Lecture Note: The Economics of Discrimination – Evidence

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1 Evidence on economic discrimination

There is a large empirical literature testing for the presence of discrimination in market and non-market settings. Studies can be roughly broken into the following categories:

1. Regression studies. These models attempt to interpret the meaning of a race or gender coefficient in an OLS model, typically a wage model. These studies often use a Oaxaca-Blinder decomposition to interpret the magnitude of findings. These analyses do not for the most part meet contemporary standards of evidence. The 1996 JPE paper by Neal and Johnson is a rare exception of a contemporary regression study that had a major impact.

2. Audit studies and 'kindred' field experiments. These studies attempt to 'randomize' race to evaluate if minorities are treated differently in job application, housing search, vehicle purchases, etc. Thanks to the Bertrand-Mullainathan innovation of using resumes to randomize applicant characteristics, this literature is enjoying a resurgence.

3. Lab experiments. In a non-market setting, experimenters look for evidence of disparate treatment of members of race or gender groups.

4. Quasi-experiments where race/gender is alternatively revealed or concealed. There are few studies in this genre. The Goldin-Rouse paper in the AER in 2001 is a nice one.

5. Learning models: Apply structural models to make inferences about what employers believe initially and how they update beliefs as productivity is revealed over time. The key tool here is to use information known to the econometrician at market entry (such as test scores) but not known by employers except through its revealed effect on productivity or its correlation with other observables.

2 Experimental studies: Fershtman and Gneezy (2001, QJE)

There are numerous experimental studies of discrimination. This is a particularly nice one. Basic idea: Randomize the ethnic identity of player faced in a series of games. Look for disparate treatment in games that attempt to isolate taste-based and statistical motives for possible discrimination.

Question: Do Ashkenazic and Eastern Jews appear to discriminate against one another. Four questions:

1. Is there differential treatment based on ethnic affiliation?
2. Does the discrimination reflect group bias in that each player favors members of his own group – or is there systematic discrimination by all against some?
3. Is discrimination simply taste-based or does it reflect the players' assessments of the differing reactions of members of groups to their actions (i.e., retaliation).
4. Are these assessments (stereotypes) accurate?

On this fourth point, note that statistical discrimination based on bad statistics (i.e., inaccurate assessments) is notionally distinct from simple taste-based discrimination, but unlike 'accurate statistical discrimination,' it's not a 'rational' economic response to group differences.

All experiments involve transfers of money between players. Transactions are done without personal contact, but names are written on paper, and are identifiably ethnic. All transactions occur over several days – so, the expectation of immediacy that might act like a disciplinary device for cooperation is absent. All results focus on males until end of paper.

Findings:

- **Trust game**:
- Transfer money to a player. Experimenter triples it. Recipients decides how much to return.
- Figure I: Male A Jews receive much larger transfers on average than male E Jews. In fact, 60% get the full amount (approximately equal to \$25 $\times$ 3).
- Figures IIIa and IIIb: Remarkably, E and A Jews treat E and A Jews similarly. In other words, neither group appears to 'trust' E Jews as much as A Jews.

- Is this statistical discrimination – i.e., are E Jews less 'trustworthy?' See Table II. There do not appear to be any differences in average amounts returned by E and A Jews (would be interesting to see distributions as well as means).
- **Taste for discrim: Dictator Game.**
- Same as trust game – transfer money, experimenter triples – but in this case, Player B is completely passive. No money can be returned. So, it's just taste for generosity.
- See Figure IV. Here, the average transfers to each ethnic group are similar but the distributions are not. More A Jews received zero, more E Jews received 5. Again, the ethnicity of the sender was not predictive of the amount sent – only the ethnicity of the recipient.
- **Stereotypes: Reaction to unfair treatment. Ultimatum game**
- There is a belief that E Jews are more likely than A Jews to act harshly in response to a perception of unfairness, i.e., have a greater sense of honor/humiliation.
- Test this with Ultimatum game. Here, Player A proposes a division. The amount given to Player B is tripled. Then Player B can accept or refuse.
- See Figure V. In this case, E Jews generally receive more. The modal A Jew receives 5 of 20 NIS (which will be tripled to 15, so this is an equal division ex post). The modal E Jew receives 10 of 20 NIS, which will be tripled to 30, so he gets a larger share of pie.
- One interpretation: Honor. A second interpretation (not in paper): Pandering. Players may not believe that E Jews will correctly understand that '5' is an equal division – and hence will reject.
- Is this discrimination rational? No evidence that E Jews systematically more likely to reject an offer...
- **Gender differences:**
- Women do not appear to take ethnicity or gender into account.

