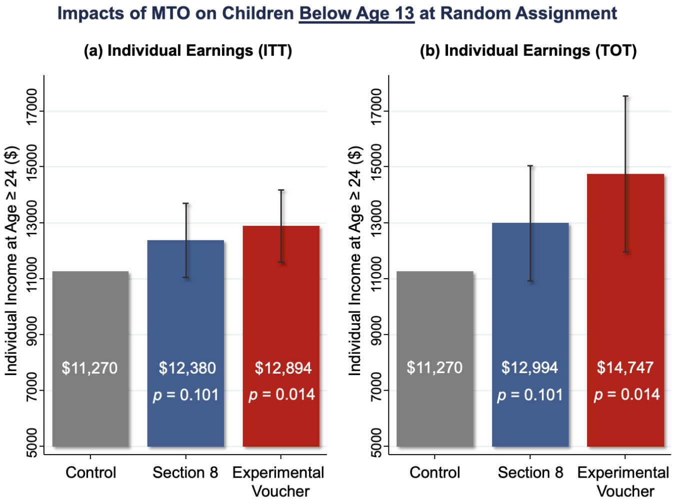
Results I: Where they live



Results II: Treatment Effects on Outcomes in Adulthood below age 13 at RA

- Now turn to impacts on outcomes in adulthood
- Begin by analyzing effects on children below age 13 at RA
- Start with individual earnings (W-2 earnings + self-employment income)
  - Includes those who don't file tax returns through W-2 forms
- Measured from 2008-12, restricting to years in which child is 24 or older
  - Evaluate impacts at different ages after showing baseline results

Results II: Treatment Effects on Outcomes in Adulthood below age 13 at RA



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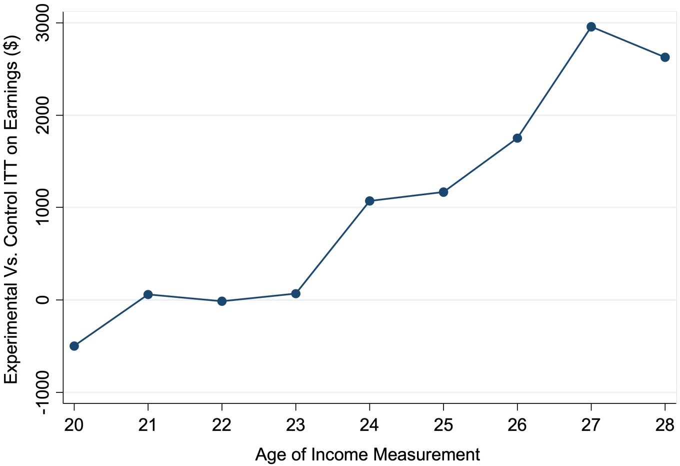
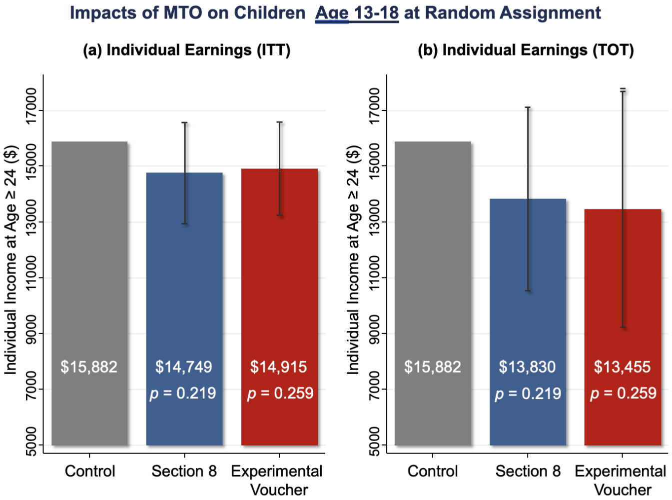
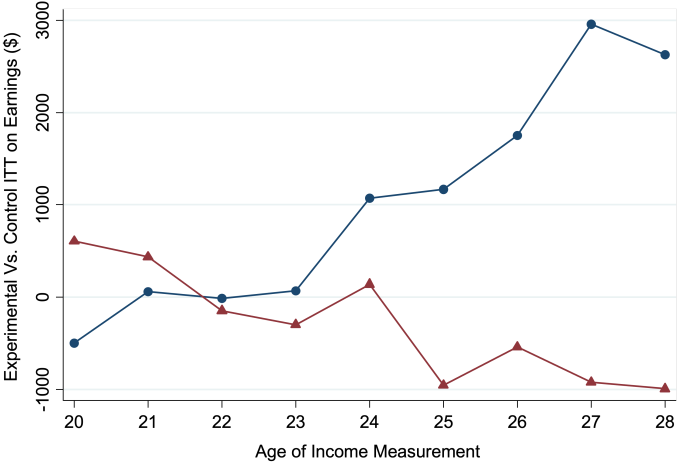


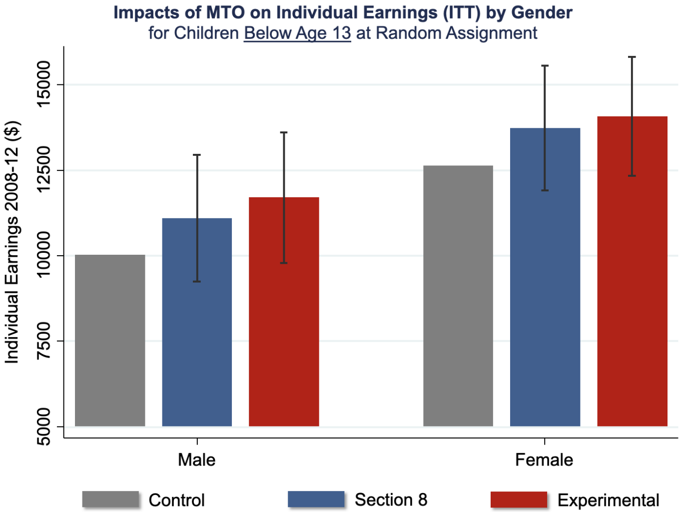
Results II: Treatment Effects on Outcomes in Adulthood ages 13-18 at random assignment



Results II: Treatment Effects on Outcomes in Adulthood ages 13-18 at random assignment



