Labor Market Discrimination: Evidence

Outline

- Taste-based discrimination and prejudice
  - Charles-Guryan (2008)
- Discrimination in hiring
  - Goldin-Rouse (2000)
- Audit/Correspondence studies
  - Bertrand-Mullainathan (2004)
  - Discussion

Guryan and Charles (2008): Prejudice and Wages

- Becker model predicated on the assumption that some whites are prejudiced, suffer disutility from interacting with blacks.
- Model has very stark predictions on how the distribution of prejudice affects equilibrium wage differential.
  - But these predictions have never been tested.

- Predictions:
  1. Equilibrium wage differential depends on the extent of prejudice of the marginal employer, not the average employer.
  2. Right tail of the distribution of prejudice does not matter for wage differential.
  3. The larger the fraction of blacks in the workforce, the larger the wage differential.

- Test these predictions using data on attitudes from the General Social Survey, and data on wages from CPS.

Measuring prejudice

- General Social Survey (GSS)
  - Repeated cross section of US households
  - Conducted every year since 1972
  - Many questions on racial attitudes
  - Also: happiness, political attitudes, role of women, etc.
  - Representative at the national level (not at the state level!), but sample not very large.

- Questions asked vary year to year.

- Four questions asked more or less every year:
  - Do you think there should be laws against marriages between blacks and whites?
  - If your party nominated a black for president, would you vote for him if he were qualified for the job?
  - Agree? White people have the right to keep black people out of their neighborhoods and blacks should respect that right.
  - Aggregation of three questions about whether you would object to sending your kids to a school that had few/half/most black students.

- Measuring prejudice: simple average of standardized responses to all questions asked for a given respondent.
  - Exclude questions that are more about government role.

Appendix

TABLE A1
GSS Questions Used to Measure Prejudice

Abbreviation	Question
AFFRMACT	Do you oppose a preference in hiring and promotion?
BUSING	In general do you favor the busing of black and white children from one school district to another?
CLOSEBLK	In general, how close do you feel to blacks?
FEELBLKS	In general, how warm or cool do you feel toward blacks?
HELPBLK	Agree? The government is obligated to help blacks.
NATRACE	Agree? We are spending too much money improving the condition of blacks.
RACAVOID	If you were driving through neighborhoods in a city, would you go out of your way to avoid going through a black section?
RACCHNG	If you and your friends belonged to a social club that would not let blacks join, would you try to change the rules?
RACDIN	How strongly would you object if a family member brought a black friend home for dinner?
RACJOB	Do you think blacks should have as good a chance as anyone to get any kind of job, or do you think white people should have the first chance at any kind of job?
RACMAR*	Do you think there should be laws against marriages between blacks and whites?
RACMAREL	How would it make you feel if a close relative of yours were planning to marry a black?
RACMARPR	Agree? You can expect special problems with marriages between blacks and whites.
RACOBJCT	If a black with the same income and education as you have moved in to your block, would it make any difference to you?
RACOPEN	Would you vote for a law that says a homeowner can refuse to sell to blacks, or one that says homeowners cannot refuse to sell based on skin color?
RACPEERS*	Aggregation of three questions about whether you would object to sending your kids to a school that had few/half/most black students.
RACPRES*	If your party nominated a black for president, would you vote for him if he were qualified for the job?
RACPUSH	Agree? Blacks shouldn't push themselves where they're not