- **Conclusions**
- A puzzling mix.
  1. There are clearly major differences in how males treat other males by ethnicity.
  2. But, this disparate treatment is not own-group biased. Both E and A Jews treat E Jews differently from A Jews. It appears they trust E Jews less.
  3. But E Jews do not appear less trustworthy conditional on receiving a transfer.
  4. Does not appear motivated by animus. In the Dictator game, groups receive roughly equal treatment.
  5. In the ultimatum game, behavior of proposers is consistent with beliefs that other group will conform to stereotype of honor/humiliation.
  6. But no evidence that groups do behave this way.
- Appears to be statistical discrimination based on bad statistics...
- Question: How much can we learn from lab experiments about the importance of discrimination in markets at the margin (as opposed to on average). One can choose to draw strong inferences from these findings or dismiss them entirely based because they are 'artificial.'

3 'Market' studies: Orchestrating Impartiality, Goldin and Rouse (AER, 2001)

This is a very creative study that attempts to isolate the importance of gender preference in a more natural, market-like (albeit rarefied) setting: orchestra auditions. Very simple idea: During the 1970s since 1990s, some orchestras started using screens during solo auditions to hide the identity of auditioners. Women were historically viewed as unsuitable for orchestras. Did the use of blind screens improve their chances of getting a job?

- See Table 1 for summary on the implementation of screens
- See Figure 3 for long-term trends in female hiring

- Table 4: On average, women do *worse* on blind rounds. But this could be due to changing composition of female pool. Possible that only the very best women competed when the game was lopsided.

- Table 5: Models limited to musicians (male and female) who auditioned both blind and non-blind suggest that women did relatively better in blind rounds (diff-females minus diff-males).

- Table 6 gives the main estimates.

- All of these models exclude orchestra fixed effects because there is no 'within' variation in orchestra blind policies.

- Table 7 estimates models for the 3 orchestras that switch policies, can include musician and orchestra fixed effects. Results are similar to Table 6, but less precise.

- Conclusion: Fascinating. Hard to generalize.

4 Audit studies and field experiments: Bertrand and Mullainathan (2003)

Limitation of canonical audit studies:

1. Tiny samples b/c expensive

2. Not double-blind. Experimenters may have an agenda.

3. Artificial setting: Experimenters do not intend to complete the transaction (e.g., take the job, buy the car, rent the apartment...)

4. Do not necessarily measure discrimination at the margin

Great idea: Apply for jobs by sending resume by mail or fax. Manipulate perceptions of race by using distinctively ethnic names. Otherwise, hold constant resume characteristics. Are 'callback' rates lower for distinctively black-named applicants?

- Table 1: Short answer is yes. Callback rates are lower for black sounding names.

- Table 2: In most cases, names receive equal treatment (a point James Heckman made about audit studies in his *JEP* article in the symposium on race listed on your syllabus). But that's because in most cases, applicants are not called back.
- Table 4: Black names appear to benefit less from resume enhancements (such as honors, more experience) than do whites. Authors view this as evidence against statistical discrimination – should work in opposite direction they believe. Is this true?
- See also Table 5
- Table 6: Discrimination based on zip-code characteristics appears quite important, and does not systematically differ between white and non-white names. (Authors also view this as evidence against statistical discrimination.) This does not seem to greatly reduce importance of racial 'soundingness' of names.
- Table 11: Considerable overlap in distributions of outcomes between white and black sounding names

Conclusion: Controversial paper that will spark a great deal of other work in this vein. Questions remaining: How do we translate call backs into outcomes we care about? What is the model of statistical discrimination they are contrasting their findings too?