Results III: Heterogenity



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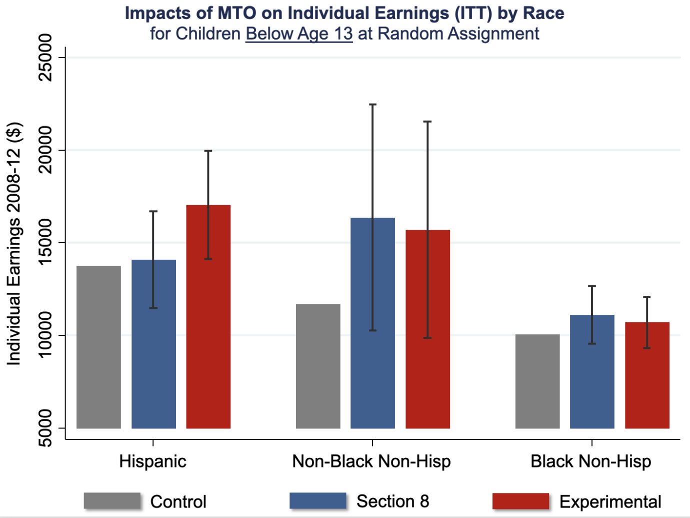


MTO Limitations

- MTO experiment shows that neighborhoods matter, but has two limitations:
- Sample size insufficient to determine which ages of childhood matter most
- Does not directly identify which neighborhoods are good or bad

Conclusion: Implications for Housing Voucher Policy

- Housing vouchers can be very effective but must be targeted carefully
  - Vouchers should be targeted at families with young children
  - Vouchers should be explicitly designed to help families move to affordable, high-opportunity areas
- In MTO experiment, unrestricted "Section 8" vouchers produced smaller gains even though families could have made same moves
- More generally, low-income families rarely use cash transfers to move to better neighborhoods [Jacob et al. 2015]
- 80% of the 2.1 million Section 8 vouchers are currently used in high-poverty, low-opportunity neighborhoods

Conclusion: Policy Lessons

- How can we improve neighborhood environments for disadvantaged youth?
- Short-term solution: Provide targeted housing vouchers at birth conditional on moving to better (e.g. mixed-income) areas
- Taxpayers may ultimately gain from this investment
- Long-term solution: improve neighborhoods with poor outcomes, concentrating on factors that affect children

Agenda

Discrimination

- Black people are less likely to find a house, be employed, more likely to be arrested by the police, and more likely to be incarcerated.
- Women are very scarce at the top echelon of the corporate, academic and political ladders despite the fact that (in rich countries at least) they get better grades in school and are more likely to graduate from college.
- While many in the media and public opinion circles argue that discrimination is a key force in driving these patterns, showing that it is actually the case is not simple.
- Indeed, it has proven elusive to produce convincing evidence of discrimination using standard regression analysis methods and observational data, in the sense in which we define discrimination: members of a minority group (women, Blacks, Muslims, immigrants, etc.) are treated differentially (less favorably) than members of a majority group with otherwise identical characteristics in similar circumstances.

Measuring Discrimination in the Field

- Earlier research on discrimination focused on individual-level outcome regressions, with discrimination estimated from the "minority" differential that remains unexplained after including as many proxies as possible.
- The limitations of this approach are well-known. The interpretation of the estimated "minority" coefficient is problematic due to OVB.
- The traditional answer has been to saturate the regression with as many relevant variables as are available.

Measuring Discrimination in the Field

- But, of course, ensuring that the researcher observes all that the decision-maker observes is a hopeless task.
- Saturating also changes the interpretation and may introduce "bad controls" (Guryan and Charles, 2013)
- Audit and correspondence methodologies were developed to address these core limitations of the regression approach to measuring discrimination.

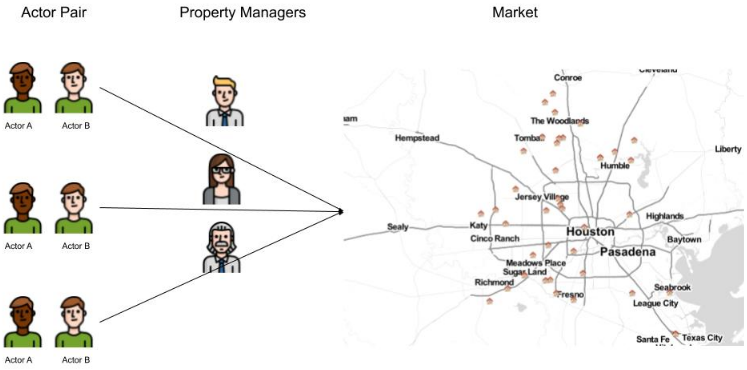
Experiment Set up: Identifying Housing Discrimination

Audit Studies – HDS 1977, 1989, 2000, 2012

1. Paired-tester: Blind-matched actors trained to be identical in every respect except for characteristic of interest (e.g., race)

2. Send teams of actors (white & minority) to real estate offices and rental management companies and record differential treatment

Traditional Way: Audit



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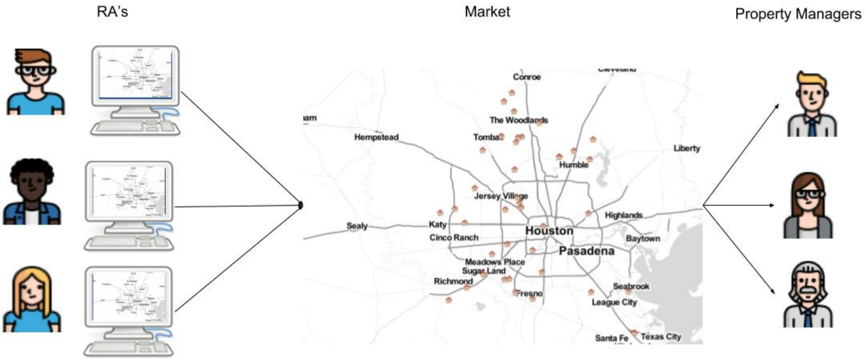
1. Paired-tester: Blind-matched actors trained to be identical in every respect except for characteristic of interest (e.g., race)
2. Send teams of actors (white & minority) to real estate offices and rental management companies and record differential treatment
3. Strong evidence of discrimination in previous reports (Turner et al 2002, 2012)
    1. Most blatant forms (e.g., refusal to show a property) to have declined over time
    2. Most persistent form: Steering (Ondrich et al 1998, 2003, 2005, Galster and Godfrey 2005)
        - Minority buyers are steered into neighborhoods with higher exposures to emissions from TRI facilities and Superfund sites (Christensen and Timmins, 2018)
4. Largest sample in 2012, 28 cities, 4,838 properties

Would a "Rose" by any other name get fewer callbacks?