Trends in prejudice

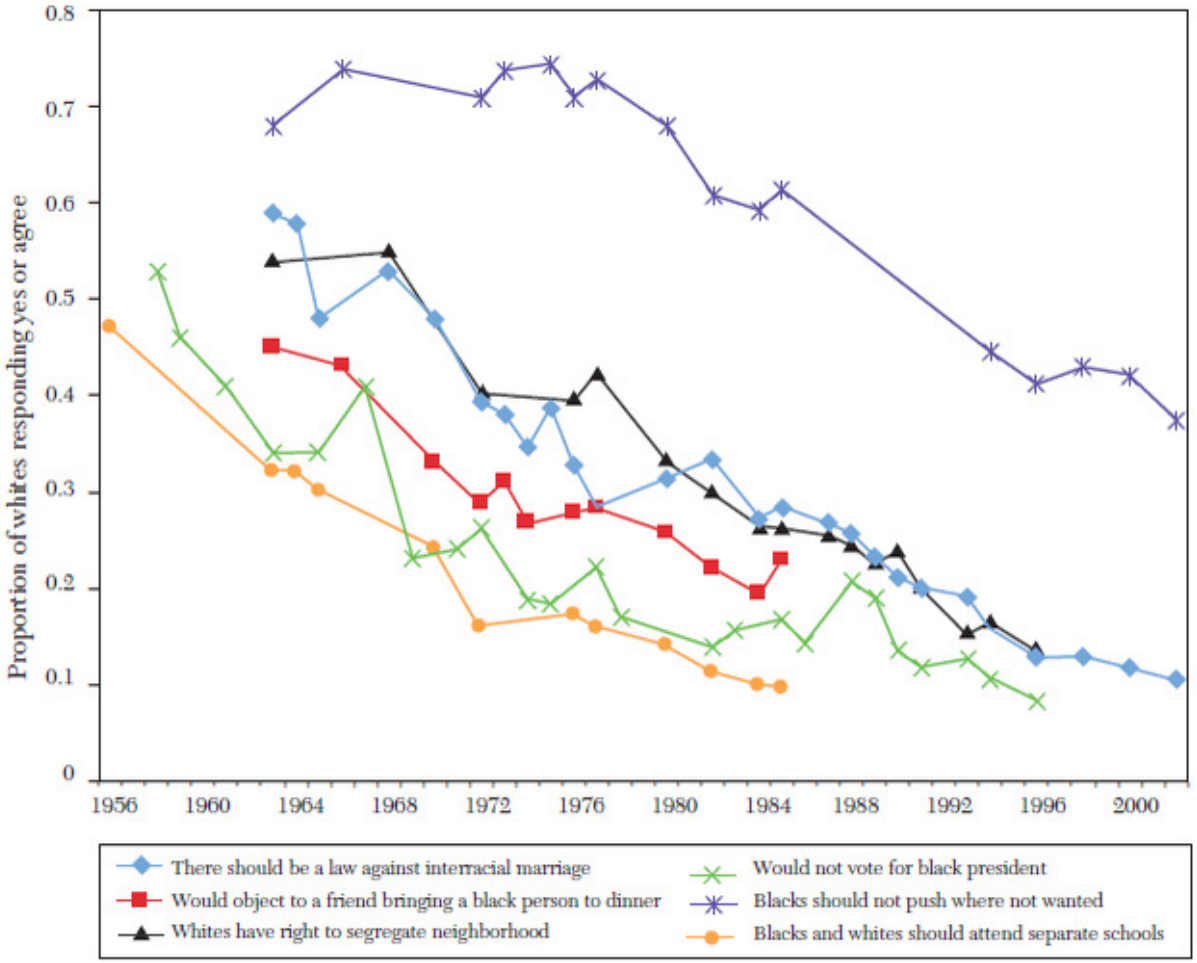


Figure 3. Trends in Prejudice Measures, 1956–2003.

Source: Lang and Lehmann (2012)

- But Becker model is not about average level of prejudice.
- Must instead calculate the extent of prejudice of the *marginal* discriminator.
- If all employers hire exactly one worker (more generally, if all firms are equal size), then the marginal discriminator is the individual at the p^{th} percentile of the prejudice distribution, where p is the fraction of blacks in the workforce.