4.1 Responses

There are already two response papers to B-M.

One is by Levitt and Fryer, who show using detailed birth records data that non-white names not substantially correlated with life outcomes once one conditions finely on mother/birth characteristics. B-M response: Not a surprise; it's not the name, and that's name is taken as proxy for race when race is shielded.

The paper by Figlio presents some fascinating initial evidence that names *do* matter. Within families, children who receive distinctively black names appear to have slightly lower test scores, are less likely to be labeled as 'gifted,' and are more likely to be labeled 'learning disabled.'

5 Regression Studies: Neal and Johnson (1996)

- This is the rare modern regression study that had a major impact. Why? Because what it give up in cleverness it more than compensates for in substance.

- This work is related to *Bell Curve* (1994), which generate a huge response literature by economists and sociologists in the late-1990s.

- See also Heckman's superb review of this book in the *JPE*.

- Basic question: How much of the B/W earnings gap is explained by differences in skills that are formed prior to market entry? This is a great question because so much of literature is focused on market discrimination. Is this focus misplaced. NJ say yes.

- Ideal test: Look at identically skilled teens before market entry and then again later in life: What is the initial earnings gap and does it grow over time. Assuming there are no diffs in tastes or costs of skill investment, one could attribute diffs to discrimination. (Idea is akin to randomly assigning race.)

- Since can't do that, use NLSY. Sample 15 to 23 years old, who took AFQT prior to age 18. Regress age effects out of AFQT.

- See Table 1: The basic result. 'Pre-market' skill appears to explain a large part of racial earnings gap for currently employed workers.

- This is the main message of the paper. Now, they ask a bunch of good questions.

5.1 Is AFQT racially biased?

- Military has done validation studies using objective performance measures. Finds no evidence that test systematically under-predicts performance of blacks.

5.2 Do blacks underinvest in skills b/c return is lower?

- This is exceedingly difficult to test.

- In Table 2 and 3, no evidence of differences in return to AFQT. But this is an endogenous comparison – conditional on having attained a given level of skill.
- Moreover, return may have been lower in prior eras, and this could affect beliefs of black parents, who choose how much to invest in kids' skills.
- How would you convincingly test this? What does this imply that we should see for IV estimates of returns to school for blacks versus whites?

5.3 Selection into labor market participation

- See Figure 1. Low scoring blacks noticeably less likely to participate in labor market.
- How do we deal with this? Two approaches:
- 1. Median regressions. If nonparticipants have wage offers less than median for group with similar observables (race, test score) and at least half participate, then the median is identified. Bingo. See Table 4. This table suggests that less is explained of the gap once we condition on participation.
- 2. Smith and Welch method (this is kind of cool):

$$E(w) = LFPR \cdot E\left(w|\text{participate}\right) + (1 - LFPR) \cdot E\left(w|\text{don't participate}\right).$$

Therefore

$$\frac{E\left(w_i\right)}{E\left(w_j\right)} = \frac{LFPR_i \cdot E\left(w_i|\text{participate}\right) + (1 - LFPR_i) \cdot E\left(w_i|\text{don't participate}\right)}{LFPR_j \cdot E\left(w_j|\text{participate}\right) + (1 - LFPR_j) \cdot E\left(w_j|\text{don't participate}\right)},$$

$$\frac{E\left(w_i\right)}{E\left(w_j\right)} = \left[\frac{E\left(w_i|\text{participate}\right)}{E\left(w_j|\text{participate}\right)}\right] \cdot \left[\frac{\left(1 - k_i\right) LFPR_i + k_i}{\left(1 - k_j\right) LFPR_j + k_j}\right],$$

where

$$k_i = \frac{E\left(w_i|\text{don't participate}\right)}{E\left(w_i|\text{participate}\right)}.$$

So, can adjust the observed earnings ratio by a selection factor. Notice that OLS wage gap is -0.072 and median gap is -0.134, implying

$$\left[\frac{E\left(w_i|\text{participate}\right)}{E\left(w_j|\text{participate}\right)}\right] = 0.93$$

and

$$\frac{E(w_i)}{E(w_j)} = \left[\frac{E(w_i|\text{participate})}{E(w_j|\text{participate})}\right] \cdot \left[\frac{(1-k_i)\,LFPR_i + k_i}{(1-k_j)\,LFPR_j + k_j}\right] = 0.875.$$