- Correspondence Research Design (Bertrand and Mullainathan, 2004)
  1. Create fictitious identities
  2. Interact with retailers, employers, or housing brokers
  3. Randomly vary racial trait

Traditional Way: Correspondence



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  1. Create fictitious identities
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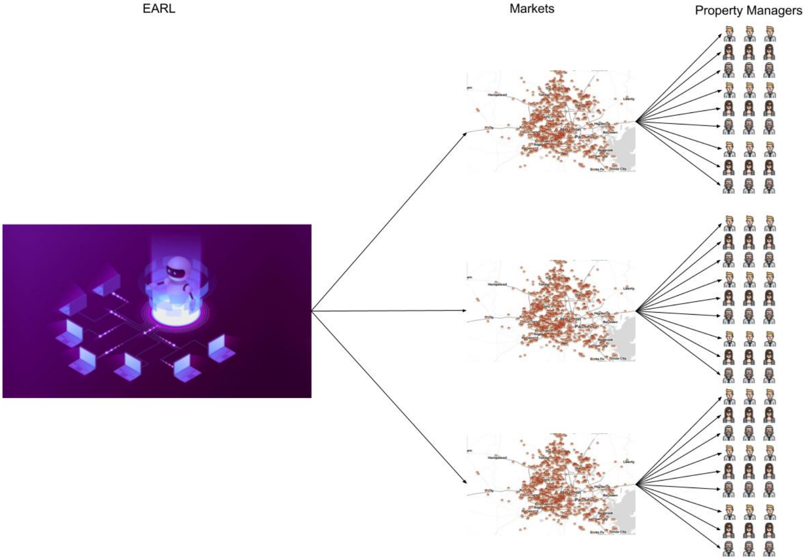
  3. Randomly vary racial trait

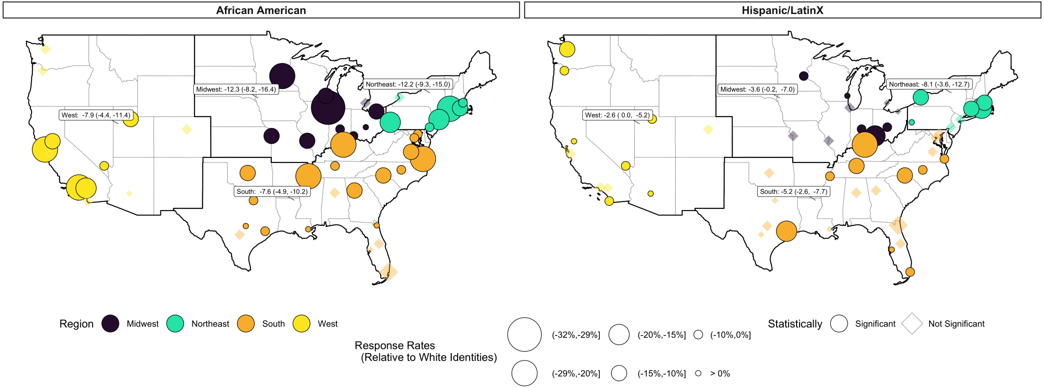
- Advantages of Correspondence Studies (vs Audit Designs)
  1. Correspondence studies give more control to the analyst (Bertrand, 2017)
  2. Hard to control for all differences between paired testers (Siegelman, 1993; Heckman, 1998)
  3. Less expensive (large, geographically targeted samples)