Distribution of prejudice across regions

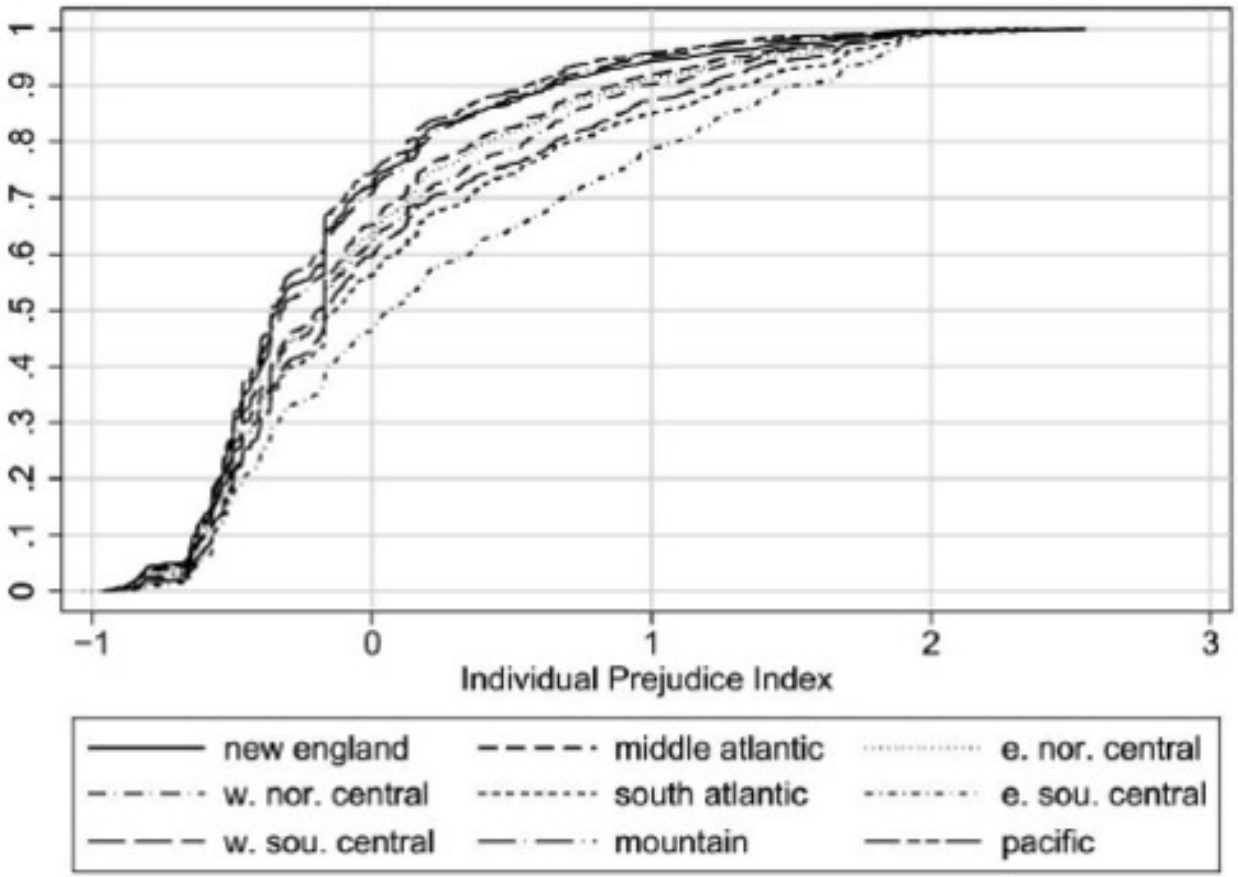


FIG. A1.—Cumulative distribution of the individual prejudice index, by census division

Empirical Test

- Two step procedure.
  1. Estimate Mincer wage regression (CPS May/MORG data, 1979-2002), controlling for education, potential experience, year effects, state effects, black dummy interacted with state effects.
  2. Regress estimated residual wage gap in every state on measures of prejudice and fraction black (WLS, weights are precision of first step estimates).

TABLE 3

ESTIMATED RELATIONSHIP BETWEEN RACIAL PREJUDICE OF WHITES IN A LABOR MARKET AND BLACK-WHITE RELATIVE WAGES

Dependent Variable: Residual Black-White Wage Gap in Market

Measure of Prejudice among All Whites	(1)	(2)	(3)	(4)	(5)	(6)
Average	−.036		.097	.050		
	(.030)		(.029)	(.033)		
Marginal		−.213	−.328	−.202		
		(.040)	(.050)	(.068)		
10th percentile					−.212	−.292
					(.180)	(.125)
Median					−.006	.007
					(.062)	(.043)
90th percentile					.016	.016
					(.029)	(.020)
Fraction black				−.157		−.304
				(.062)		(.045)
State	45	45	45	45	45	45
R^2	.03	.40	.52	.59	.05	.56

NOTE.—The table reports coefficients (standard errors) from OLS regressions of residual state-level black-white wage gaps on various measures of prejudice among all whites (the mean of the black-white wage gap across states is −0.123, and the standard deviation is .044). Residual black-white wage gaps are estimated using 1977–2002 May/MORG CPS data and control for education, a quadratic in experience, race-specific year effects, and state effects. Data from 1973–76 are dropped because the CPS reports states in groups in those years. States are dropped if they are not sampled in the GSS in the years necessary to measure the marginal index of prejudice. The "marginal" is the pth percentile of the prejudice distribution of the relevant population of whites, where p is the fraction of the population that is black. See the text for details.

Strong support for all predictions of the theory.

- Average prejudice has no effect (when no other controls).
- Marginal prejudice strongly negatively correlated with wage gap, even when controlling for average prejudice and fraction black.
- Fraction black strongly negatively correlated with wage gap.
- Median and 90th percentile prejudice don't matter, 10th percentile does (despite there being less variation in 10th percentile).
- Back of the envelope calculation: prejudice can explain about 25% of wage gap.
  - Large, but it means that an even larger difference in the wage gap must be attributed to other factors.

Wage discrimination

- Employer taste-based discrimination should vanish with competition.
- Black and Strahan: look at bank deregulation in the 1970s.
  - Bank regulations prevented banks from entering new markets: restrictions to competition.
  - States gradually lifted these restrictions between 1975 and 2000.
- Prediction of the model: lifting of bank regulations should reduce discriminatory practices.
- Evidence: female wages fell less relative to men, female share in managerial positions increased.
  - Diff in diff to separate effects from secular trends in female wages and occupational choice.