Using $k_b = k_w = 0.1$ and noting that $LFPR_b = 0.91$ and $LFPR_w = 0.94$, we have

$$\left[\frac{(1-0.1)\cdot 0.91 + 0.1}{(1-0.1)\cdot 0.975 + 0.1}\right] = 0.94,$$

and

$$0.93 \cdot 0.94 = 0.874.$$

So, by making a *very* conservative assumption on potential earnings of non-employed, we get back the observed ratio. Of course, this calculation does not account for AFQT scores (it's treated as unconditional). If score gap is much larger among non-employed, this suddenly does not seem so conservative.

5.4 Determinants of AFQT scores

- Figures 2 and 3.
- Figures 5 and 6.
- There is a 1.0 SD test score gap. This is very large. (But both groups have gained more than this amount over century due to 'Flynn effect.')
- Adding a bunch of family background, home environment, and school quality covariates reduces this considerably. Suggests that pre-market factors have a lot of potential explanatory power for this gap.
- *Bell Curve* argued these score differences are genetic – inherent ability.
- But score gap is larger in older cohorts, suggesting skills investment (Table A3)
- When quarter-of-birth used as instrument for schooling, significantly increases AFQT (not shown). Again suggests some part of cognitive skill acquired.

5.5 Conclusion

- This was the right paper for its time. It moved the literature simply by presenting conditional means. But this would probably be a very risky job paper.

6 Learning Models: Farber and Gibbons (1996)

Though it may not seem immediately related, the 1996 *QJE* paper by Farber and Gibbons contributes to economic understanding of how information is resolved over time in labor markets. The follow-up paper by Altonji and Pierret, *QJE* 2001, shows exactly how the tools developed by F&G can be used to test for the presence of statistical discrimination (something Farber and Gibbons may have overlooked).

Basic idea:

- When a person enters the labor market, some things about productivity are known to employers, but much is not yet known.
- Given the right data, there may be things about productivity known to econometrician that cannot be known to employers (like AFQT scores).
- Employers should learn this productivity information as they gather information about workers' productivity.
- Question: What does this learning process imply about wage dynamics?

Two strong initial assumptions:

1. Wages equal expected output at each date (no long term contracts).
2. "The stochastic component of a worker's output has a time invariant distribution, so human capital acquisition is deterministic, and both innate ability and acquired skill have the same value in all jobs." [Translation: error distribution for individual wages is stable at all times, so expected output at any give data is all we need to know for wage determination.]

6.1 Theory: Time invariant worker characteristics

- Let η_i and s_i describe worker's time-invariant ability and (fixed) schooling.
- Assumption: η_i not observed by employers directly
- Let X_i be a vector of time-invariant worker attributes (race, gender, date of birth) observable to employers and included in data.
- Let Z_i be a vector of time-invariant worker attributes that are not included in data.
- Let B_i be a vector of time-invariant worker characteristics that are observed in data but not by employers (such as test scores).
- Write the joint distribution of these attributes as $F(\eta_i, s_i, X_i, Z_i, B_i)$.
- Let y_{it} be the output of worker i in the t^{th} period in which she is labor market.
- Assume that outputs $\{y_{it} : t = 1, ..., T\}$ are independent draws from conditional distribution $G(y_{it}|\eta_i, s_i, X_i, Z_i)$ Note that B_i does not appear in this expression, meaning that it only affects productivity through its relation to other variables (such as η).
- Now assume
  1. All employers know the joint distribution $F(\eta_i, s_i, X_i, Z_i, B_i)$ and conditional distribution $G(y_{it}|\eta_i, s_i, X_i, Z_i)$.
  2. All observe schooling s_i and other worker characteristics X_i, Z_i.
  3. All observe the sequence of outputs $\{y_{i1}, ..., y_{iT}\}$. This last assumption is not mild: **'public learning.'**
- Given these assumptions, wage paid to a worker is expected output given all available information

$$w_{it} = E(y_{it}|s_i, X_i, Z_i, y_{i1}, ..., y_{it-1}).$$

- The rest of the theoretical part of the paper develops implications. These arguments are subtle, but intuition is pretty accessible.