(Aside) What we do: EARL

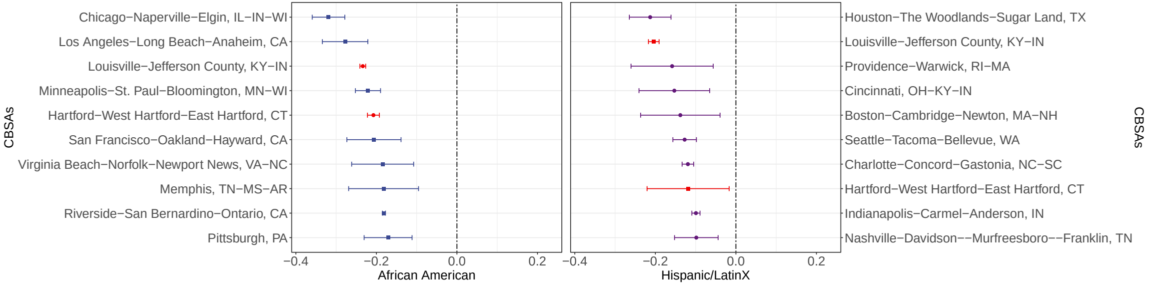


The Geography of Discriminatory Behavior in the US

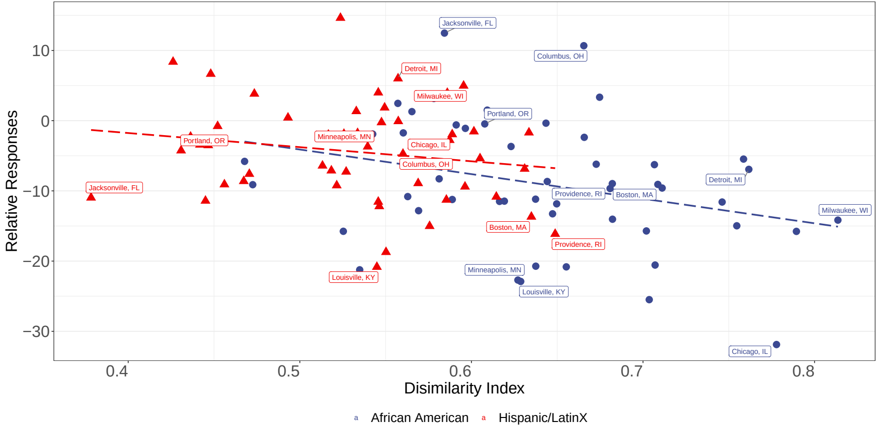
Figure 1: Response Rates CBSAs



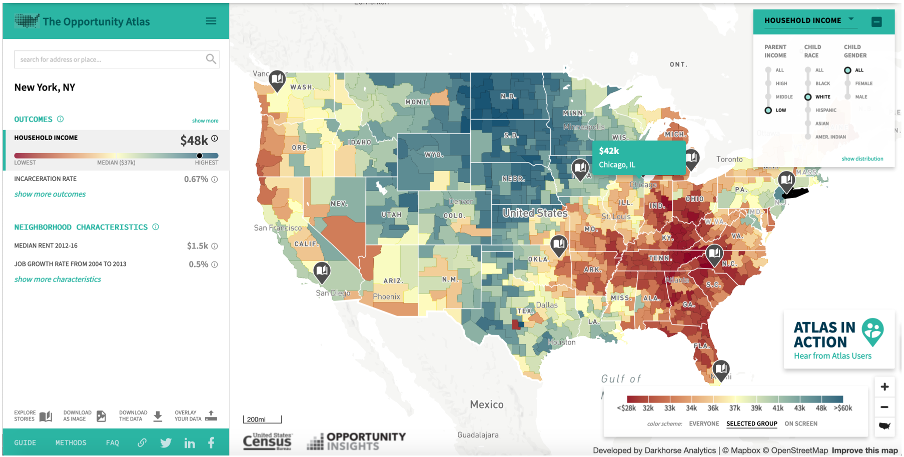
The Geography of Discriminatory Behavior in the US: The "Not" Top Ten



Discriminatory Behavior and Segregation



Discriminatory Behavior and the Income Mobility Gap



Source: https://www.opportunityatlas.org/

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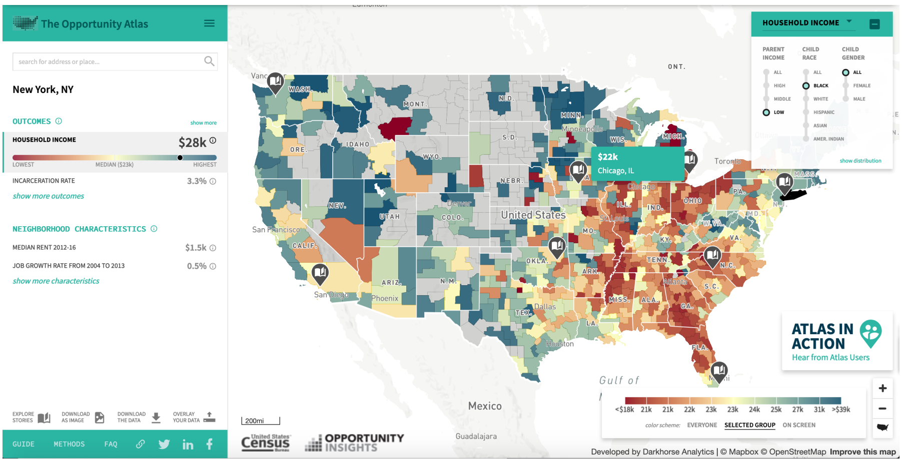
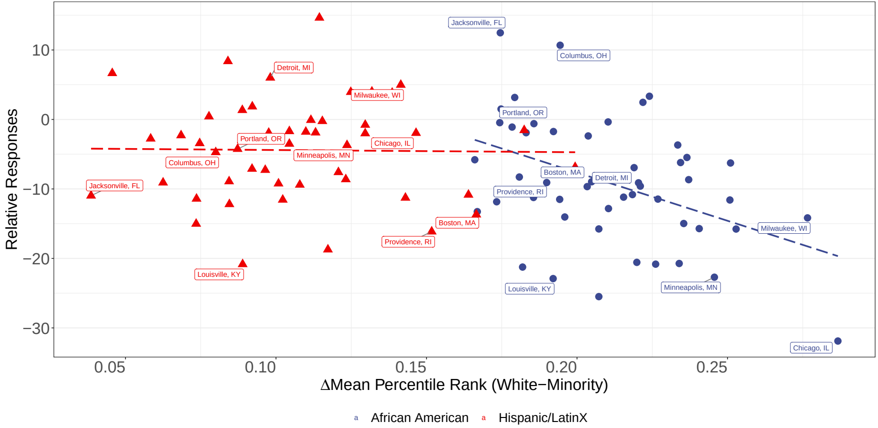


Source: https://www.opportunityatlas.org/

Discriminatory Behavior and the Income Mobility Gap



Discrimination: Two theories

- The two workhorse models of discrimination in the economics literature give drastically different answers, particularly with respect to the societal consequences.
  1. Taste based
  2. Statistical Discrimination

Ewens et al. paper

- Test: taste based vs statistical discrimination
- Use vacancy listings on Craigslist.org, across 34 U.S. cities,
- They send inquiry e-mails to 14,000 landlords.
- E-mails have information about the applicants: positive, negative, and no signals beyond race.
  - In the no-signal inquiry, landlords receive e-mails with racial-sounding names as the only signal.
  - In the positive information inquiry, the fictional applicant informs the landlord that she is a nonsmoker with a respectable job.
  - In the negative information inquiry, the applicant tells the landlord she has a below-average credit rating and smokes.

Ewens et al. model set up

- A landlord seeks to maximize the expected utility
- The expected utility derived from each applicant depends on the stream of future rental income (tenant quality) from renting the apartment successfully. Summarized by θ
- Although the rent is preannounced, θ may still vary as a result of default, lease renewal, and so on.
- Hence, the landlord forms a predicted quality $\hat{\theta}_i$ (a random variable) for each applicant and maximizes the expected utility $E[U(\hat{\theta}_i)]$

Ewens et al. model set up

- Four-stage process of matching potential tenants to apartments:
  1. **Inquiry**: An applicant with quality θ selects publicly posted rental units to send cost less inquiries with signal x to landlords.
  2. **Screening**: Given signals $X_T = \{x_1, \ldots, x_T\}$ received from T independent applicants, the landlord forms a set of predicted qualities $\Theta_T = \{\hat{\theta}_1, \ldots, \hat{\theta}_T\}$ and responds to n applicants.
  3. **Interview**: Interviews, which include credit and reference checking, reveal the true quality θ and these have costs.
  4. **Decision**: The candidate with the highest true quality θ is offered the apartment.