TABLE 1—YEAR OF DEREGULATION OF RESTRICTIONS ON GEOGRAPHICAL EXPANSION, BY STATE

State	Multibank holding companies permitted	Intrastate branching via M&A	Unrestricted intrastate branching permitted	Interstate banking permitted
Alabama	<1970	1981	1990	1987
Alaska	<1970	<1970	<1970	1982
Arizona	<1970	<1970	<1970	1986
Arkansas	1985	1994	**	1989
California	<1970	<1970	<1970	1987
Colorado	<1970	1991	**	1988
Connecticut	<1970	1980	1988	1983
Delaware	<1970	<1970	<1970	1988
DC	<1970	<1970	<1970	1985
Florida	<1970	1988	1988	1985
Georgia	1976	1983	**	1985
Hawaii	<1970	1986	1986	**
Idaho	<1970	<1970	<1970	1985
Illinois	1982	1988	1993	1986
Indiana	1985	1989	1991	1986
Iowa	1984	**	**	1991
Kansas	1985	1987	1990	1992
Kentucky	1984	1990	**	1984
Louisiana	1985	1988	1988	1987
Maine	<1970	1975	1975	1978
Maryland	<1970	<1970	<1970	1985
Massachusetts	<1970	1984	1984	1983

TABLE 5—THE MARGINAL IMPACT OF DEREGULATION ON THE WAGES OF BANKING EMPLOYEES
DIFFERENTIAL EFFECTS FOR MEN AND WOMEN (CPS DATA)

	Females only		Males only	
Post-M&A branching deregulation	−0.029*	—	−0.125*	—
	(0.012)		(0.024)	
Post-interstate banking	0.012	0.009	−0.026	−0.027
	(0.017)	(0.017)	(0.027)	(0.029)
Branching deregulation index	—	−0.017*	—	−0.056*
		(0.006)		(0.013)
N	336,121	328,208	473,246	462,357
F (H_0: all regulatory variables = 0)	3.42*	4.23*	14.33*	10.14*
R^2	0.28	0.28	0.34	0.34

Notes: Standard errors are in parentheses. The dependent variable equals the log weekly wage for all male or female full-time employees in the March Current Population Survey (CPS). The log wage equation also allows for time-varying returns to worker characteristics (which include education broken down into less than high school, high school, some college, and college, age, age squared, race, and sex), time-varying state effects, banking-specific year effects, and banking-specific state effects. The coefficients presented above are the estimates on the interaction between a banking indicator and the deregulation variable. The sample includes data from 1977 to 1997. We drop all observations during the year of deregulation and for two states, South Dakota and Delaware. The deregulation index equals 0 if a state permits neither branching via M&A, nor de novo branching, nor the formation of multibank holding companies; otherwise, the index equals the sum of the number of ways banks may expand within state. For instance, if a state permits multibank holding companies and branching via M&A, the index equals 2. The unit banking interaction equals 1 if the state began the sample with a complete prohibition on branching. Because we have multiple observations per state/year for both banking and nonbanking employees, we estimate robust standard errors where the observations are clustered on state, year, and industry (banking versus all other).

* Statistically significant at the 10-percent level.

Drop in wages for both men and women, but larger for men.

TABLE 6—THE MARGINAL IMPACT OF DEREGULATION ON OCCUPATIONAL DISTRIBUTION AND WAGES BY SEX

	Logit of share of managerial positions held by women (state-level aggregate)	Managerial workers		Nonmanagerial workers		Nonmanagerial workers (tellers only for banking)	
		Female	Male	Female	Male	Female	Male
Post-M&A branching	0.271*	−0.027	−0.089*	−0.035*	−0.194*	−0.005	−0.290*
	(0.161)	(0.024)	(0.025)	(0.012)	(0.042)	(0.023)	(0.091)
Post-interstate banking	0.131	0.033	−0.026	0.013	−0.015	0.008	0.030
	(0.174)	(0.041)	(0.028)	(0.018)	(0.050)	(0.031)	(0.117)
N	944	110,100	146,477	226,021	326,769	217,710	324,878
F (H_0: all regulatory variables = 0)	1.65	0.83	6.69*	4.47*	11.14*	0.05	5.21*
R^2	0.20	0.23	0.26	0.16	0.27	0.16	0.27

Notes: Standard errors are in parentheses. Dependent variable for the share of managerial positions held by women is based on the average value observed for all banking employees in the March CPS during a given state and year. We use the logistic transformation for this variable; the regressions are weighted by the square root of number of employees (i.e., the cell size) times the product of the predicted share times one minus the predicted share from a first-stage OLS model [see Greene (1993) for details]. The results based on residual wages are based on individual banking employee data, as in Tables 4 and 6. The wage results use the log weekly wage for all male or female full-time employees in the March Current Population Survey (CPS) for each occupation group as the dependent variable. The log wage equation also allows for time-varying returns to worker characteristics (which include education broken down into less than high school, high school, some college, and college, age, age squared, race, and sex), time-varying state effects, banking-specific year effects, and banking-specific state effects. The coefficients presented above are the estimates on the interaction between a banking indicator and the deregulation variable. The sample includes data from 1977 to 1997. We drop all observations during the year of deregulation and for two states, South Dakota and Delaware. Because we have multiple observations per state/year for both banking and nonbanking employees, we estimate robust standard errors where the observations are clustered on state, year, and industry (banking versus all other).