6.2 Three predictions: (1) Effect of schooling on wages

- Consider a cohort of workers entering the labor market simultaneously. For each worker, we observe s_i, X_i and the wage, but *not* output.

- We estimate the following equation for the *level* of earnings (one thing that's unusual about the theory: written in levels not logs, which therefore requires them do the analysis in levels).

  $$w_{it} = \alpha_t + \beta_t s_i + X_i \gamma_t + \varepsilon_{it}.$$

  Estimated coefficients from this regression are coefficients from linear projection of w_{it} on s_i and X_i. A linear projection is analogous to a conditional expectation function (CEF) except that we have constrained the relationship to linearity. [It could be that the true conditional expectation of structural equation is a non-linear function of independent variables.] Most of us use this terminology loosely, but the F&G usage is more precise.

- Denote linear projection as $E^* (\cdot)$.

  $$E^* (w_{it}|s_i, X_i) = \hat{\alpha}_t + \hat{\beta}_t s_i + X_i \hat{\gamma}_t, \tag{1}$$

  where the $\widehat{hats}$ denote estimated coefficients. Now, iterate expectations:

  $$E^* (E^* (y|X, Z)|X)) = E^* (y|X).$$

  So, although Z_i, B_i are not included in the data, employers will still be able to form unbiased estimates of expected worker productivity just using the $X's$. Why? Because the estimated coefficients $\hat{\alpha}, \hat{\beta}, \hat{\gamma}$ effectively contain the implicit regression of Z, B, on X. So, the omitted variable bias formula says that the coefficients obtained from estimating (1) account for the direct effect of X on w plus the covariance between X and Z, B and the effects of Z, B on w. [You should review/remind yourself of the omitted variable bias formula if you are not already intimately familiar with it. See any intro econometrics text.]

- Some additional iterations gives you

  $$E^* (w_{it}|s_i, X_i) = E^* (y_{it}|s_i, X_i),$$

meaning that the regression equations gives expected output (equal to the wage by assumption).

- Now, given the assumption that outputs are identically distributed, this arguments says that the effect of schooling on the level of wages is independent of experience.

- Intuition (badly needed...). Assumption that wages equal expected output implies not only that 1^{st} period wage w_{i1} is expectation of first period output given s_i, X_i but also that no part of 'innovation in wages' between periods 1 and 2, $w_{i2} - w_{i1}$, can be forecast from the information used to determine w_{i1}. This does not necessarily mean that wages are not expected to grow between these two periods (although, in this version of the model, they are not – but see below). They key condition is that the 'surprise' element of this growth cannot be forecasted from time invariant characteristics. So this means that education's effect on wages is the same in all periods. (Note, in this version of the model: current age and experience are not included in X_i because they are not time invariant.).

- Thus, w_{i2} is equal to w_{i1} plus a term that depends on y_{i1} but is orthogonal to s_i, X_i, $w_{i2} - w_{i1}$. This means that the estimated coefficients for β, γ are the same in all periods. Note that this does not necessarily apply to α.

6.3 Three predictions: (2) Effect of unobserved characteristics

- This second prediction is most important of model.

- Recall that B_i is observed by econometrician but not by employers.

- Other observed variables X, Z, s could be correlated with B, however. So let's purge this correlation using the following procedure:

$$B_i^* = B_i - E\left(B_i | s_i, X_i, w_{i1}\right).$$

  Note that we are including w_{i1} because it contains (by assumption) employers' expectation of productivity at market entry, so by conditioning this out, we are in theory purging everything about B_i that employers may observe.

- Now augment previous regression equation

$$w_{it} = \alpha_t + \beta_t s_i + X_i \gamma_t + B_i^* \pi_t + \varepsilon_{it}.$$

- Question: How will π_t vary with experience? (This is the 'learning' part of the model.) Take B to be scalar. Since B^* is orthogonalized from other regressors, we know that

$$\hat{\pi}_t = \frac{cov\left(B_i^*, w_{it}\right)}{var\left(B_i^*\right)}.$$

Now, note that

$$w_{it} = w_{it-1} + \zeta_{it} = w_{i1} + \sum_{t=2}^{T} \zeta_{it},$$

where ζ_{it} is equal to the 'innovation' in wages in each period following the 1st.