Ewens et al. model Statistical Discrimination

- Statistical Discrimination: Utility is **not** race dependent, but is (forecasted) quality dependent

$$E[U(\hat{\theta}_r)] = E[U(\hat{\theta}_{-r})] \tag{3}$$

- when

$$\tilde{\theta}_r = \tilde{\theta}_{-r} \tag{4}$$

Ewens et al. model Statistical Discrimination

- Statistical Discrimination:

$$x_r = \theta_r + \epsilon_r \tag{5}$$

  - $\theta_r \sim N(\mu_r, \sigma_\theta^2)$
  - $E(\theta_r) = \mu_r$
  - $V(\theta_r) = \sigma_\theta^2$
  - $E(\epsilon_r|\theta_r) = 0$
  - $V(\epsilon_r|\theta_r) = \sigma_{\epsilon,r}^2$

Ewens et al. Statistical Discrimination

- Landlord forecasted $\hat{\theta}$ for each race r:

$$\hat{\theta}_r = \hat{\mu}_r^L + \hat{\gamma}_r x_r \tag{6}$$

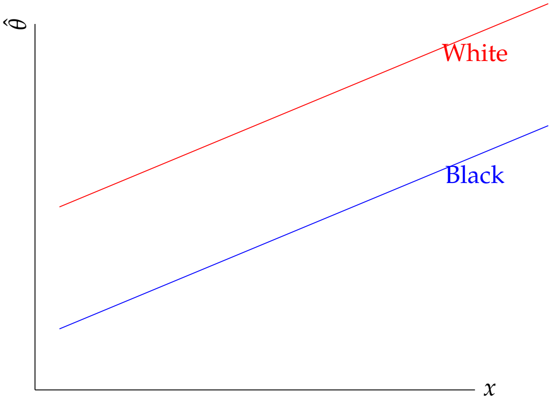
- were

$$\hat{\gamma}_r = \frac{cov(\theta_r, x_r)}{var(x_r)} \tag{7}$$

$$\hat{\mu}_r^L = \bar{\theta}_r - \hat{\gamma}_r \bar{x}_r \tag{8}$$

Ewens et al. Statistical Discrimination

- Landlord forecasting regression for each race r:



Ewens et al. Statistical Discrimination

- Two types of signals
  1. A negative signal $-\tilde{x}^- < 0$
  2. A positive signal $\tilde{x}^+ > 0$
- The mean difference between black and white applicants sending
  - A positive signal is

$$E(\hat{\theta}_B|\tilde{x}^+) - E(\hat{\theta}_W|\tilde{x}^+) = \hat{\mu}_B^L - \hat{\mu}_W^L - (\hat{\gamma}_B - \hat{\gamma}_W)\,\tilde{x}^+ \tag{9}$$

  - Negative signal

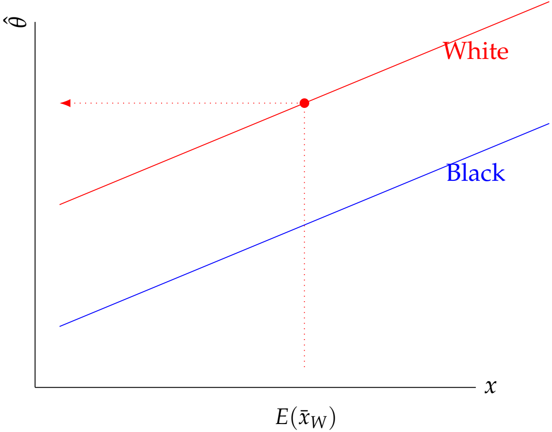
$$E(\hat{\theta}_B| - \tilde{x}^-) - E(\hat{\theta}_W| - \tilde{x}^-) = \hat{\mu}_B^L - \hat{\mu}_W^L + (\hat{\gamma}_W - \hat{\gamma}_B)\,\tilde{x}^- \tag{10}$$

  - The difference

$$E(\hat{\theta}_B - \hat{\theta}_W| - \tilde{x}^-) - E(\hat{\theta}_B - \hat{\theta}_W|\tilde{x}^+) = (\hat{\gamma}_W - \hat{\gamma}_B)\left(\tilde{x}^+ + \tilde{x}^-\right) \tag{11}$$

Ewens et al. model Statistical Discrimination

- Suppose that $E(\bar{x}_W) > E(\bar{x}_B)$



Ewens et al. model Statistical Discrimination

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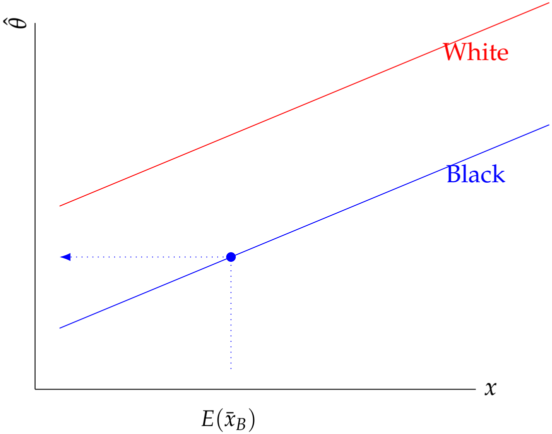

Ewens et al. model Statistical Discrimination

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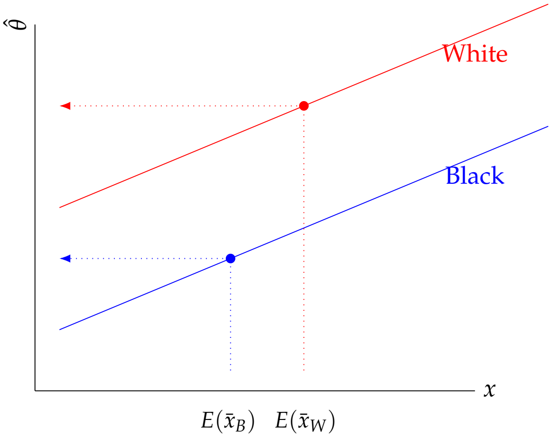
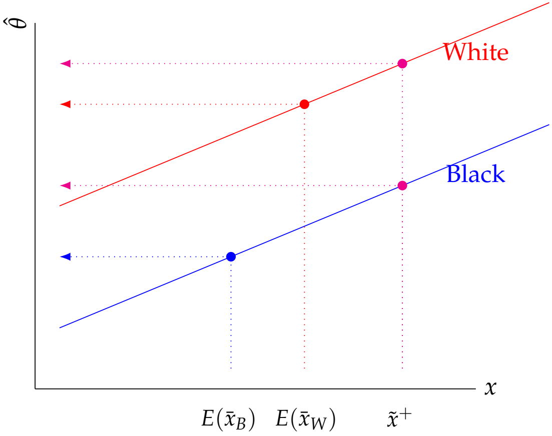


