

Audit Studies

- Simulated market transaction under controlled conditions
- Frequently used in studies of discrimination. Paired individuals and sent out to see if they can buy a car, a house, rent an apartment, ...
- Individuals are given identical personas , so they differ only by observable demographic attributes.

Bertrand, Marianne and Sendhil Mullainathan. 2004. "Are Emily and Greg More Employable than Lakisha and Jamal? A Field Experiment on Labor Market Discrimination." *American Economic Review* 94 (Sept): 991-1013.

Do names affect employer assessments of applicant qualifications, holding all other factors (education, experience, location) constant?

Data

- Resumes from fictitious applicants are sent to firms that posted in the help wanted section of Chicago and Boston newspapers
- Paired resumes that are comparable across all attributes except race
- Used callbacks to determine extent of discrimination

TABLE 1—MEAN CALLBACK RATES BY RACIAL SOUNDINGNESS OF NAMES

	Percent callback for White names	Percent callback for African-American names	Ratio	Percent difference (<i>p</i> -value)
Sample:				
All sent resumes	9.65 [2,435]	6.45 [2,435]	1.50	3.20 (0.0000)
Chicago	8.06 [1,352]	5.40 [1,352]	1.49	2.66 (0.0057)
Boston	11.63 [1,083]	7.76 [1,083]	1.50	4.05 (0.0023)
Females	9.89 [1,860]	6.63 [1,886]	1.49	3.26 (0.0003)
Females in administrative jobs	10.46 [1,358]	6.55 [1,359]	1.60	3.91 (0.0003)
Females in sales jobs	8.37 [502]	6.83 [527]	1.22	1.54 (0.3523)
Males	8.87 [575]	5.83 [549]	1.52	3.04 (0.0513)

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Does this prove that wage gap is
due to Employer discrimination?

Why or Why not?

Arai, Mahmood and Peter Skogman Thoursie. "Renouncing Personal Names: An Empirical Examination of Surname Change and Earnings." *Journal of Labor Economics* 27 (January 2009):127-147.

Sweden prevents individuals from changing to an existing
surname unless they are a linear descendant

For \$150, you can change to a new Swedish sounding surname

Does this investment pay for itself?

Table 2
Means of Name Changers and Name Keepers in Sweden from
Asian/African/Slavic Countries, by Gender

	Females		Males	
	Name Changers	Name Keepers	Name Changers	Name Keepers
Earnings before name change (SEK hundreds)	648 (769)	693 ^a (883)	705 (895)	848 ^a (1,093)
Earnings after name change (SEK hundreds)	976 (1,052)		974 (1,178)	
Fraction positive earnings before name change	.65	.62 ^a	.62	.63 ^a
Fraction positive earnings after name change	.73		.64	
Year of birth	1960 (8.0)	1961 (8.7)	1960 (7.3)	1960 (8.7)
Resident in large city	.65	.58	.62	.56
Latest immigration year	1984 (7.0)	1987 (8.5)	1985 (7.3)	1987 (8.6)
Slavic countries	.66	.54	.30	.40
Education level:				
Below secondary	.17	.18	.19	.16
Secondary school	.41	.28	.36	.30
Above secondary	.19	.15	.21	.16
Unknown	.23	.39	.23	.38
N	219	80,430	422	105,175

NOTE.—All earnings are in year 2000 prices. Residence in a large city is measured as the first year that an individual is observed in the panel. The sample of name keepers is constructed according to the same selection criteria as for name changers; see Sec. II. Standard deviations are in parentheses.

^a Earnings and fraction of positive earnings are averaged across all years between 1990 and 2001.

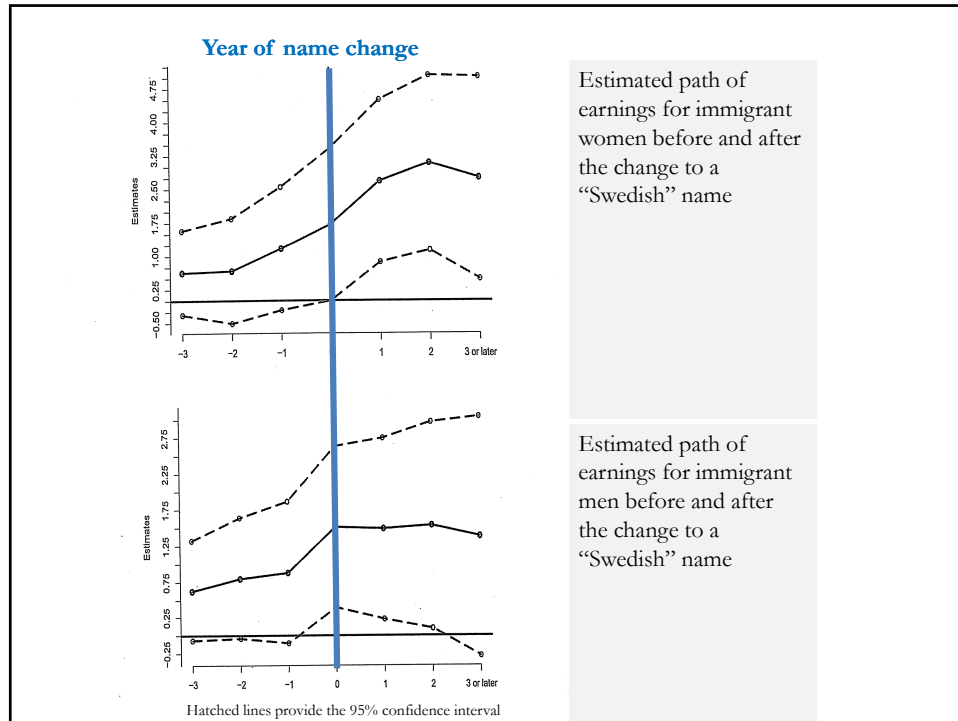
Table 5
Fixed Effect Estimations of Pretreatment and
Posttreatment Effects of Name Change on Log
Annual Earnings, 1990–2001, by Gender