- Since B_i^* is orthogonal to w_{i1} by construction, $\hat{\pi}_0 = 0$. Therefore

$$cov\left(B_i^*, w_{it}\right) = \sum_{t=2}^{T} cov\left(B_i^*, \zeta_{it}\right).$$

In general, this term $cov\left(B_i^*, \zeta_{it}\right)$ will be positive for all $t > 0$. If that regularity condition is satisfied, then $\hat{\pi}_t$ will be rising in t. This is the key result: as experience accumultes, employers will learn about η, and this makes B_i^* increasingly (important) for wage determination.

- Intuition: B_i^* is correlated with ability, and ability affects output. Employers do not observe B_i^* at market entry, but they will learn about output over time. Therefore, the effect of ability – and hence B_i^* – should be rising with market experience. This is quite a nice prediction. It's sensible enough to be intuitive, but sufficiently non-obvious that testing it has power to affirm/reject the plausiblity of the model. Notice: there is no experiment, but still a good test of theory.

6.4 Three predictions: (3) Wage residuals are a martingale

- A martingale is a generalization of a random walk. A random walk has the following two properties:

$$E\left(w_{it+1} | w_{it}\right) = w_{it}, \tag{2}$$

$$var\left(E\left(w_{it+1}|w_{it}\right)\right)=\sigma^2, \tag{3}$$

where σ^2 is not time varying. Note that unconditional variance of w_{it} rises unboundedly with t.

- Like a random walk, a martingale has the the first conditional expectation property. But the second property is more general:

$$var\left(E\left(w_{it+1}|w_{it}\right)\right)=\sigma_t^2.$$

That is, the variance of 'innovations' is not necessarily constant. So, a random walk is a martingale, but a martingale may not be a random walk.

- The martingale property applies readily to the F&G model. Because wages are assumed to incorporate all contemporaneous information on expected productivity, the expected innovation in each period is zero:

$$E\left(\zeta_{it}|w_{it-1}\right)=0,$$

which implies that

$$E\left(w_{it}|w_{it-1}\right)=w_{it-1}.$$

Since successive observations of the wage can be written as

$$w_{it}=w_{i1}+\sum_{t=2}^{T}\zeta_{it},$$

where ζ_{it} are uncorrelated due to the independence of draws assumption. Therefore, $\mathrm{Var}(w_{it})=\mathrm{Var}(w_{i1})+\sum_{t=2}^{T}\sigma_t^2$. This implies that wages (or at least their residuals) are a martingale.

6.5 Productivity that grows with experience

- The problem so far is that they've not really allowed for productivity growth.

- Assume now that productivity grows over time due to experience, human capital, etc.:

$$Y_{it}=y_{it}+h(t),$$

where t is time, and $h(t)$ is the component of output growth due to acquired skill (y_{it} is the part due to fixed characteristics like ability).

- Continue to assume that $\{y_{i1}, ..., y_{iT}\}$ are *iid* draws from $G(y_{it}|\eta_i, s_i, X_i, Z_i)$.
- Of course, $\{Y_{i1}, ..., Y_{iT}\}$ are not *iid*, but if $h(t)$ is deterministic or deterministic plus white noise, then the regression equation becomes something like

$$w_{it} = \alpha_0 + \alpha_1 t + \beta_0 s_i + \beta_1 s_i t + \varepsilon_{it}.$$

So, by conditioning on a trend in experience and education, we can get back to a (conditional) *iid* case.

6.6 Farber and Gibbons: Results

- F&G use the NLSY to examine the earnings dynamics of a cohort of workers. The key features of the NLSY for this analysis:

  1. Panel data – can observe wage dynamics
  2. AFQT – correlated with productivity, probably not initially fully observed by employers
  3. Can measure actual experience rather than potential experience.