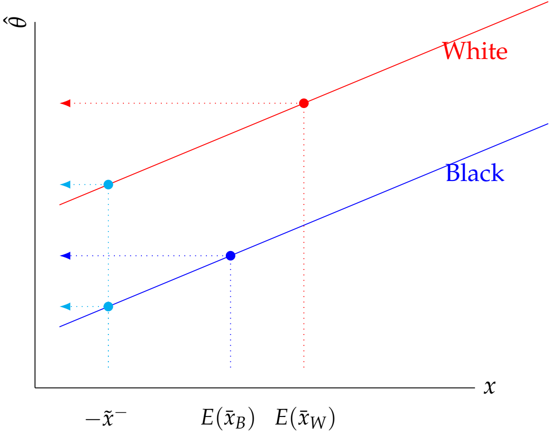
Ewens et al. model Statistical Discrimination

- Suppose that $E(\bar{x_W}) > E(\bar{x}_B)$ and a surprise positive signal $\tilde{x}^+ > E(\bar{x}_W)$



Ewens et al. model Statistical Discrimination

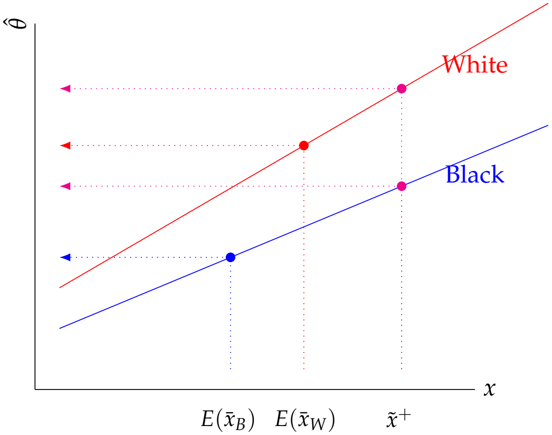
- Suppose that $E(\bar{x_W}) > E(\bar{x}_B)$ and a surprise negative signal $-\tilde{x}^- < E(\bar{x}_B)$



Ewens et al. model Statistical Discrimination

$\gamma_W > \gamma_B$

- Suppose that $E(\bar{x_W}) > E(\bar{x}_B)$ and a surprise positive signal $\tilde{x}^+ > E(\bar{x}_W)$



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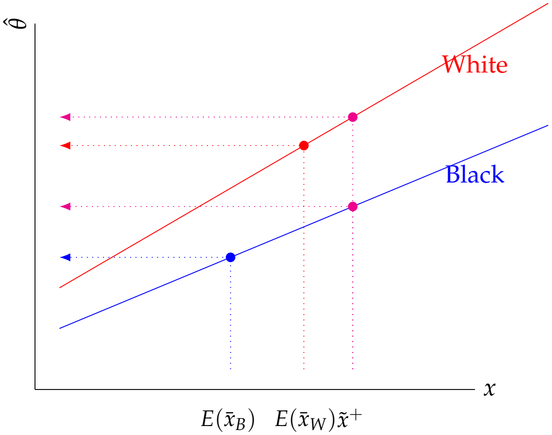

Ewens et al. model Statistical Discrimination

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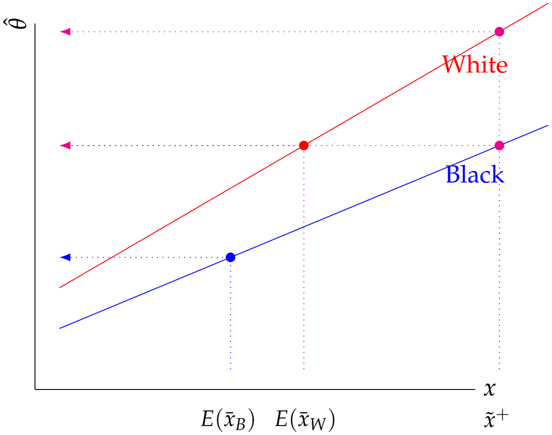

Ewens et al. model Statistical Discrimination

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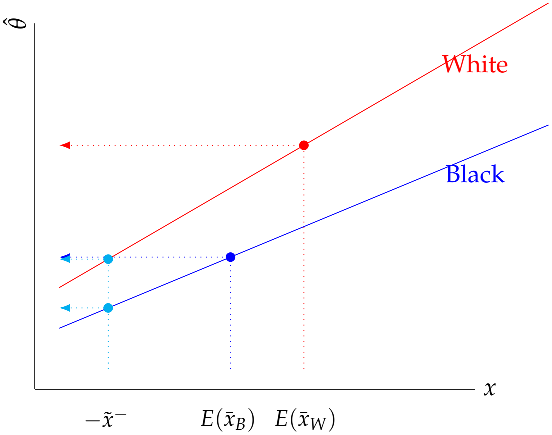

Ewens et al. model Statistical Discrimination

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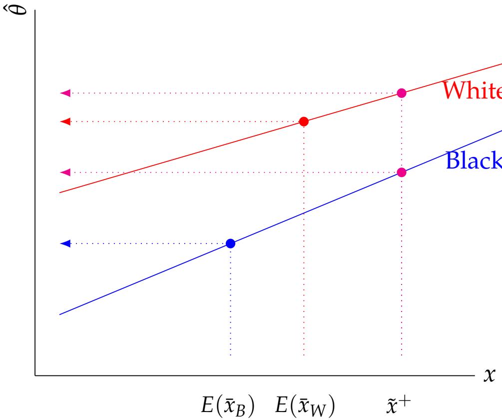