* Statistically significant at the 10-percent level.

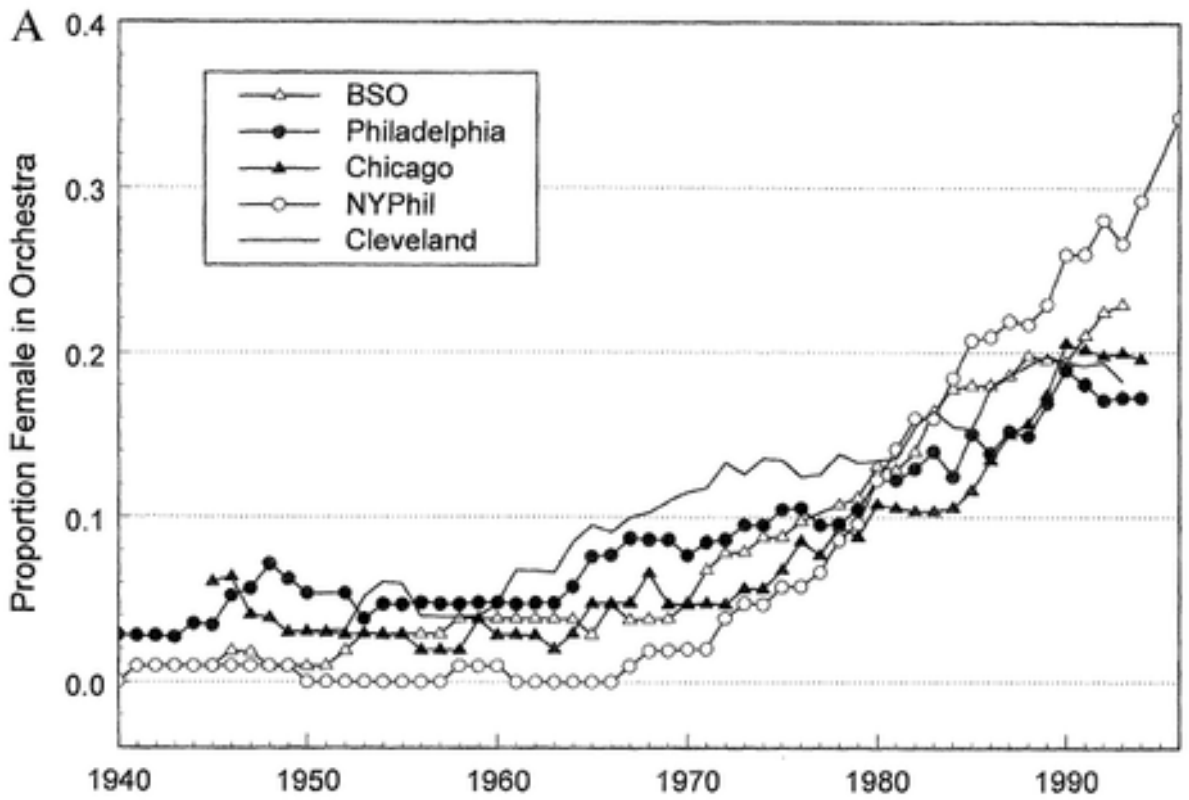
- Levine, Levkov and Rubinstein (NBER WP 2008): similar analysis for black-white race gap.
- But they look at the effect of banking deregulation on non-financial firms.
  - Bank deregulation facilitated access to credit, spurred entry of new firms.
- Both racial wage gap and racial segregation in the workplace were reduced following deregulation.