- First step is to construct B_i^* as

$$B_i^* = B_i - X_i\hat{\gamma} - \hat{\delta} w_{i0}.$$

By regressing out other observables *and* the 1^{st} period wage, will have orthogonalized this measure to components that are presumably not known at labor market entry. Though if there is measurement error in wage or if wage does not perfectly reflect expected productivity, this is not going to be quite perfect.

- Table 2 tests their two of three of their main hypotheses:

  1. Estimate effect of education on level of wages does not vary with experience

2. Estimated effect of variables correlated with ability but not observed by the market increases with experience (this is the most important prediction of the paper).

- Since regressions are in levels, hard to read. But the estimated return to education is roughly 9 percent, which is plausible in this time period, 1981 - 1990.

- There is no evidence that the relationship between education and earnings varies with time, exactly as the learning model predicts. (This is tested by interacting education with experience; experience is almost synonymous with time within a cohort.)

- This finding is sensitive to inclusion/exclusion of education $\times$ year effects; when these are excluded, interaction *is* significant. This is not surprising given the very rapid rise in the return to education during this period; this will look like an 'age' effect if we don't make it into a 'time' effect by adding education $\times$ time.

- Most striking are the 'returns' on the AFQT (and library card) residuals. The interactions between these variables and experience is positive, significant and large in all specifications. This suggests that employers learn about these variables over time, and so they become 'priced' into the wage.

- One concern here is that result could be contaminated by a rise in the return to ability over this period – again, the general identification problem in distinguishing age from time effects. But F&G results are robust to addition of interactions between calendar dummies and the AFQT and library card variables. This suggests that they have found something real.

- The final question that F&G address is whether wages are a martingale. This is of great technical interest if you do optimal minimum distance (OMD) estimation and/or error components models. This material (which draws on seminal 1984 work by Chamberlain), is deeply important if you want to work on dynamic problems with panel data. The martingale result is probably not of great substantive import to us, however, and so I will not spend class time on it.

6.7 Conclusion: Farber and Gibbons

This is an original and important paper that presents a rigorous way to think about a difficult problem – wage dynamics with employer learning. The key result (in my mind) is that unmeasured skill becomes increasingly important to wage setting over time, as employers' learn about ability. The intuition for this result is quite natural, and that makes the finding all the more powerful.

Digression: A note on age, time, and cohort effects

- Write the wage of a person

  $$w_{iatm} = \gamma_a + \delta_t + \theta_m + \varepsilon_{it},$$

  where a indexes age, t indexes calendar time, and y indexes the year of $i's$ labor market entry. In this notation, γ is an age effect, δ is a time effect, and θ is a cohort effect.

- In theory, all of these 'effects' can be said to exist. That is, there can be a pure wage effect of age (or experience), a pure effect of time (operating through price changes, for example), and a pure effect of being a member of a given cohort (if that cohort has special attributes or, equivalently, if cohorts are imperfect productive substitutes).

- However, there is a fundamental identification problem in measuring these three effects simultaneously. Conditional on year of market entry, age and time are perfectly linearly related – that is, calendar time is simply year of market entry plus age minus age at market entry. So, although we can write this decomposition, we can never estimate all three components simultaneously.

- This identification problem arises repeatedly in labor economics. In particular, if wages are rising for a group of workers, is it because they are getting older (an age effect) or because prices are rising (a time effect)? We'll talk more about this issue in the spring semester.

7 Learning Models: Altonji and Pierret (2001)

Altonji and Pierret pick up a thread that Farber and Gibbons left hanging. F&G considered how employer learning affects the evolution of the wage loading of variables that employers do not originally observe. They did not consider what this implies about the evolution of coefficients for variables that: employers do originally observe **and** which are correlated with observables variables that they do not observe. In fact, their estimation strategy precludes investigating this possibility because they purge B_i of correlations with all observables and the 1^{st} period wage.

Here's where A&P come in. Let's say, for example, that race is correlated with AFQT score, and that employers know this. If employers value AFQT score (because of its link to productivity) and use race (or education) as a statistical signal of expected AFQT (that is, they statistically discriminate), this has an immediate implication for the evolution of both the AFQT and race (or education) coefficient. Specifically, AFQT should become more important in wage setting over the workers' career, whereas conditional on AFQT, race (or education) should become *less* important. This is what Altonji and Pierret test.