Ewens et al. model Statistical Discrimination

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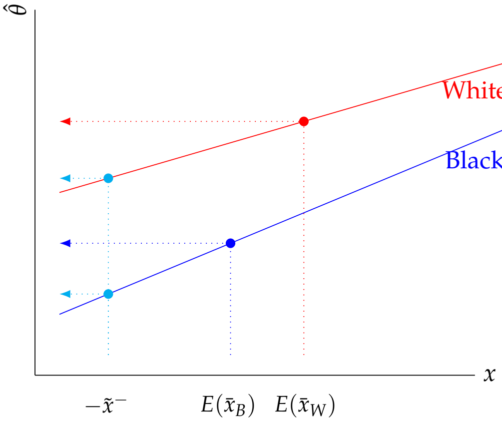


Ewens et al. model Taste Based Discrimination

- Taste Based Discrimination
  - Let a prejudiced landlord predict applicant quality based on a race-independent signal:

$$\hat{\theta}_i = \hat{\mu}^L + \hat{\gamma} x_i \tag{12}$$

  - Now their utility is race dependent.
    - Assume that the landlord exhibits out-group prejudice such that a prejudice parameter, k, discounts the utility derived from an out-group applicant so that

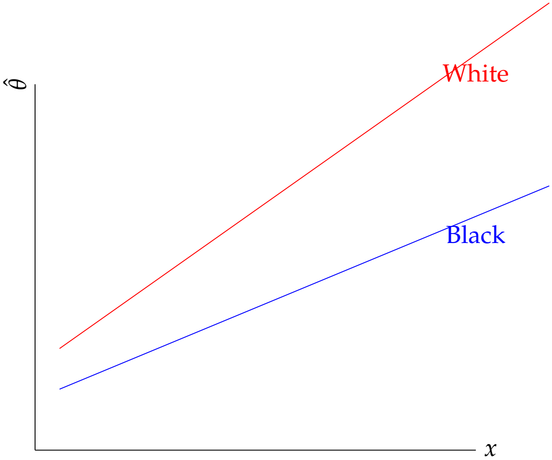
$$E[U(\tilde{\theta}_r)] > E[U(\tilde{\theta}_{-r})] \tag{13}$$

  - when

$$\tilde{\theta}_r = \tilde{\theta}_{-r} \tag{14}$$

Ewens et al. model Taste Based Discrimination

- For example $E[U(\tilde{\theta}_{-r})] = \left(\hat{\mu}^L + \hat{\gamma} x_i\right) k$



Ewens et al. model Taste Based Discrimination

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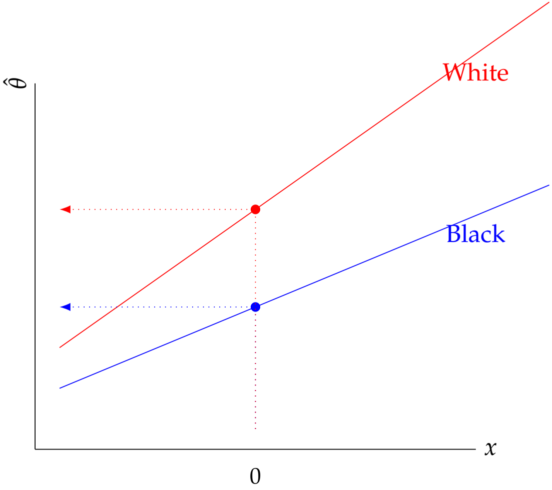

Ewens et al. model Taste Based Discrimination

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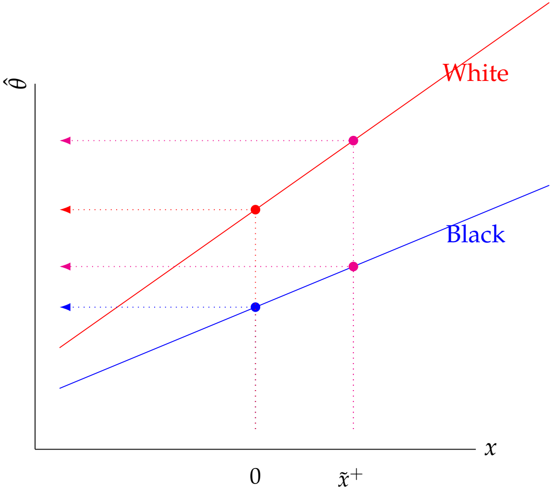

Ewens et al. model Taste Based Discrimination

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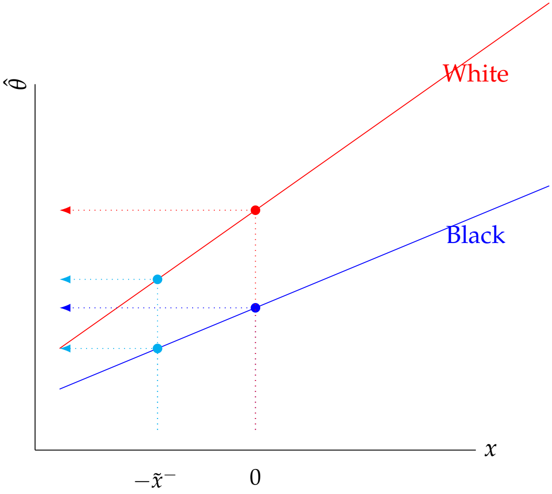

Ewens et al. results

- H1 Stat: On average, a white applicant is more likely to receive a positive response than a black applicant in the no-signal base case
- H1 Taste: On average, a white applicant is more likely to receive a positive response than a black applicant in the no-signal base case.

$$R_i = \alpha_W + \alpha_B B_i + u_i$$

Ewens et al. results

TABLE 6.—DIFFERENTIAL TREATMENT BY RACE AND INFORMATIONAL SIGNALS

	(1)	(2)	(3)	(4)
Black	−0.093***			
	(0.015)			
Positive Information				
Positive Information × Black				
Negative Information				
Negative Information × Black				
% Black				
Black × %Black				
Positive Information × %Black				
Positive Information × Black × %Black				
Negative Information × %Black				
Negative Information × Black × %Black				
Constant	0.581***			
	(0.012)			
Omitted category	White Baseline			
Observations	4,226			
R^2	0.009			