Discrimination in hiring

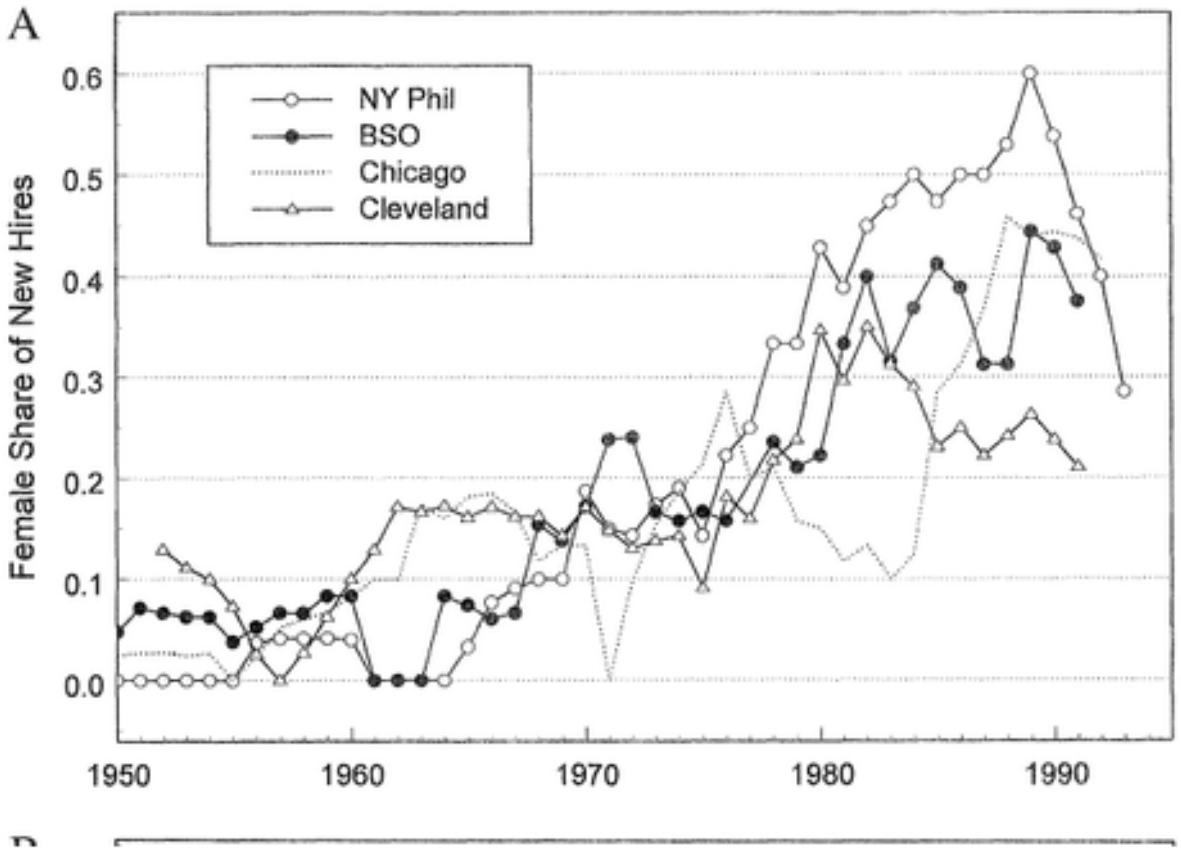
- Goldin and Rouse (AER 2000): the impact of blind auditions on hiring of female musicians.
- Symphony orchestras: new hires must go through a lengthy process of auditions.
- Music directors almost exclusively male, some known to be prejudiced.

  Claims abound in the world of music that "women have smaller techniques than men," "are more temperamental and more likely to demand special attention or treatment," and that "the more women [in an orchestra], the poorer the sound."

  Zubin Mehta: "I just don't think women should be in an orchestra"
- Starting in 1970s, many orchestras started introducing screens, making the auditions "blind."



- Pretty stark trend break in the proportion female, starting in 1970s.