There are a few important differences between A&P and F&G:

1. A&P do their analysis in logs, which is slightly easier to deal with.

2. Unlike F&G, they do not orthogonalize B_i with respect to X_i, w_{i0}. This is crucial. If B is orthogonal to other covariates, changes in its loading on wages over time *cannot* by construction affect the coefficients on variables in X such as schooling. Hence, their test requires them not to orthogonalize.

3. They look at statistical discrimination on education and race.

- I won't develop their model, but the intuition is stated succinctly on page 321, "As employers learn about the productivity of workers, s [which is an observable variable, such as schooling] will get less of the credit for an association with productivity that arises because s is correlated with z [variable like AFQT that is initially unobserved, but is positively correlated with both s and output], provided that z is included in the wage equation with a time-dependent coefficient and can claim the credit."

7.1 Results for education as a signal of ability

- See Tables I, II, III. Results here are quite striking.

- In Table I, first column shows that both education and AFQT have an important effect on wages, and that the effect of education declines insignificantly with time.

- In column (2), they add AFQT $\times$ time/10. Coefficients imply that AFQT has essentially zero effect on wages in year of market entry but by year 10, a one standard deviation higher AFQT raises earnings by 7 log points.

- Most strikingly, addition of this measure dramatically changes the effect of education on earnings. In the first year, the 'return' to education is now 8.3 log points (up from 5.9 in the model excluding AFQT $\times$ time).

- But the education $\times$ time interaction is strongly negative. After 10 years in the market, the effect of education is only 6.0 log points.

- This suggests that employers are 'statistically discriminating' on education – that is, they are initially using education as a proxy of unobserved ability, and they rely on this less as ability becomes known.

- Results using sibling wage and father's education work similarly. All can be thought of correlates of underlying productivity that become know to employers with time.

7.2 Is race used for statistical discrimination

- A&P report a 1.1 standard deviation black/white mean difference on the AFQT in their sample – large, but consistent with Neal and Johnson.

- Let's say employers statistically discriminate on race as a proxy for AFQT.

- Absent the AFQT $\times$ time measure, we would expect race to be negative.

- Adding AFQT $\times$ time should make the race main effect more negative, but make the time interaction more positive. That is, if race is taken as a (negative) productivity signal early in the career, it should become less important as actual productivity is revealed. [This does not mean that expectations are inaccurate, but that they are coorborated by AFQT latter – and so AFQT eventually becomes the determinant, not race.]

- Table I shows that the opposite occurs. That is, the race main effect becomes substantially larger when the AFQT $\times$ time interaction is added, and this time interaction is significantly negative. This does not support the inference that employers are using race as a s variable like schooling.

- In fact, a more likely interpretation is the opposite: employers are not 'accounting' for racial differences in expected productivity at hire (witness: the race intercept is insignificantly positive in year 0 of market entry) and then are learning about this over time. They appear 'surprised.'

- This suggest that employers are obeying the legal prohibition on statistical discrimination.

- Other explanations?

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Fershtman, Chaim, and Uri Gneezy. Figures 1, 3a, 3b, 4, 5, Table 2. In "Discrimination in a Segmented Society." Quarterly Journal of Economics 116, no. 1 (2001).

Goldin, Claudia, and C. Rouse. Tables 1-7, Figure 3. In "Orchestrating Impartiality: The Impact of Blind Auditions on the Sex Composition of Orchestras." American Economic Review 90, no. 4 (2000).

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Figlio, David N. Tables 1-3. In "Names, Expectations and Black Children's Achievement." University of Florida and NBER (December 1, 2003).

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Farber, Henry, and Robert Gibbons. Tables 2 and 3. In "Learning and Wage Dynamics." Quarterly Journal of Economics 111, no. 4 (1996).

Altonji, Joseph, and Charles Pierret. Tables 1-3. In "Employer Learning and Statistical Discrimination." Quarterly Journal of Economics 116, no. 1 (2001).