See definitions of variables in the notes to tables 2 and 3. Robust standard errors clustered by neighborhood reported in parentheses. Columns 1–4 correspond to hypotheses 1 and 1A, 2 and 2A, 3 and 3A, and 4 and 4A, respectively. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Ewens et al. results

- H2 Stat: On average, the positive response gap between white and black applicants is larger with a positive signal sent than with a negative signal sent.
- H2 Taste: On average, the response gap between white and black applicants when a positive signal is sent is larger than the response gap between white and black applicants when a negative signal is sent.

$$R_i = \alpha_{PW} + \alpha_{PB} B_i + \alpha_{NW} N_i + \alpha_{NB}(N_i \times B_i) + u_i \tag{15}$$

Ewens et al. results

Table 6.—Differential Treatment by Race and Informational Signals

	(1)	(2)	(3)	(4)
Black	−0.093***	−0.092***		
	(0.015)	(0.012)		
Positive Information				
Positive Information × Black				
Negative Information		−0.377***		
		(0.013)		
Negative Information × Black		0.044**		
		(0.018)		
% Black				
Black × %Black				
Positive Information × %Black				
Positive Information × Black × %Black				
Negative Information × %Black				
Negative Information × Black × %Black				
Constant	0.581***	0.619***		
	(0.012)	(0.009)		
Omitted category	White	White		
	Baseline	Positive information		
Observations	4,226	10,011		
R^2	0.009	0.128		

See definitions of variables in the notes to tables 2 and 3. Robust standard errors clustered by neighborhood reported in parentheses. Columns 1–4 correspond to hypotheses 1 and 1A, 2 and 2A, 3 and 3A, and 4 and 4A, respectively. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Ewens et al. results

- H3 Stat: On average, negative information will shrink the racial gap observed in the base case, but positive information will have an ambiguous effect on the racial gap observed in the base case.
- H3 Taste: On average, negative information will unambiguously narrow the racial gap observed in the no-signal base case, but positive information will unambiguously widen the racial gap observed in the base case.

$$R_i = \beta_W + \beta_B B_i + \beta_P P_i + \beta_{PB}(P_i \times B_i) + \beta_{NW} N_i + \beta_{NB}(N_i \times B_i) + u_i \tag{16}$$

Ewens et al. results

TABLE 6.—DIFFERENTIAL TREATMENT BY RACE AND INFORMATIONAL SIGNALS

	(1)	(2)	(3)	(4)
Black	−0.093***	−0.092***	−0.093***	
	(0.015)	(0.012)	(0.015)	
Positive Information			0.039***	
			(0.013)	
Positive Information × Black			0.001	
			(0.019)	
Negative Information		−0.377***	−0.338***	
		(0.013)	(0.016)	
Negative Information × Black		0.044**	0.045**	
		(0.018)	(0.020)	
% Black				
Black × %Black				
Positive Information × %Black				
Positive Information × Black × %Black				
Negative Information × %Black				
Negative Information × Black × %Black				
Constant	0.581***	0.619***	0.581***	
	(0.012)	(0.009)	(0.012)	
Omitted category	White	White	White	
	Baseline	Positive information	Baseline	
Observations	4,226	10,011	14,237	
R^2	0.009	0.128	0.100	

See definitions of variables in the notes to tables 2 and 3. Robust standard errors clustered by neighborhood reported in parentheses. Columns 1–4 correspond to hypotheses 1 and 1A, 2 and 2A, 3 and 3A, and 4 and 4A, respectively. $***p < 0.01$, $**p < 0.05$, $*p < 0.1$.

Ewens et al. results

- H4 Stat: Positive treatment should shrink the racial gap in positive responses relatively more in predominantly black neighborhoods. Conversely, negative treatment will shrink the racial gap in predominantly white neighborhoods, but not necessarily in predominantly black neighborhoods.
- H4 Taste: As the share of black residents in a neighborhood S_B increases, the response gap between white and black applicants in the base case decreases. In a majority black neighborhood, a surprising positive signal will unambiguously benefit a black applicant relatively more than a white applicant, while a surprising negative signal will unambiguously hurt a black applicant relatively more than a white applicant.

$$
\begin{aligned}
R_i = {} & \beta_W + \beta_{SW} S_{Bi} + \beta_B B_i + \beta_{SB}(S_{Bi} \times B_i) + \beta_{PW} P_i && (17)\\
& + \beta_{SPW}(S_{Bi} \times P_i) + \beta_{PB}(P_i \times B_i) && (18)\\
& + \beta_{SPB}(S_{Bi} \times P_i \times B_i) + \beta_{NW} N_i + \beta_{SNW}(S_{Bi} \times N_i) && (19)\\
& + \beta_{NB}(N_i \times B_i) + \beta_{SNB}(S_{Bi} \times N_i \times B_i) + u_i && (20)
\end{aligned}
$$

Ewens et al. results

TABLE 6.—DIFFERENTIAL TREATMENT BY RACE AND INFORMATIONAL SIGNALS

	(1)	(2)	(3)	(4)
Black	−0.093***	−0.092***	−0.093***	−0.084***
	(0.015)	(0.012)	(0.015)	(0.019)
Positive Information			0.039***	0.053***
			(0.013)	(0.017)
Positive Information × Black			0.001	−0.032
			(0.019)	(0.025)
Negative Information		−0.377***	−0.338***	−0.347***
		(0.013)	(0.016)	(0.018)
Negative Information × Black		0.044**	0.045**	0.044*
		(0.018)	(0.020)	(0.026)
% Black				0.014
				(0.067)
Black × %Black				−0.077
				(0.099)
Positive Information × %Black				−0.118
				(0.082)
Positive Information × Black × %Black				0.267**
				(0.125)
Negative Information × %Black				0.078
				(0.093)
Negative Information × Black × %Black				0.009
				(0.130)
Constant	0.581***	0.619***	0.581***	0.579***
	(0.012)	(0.009)	(0.012)	(0.014)
Omitted category	White	White	White	White
	Baseline	Positive information	Baseline	Baseline
Observations	4,226	10,011	14,237	14,237
R^2	0.009	0.128	0.100	0.101

See definitions of variables in the notes to tables 2 and 3. Robust standard errors clustered by neighborhood reported in parentheses. Columns 1–4 correspond to hypotheses 1 and 1A, 2 and 2A, 3 and 3A, and 4 and 4A, respectively. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.