- ...and in the female share of new hires

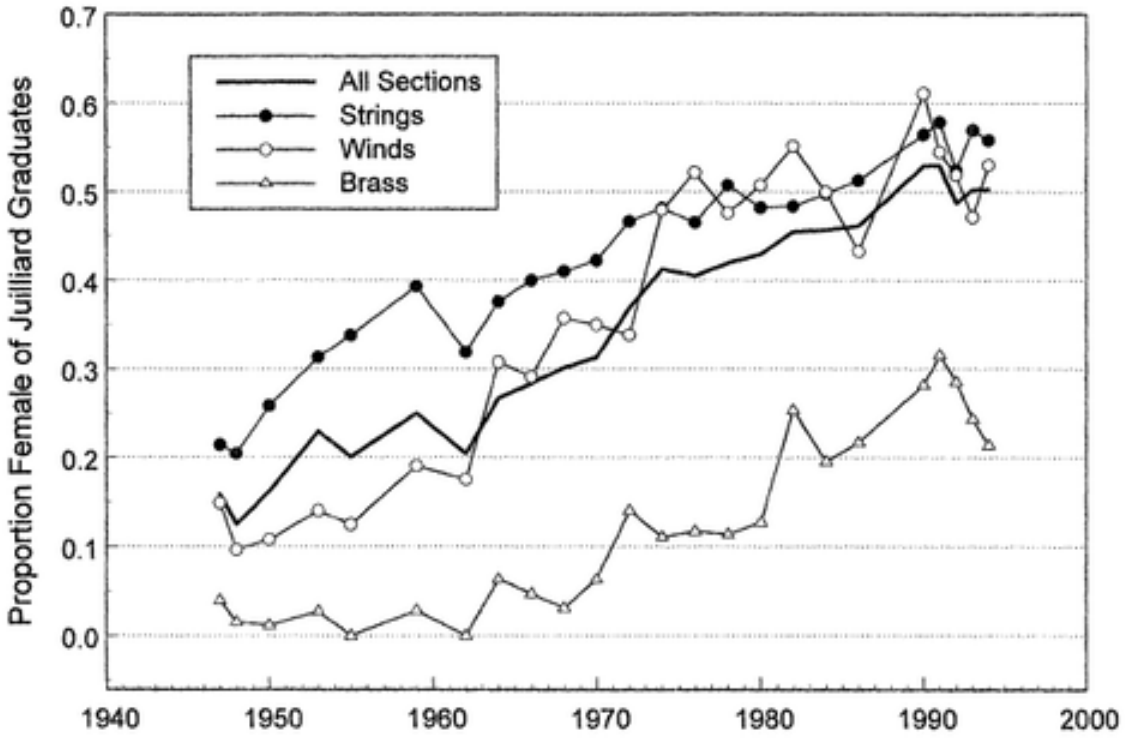


FIGURE 4. PROPORTION FEMALE OF JUILLIARD GRADUATES, TOTAL AND BY SECTION: 1947 TO 1995

Source: Juilliard Music School files.

- No trend break in females graduating from (one) top music school.

TABLE 1—ORCHESTRA AUDITION PROCEDURE SUMMARY TABLE

Orchestra	Preliminaries	Semifinals	Finals
A	Blind since 1973	Blind (varies) since 1973	Not blind
B	Blind since at least 1967	Use of screen varies	Blind 1967–1969; since winter 1994
C	Blind since at least 1979 (definitely after 1972)	Not blind: 1991–present Blind: 1984–1987	Not blind
D	Blind since 1986	Blind since 1986; varies until 1993	1st part blind since 1993; 2nd part not blind
E	Use of screen varies until 1981	Use of screen varies	Not blind
F	Blind since at least 1972	Blind since at least 1972	Blind since at least 1972
G	Blind since 1986	Use of screen varies	Not blind
H	Blind since 1970	Not blind	Not blind
I	Blind since 1979	Blind since 1979	Blind since fall 1983
J	Blind since 1952	Blind since 1952	Not blind
K	Not blind	Not blind	Not blind

Notes: The 11 orchestras (A through K) are those in the roster sample described in the text. A subset of eight form the audition sample (also described in the text). All orchestras in the sample are major big-city U.S. symphony orchestras and include the "Big Five."

Sources: Orchestra union contracts (from orchestra personnel managers and libraries), personal conversations with orchestra personnel managers, and our mail survey of current orchestra members who were hired during the probable period of screen adoption.

TABLE 4—AVERAGE SUCCESS AT AUDITIONS BY SEX, YEAR, AND ROUND OF AUDITION

	Relative female success		
Year	All auditions	Completely blind auditions	Not completely blind auditions
All	−0.001	−0.022	0.006
	(0.008)	(0.012)	(0.010)
Pre-1970	0.053		0.053
	(0.115)		(0.115)
1970–1979	0.001		0.001
	(0.021)		(0.021)
1980–1989	−0.006	−0.039	0.010
	(0.009)	(0.016)	(0.009)
1990+	−0.003	−0.001	−0.003
	(0.010)	(0.017)	(0.013)
Round	All rounds	Blind rounds	Not-blind rounds
Preliminaries, without semifinals	−0.032	−0.048	0.012
	(0.019)	(0.021)	(0.040)
Preliminaries, with semifinals	−0.048	−0.052	0.116
	(0.016)	(0.016)	(0.228)
Semifinals	−0.030	−0.059	0.071
	(0.038)	(0.044)	(0.080)
Finals	0.009	−0.028	0.016
	(0.036)	(0.102)	(0.038)

Notes: For the top part of the table "success" is a "hire," whereas for the bottom portion "success" is advancement from one stage of an audition to the next. The unit of observation for the top portion is the audition, whereas it is the round for the bottom portion (e.g., relative female success in the top portion of the table is averaged across the auditions). Standard errors are in parentheses. "Relative female success" is the proportion of women advanced (or hired) minus the proportion of men advanced (or hired). By hired, we mean those who were advanced from the final round out of the entire audition.
Source: Eight-orchestra audition sample. See text.