	Males (1)	Females (2)
3 years before (δ_{t+3})	.627 (.355)	.630 (.482)
2 years before (δ_{t+2})	.799 (.428)	.671 (.602)
1 year before (δ_{t+1})	.878 (.502)	1.169 (.705)
Year of name change (δ_t)	1.510** (.573)	1.710* (.873)
1 year after (δ_{t-1})	1.488* (.644)	2.680** (.929)
2 years after (δ_{t-2})	1.534* (.733)	3.088** (1.000)
3 years after or later (δ_{t-3})	1.384 (.849)	2.751* (1.158)
F(lead effects)	1.31	1.13
P-value	.27	.34
Total observations	4,725	2,468
No. of individuals	422	219

NOTE.—All estimations include year dummies, county dummies, county and year interactions, and age group and year interactions. See also notes to table 3. Standard errors are in parentheses.

* $p < .05$.

** $p < .01$.



Heckman: Possible audit biases

1. Individual firm versus market discrimination
2. Auditor bias
3. Statistical discrimination—distribution of unobserved productivity
4. Gross or net differences versus equal treatment

Table 1

Outcomes From Major Audit Studies For Blacks*(outcome: get job or not)*

Number of Audits	Pair	(a)		(b)		Equal Treatment a + b	White Yes, Black No	White No, Black Yes
		Both Get Job	Neither Gets a Job					
Chicago*								
35	1	(5) 14.3%	(23) 65.7%	80.0%	(5) 14.3%	(2) 5.7%		
40	2	(5) 12.5%	(25) 62.5%	75.0%	(4) 10.0%	(6) 15.0%		
44	3	(3) 6.8%	(37) 84.1%	90.9%	(3) 6.8%	(1) 2.3%		
36	4	(6) 16.7%	(24) 66.7%	83.4%	(6) 16.7%	(0) 0%		
42	5	(3) 7.1%	(38) 90.5%	97.6%	(1) 2.4%	(0) 0%		
197	Total	(22) 11.2%	(147) 74.6%	85.8%	(19) 9.6%	(9) 4.5%		
Washington*								
46	1	(5) 10.9%	(26) 56.5%	67.4%	(12) 26.1%	(3) 6.5%		
54	2	(11) 20.4%	(31) 57.4%	77.8%	(9) 16.7%	(3) 5.6%		
62	3	(11) 17.7%	(36) 58.1%	75.8%	(11) 17.7%	(4) 6.5%		
37	4	(6) 16.2%	(22) 59.5%	75.7%	(7) 18.9%	(2) 5.4%		
42	5	(7) 16.7%	(26) 61.9%	77.6%	(7) 16.7%	(2) 4.8%		
241	Total	(40) 16.6%	(141) 58.5%	75.1%	(46) 19.1%	(14) 5.8%		
Denver**								
18	1	(2) 11.1%	(11) 61.1%	72.1%	(5) 27.8%	(0) 0.0%		
53	2	(2) 3.8%	(41) 77.4%	81.2%	(0) 0.0%	(10) 18.9%		
33	3	(7) 21.2%	(25) 75.8%	97.0%	(1) 3.0%	(0) 0.0%		
15	4	(9) 60.0%	(3) 20.0%	80.0%	(2) 6.7%	(2) 13.3%		
26	9	(3) 11.5%	(23) 88.5%	100.0%	(0) 0.0%	(0) 0.0%		
145	Total	(23) 15.8%	(103) 71.1%	86.9%	(7) 4.8%	(12) 8.3%		

Note: Results are percentages; figures in parentheses are the relevant number of audits.

Sources: Heckman and Siegelman (1993).

* This study was conducted by the Urban Institute.

Heckman, James J. 1998. "Detecting Discrimination." *The Journal of Economic Perspectives* 12 (Spring): 101-116.