TABLE 5—AVERAGE SUCCESS AT AUDITIONS BY SEX AND STAGE OF AUDITION FOR THE SUBSET OF MUSICIANS WHO AUDITIONED BOTH BLIND AND NOT BLIND

	Blind		Not blind	
	Proportion advanced	Number of person-rounds	Proportion advanced	Number of person-rounds
	Preliminaries without semifinals			
Women	0.286	112	0.193	93
	(0.043)		(0.041)	
Men	0.202	247	0.225	187
	(0.026)		(0.031)	
	Preliminaries with semifinals			
Women	0.200	20	0.133	15
	(0.092)		(0.091)	
Men	0.083	12	0.000	8
	(0.083)		(0.000)	
	Semifinals			
Women	0.385	65	0.568	44
	(0.061)		(0.075)	
Men	0.368	68	0.295	44
	(0.059)		(0.069)	
	Finals			
Women	0.235	17	0.087	23
	(0.106)		(0.060)	
Men	0.000	12	0.133	15
	(0.000)		(0.091)	
	"Hired"			
Women	0.027	445	0.017	599
	(0.008)		(0.005)	
Men	0.026	816	0.027	1102
	(0.005)		(0.005)	

Notes: The unit of observation is a person-round. Standard errors are in parentheses. For the round in question, only musicians who auditioned more than once and who auditioned at least once behind a screen and at least once without a screen are included. "Hired" means those who were advanced from the final round out of the entire audition. Blind in the "hired" category means for all rounds. Not blind in the "hired" category means that at least one round was not blind. This difference in the definition of what constitutes a "blind" round or audition is one reason why the number of observations in the first four panels is less than the number of observations in the "hired" panel. The number of observations also differ because we exclude auditions or rounds in which no individual is advanced or in which there are only women or no women. Finally, unlike in subsequent tables, we exclude a few candidates for whom we could not determine or impute their sex. Note that the binding constraint for the preliminaries is the not-blind category, for which we have only one orchestra. The binding constraint in the "hired" category are the blind auditions, for which we have (at most) three orchestras. Musicians can appear more than once in either the blind or not-blind categories.

Source: Eight-orchestra audition sample. See text.

Audit/Correspondence Studies

- Audit study: hire pairs of RA/actors, one minority, one nonminority, have them apply to jobs (apartment rentals, loans, etc.) and test for discrimination.
- Advantages: apparently "clean" evidence of discrimination.
- Disadvantages:
  - small samples
  - differences between auditors other than race/gender/etc.
  - Auditors know that the study is about discrimination, may affect their demeanor.
- Alternative: correspondence studies. Send identical resumes to firms, varying only one characteristic (race or gender)

Bertrand and Mullainathan (AER 2004)

- "Are Emily and Greg More Employable than Lakisha and Jamal? A Field Experiment on Labor Market Discrimination."
  - Title says it all...

- First names can be very clear indicators of race. The phenomenon of distinctively black names increased since the 1960s.

- Sent many almost identical resumes in response to ads in Boston Globe and Chicago Tribune.

- Some resumes had distinctively black names.

TABLE 1—MEAN CALLBACK RATES BY RACIAL SOUNDINGNESS OF NAMES

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

Notes: The table reports, for the entire sample and different subsamples of sent resumes, the callback rates for applicants with a White-sounding name (column 1) an an African-American-sounding name (column 2), as well as the ratio (column 3) and difference (column 4) of these callback rates. In brackets in each cell is the number of resumes sent in that cell. Column 4 also reports the p-value for a test of proportion testing the null hypothesis that the callback rates are equal across racial groups.

- Blacks have substantially lower callback rates.

TABLE 4—AVERAGE CALLBACK RATES BY RACIAL SOUNDINGNESS OF NAMES AND RESUME QUALITY

	Low	High	Ratio	Difference (p-value)
Panel A: Subjective Measure of Quality (Percent Callback)				
White names	8.50	10.79	1.27	2.29
	[1,212]	[1,223]		(0.0557)
African-American names	6.19	6.70	1.08	0.51
	[1,212]	[1,223]		(0.6084)
Panel B: Predicted Measure of Quality (Percent Callback)				
White names	7.18	13.60	1.89	6.42
	[822]	[816]		(0.0000)
African-American names	5.37	8.60	1.60	3.23
	[819]	[814]		(0.0104)

Notes: Panel A reports the mean callback percents for applicant with a White name (row 1) and African-American name (row 2) depending on whether the resume was subjectively qualified as a lower quality or higher quality. In brackets is the number of resumes sent for each race/quality group. The last column reports the p-value of a test of proportion testing the null hypothesis that the callback rates are equal across quality groups within each racial group. For Panel B, we use a third of the sample to estimate a probit regression of the callback dummy on the set of resume characteristics as displayed in Table 3. We further control for a sex dummy, a city dummy, six occupation dummies, and a vector of dummy variables for job requirements as listed in the employment ad (see Section III, subsection D, for details). We then use the estimated coefficients on the set of resume characteristics to estimate a predicted callback for the remaining resumes (two-thirds of the sample). We call "high-quality" resumes the resumes that rank above the median predicted callback and "low-quality" resumes the resumes that rank below the median predicted callback. In brackets is the number of resumes sent for each race/quality group. The last column reports the p-value of a test of proportion testing the null hypothesis that the callback percents are equal across quality groups within each racial group.

Lower returns to resume quality for blacks