Lecture Notes on
Labor Economics

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Abstract

These lecture notes were written for an M.A. level course in labor economics with focus on empirical identification strategies.

Contents

Syllabus

The course covers the topics of labor supply, demand, wage setting (discrimination, compensating differentials), education, and incentives, time allowing. A review of empirical identification strategies represents an integral part of the course. Prerequisites consist of basic-to-intermediate microeconomics and a course in econometrics. Grades will be based on student's performance in final exam (70%) and midterm exam (30%). Lecture notes and further readings are available at
`http://home.cerge-ei.cz/jurajda/teaching.html`

The main textbook for the course, denoted [CZ], is

- *Labor Economics* by Pierre Cahuc and André Zylberg, MIT Press 2004.

Other useful texts are

- Boeri, T., and J. van Ours *The Economics of Imperfect Labor Markets.*
- Borjas, G. *Labor Economics*,
- Ehrenberg, R., and R. Smith *Modern Labor Economics*,
- Wooldridge, J. *Introductory Econometrics*, and, of course,
- The *Handbook of Labor Economics* [HLE].

1. Introduction

In the first lecture, we start with the simple perfectly competitive model, where wages equal the marginal product of labor. We show that wages are determined by demand and supply factors and by elasticities in a simple two-skill-types labor market. Institutions can matter too. Next, we present a simple version of the Burdett-Mortensen 1998 IER model of labor market with search frictions, where luck matters for wages too and unemployment is not voluntary.

In the rest of the course, we build the labor market from supply and demand side up. We discuss both theoretical and empirical analysis for each covered topic. To gain some perspective on the empirical strategies used in different areas, we first briefly survey the research strategies used in empirical labor economics. Note that economic policy in the US, UK, and recently even in Germany (as part of the Hartz reform) is increasingly based on empirical evidence; *evidence based policies* have so far made less of a foray in post-soviet economies.

2. Empirical Analysis in Economics

In empirical economics, we estimate (quantify) causal *effects,* i.e., we want to know whether x affects y and how much.[1] Answering these questions in economics is hard since we are typically not allowed (by nature, laws, or finance) to run experiments on many important questions. Why is this a problem? There are often elusive unobservables affecting the outcome, such that even strong correlations may not reflect causality. Economic theory models some of the sources of simultaneity (endogeneity).

Example 2.1. *The issue of causation versus correlation is not unique to economics, of course. Dozens of useless and sometime dangerous health recommendations*[2] *were and are being based on observational studies (such as the Nurses' Health Study), which can only establish associations, not causation. See also evidence based medicine.*

[1] See Angrist and Krueger (1999) "Empirical Strategies in Labor Economics".

[2] Long-term use of estrogen (aspirin, vitamin C) was thought to lower chances of heart attacks or heart disease based on correlations from observational studies, until randomized trials refuted the idea.

Testing hypotheses requires a randomized-controlled trial—an experiment comparing the intervention (treatment) group with the control group (ideally using placebo, i.e., double-blind). In economics, we often get around the need for experiments and we establish causality (e.g., from education to health) using experiment-like (exogenous) events (variation).

Example 2.2. *Does Disability Insurance (DI) negatively affect labor force participation? Parsons (1980) suggests so (negative effect of replacement ratio = DI/wage). Bound (1989) says replacement ratio is a decreasing function of past earnings and past earnings reflect pre-existing labor force participation patterns. So Bound estimates the effect of replacement ratio on workers who never applied for DI and gets the same negative effect. Next, he also studies those who applied but were turned down. These people are presumably healthier than the recipients and they still did not work. So the effect is about being handicapped, not about collecting DI. What we need is exogenous variation in the replacement ration—such as coming from policy changes.*

2.1. Experimental Setup and Solution

Experimental sciences have it easy. Consider a study of the effect of a training program where workers are randomized into and out of treatment (training). Think of the effect of the program as corresponding to the difference between two hypothetical outcomes for each person: y_{1i} is earning with training, y_{0i} is earnings without training. We only observe one of the two potential outcomes: Eligible workers first choose to apply for the training program or not. We observe y_{1i} only when $D_i = 1$ (the person applied for and took training) and observe y_{0i} only when $D_i = 0$ (these are the so called eligible non-participants, ENPs). We want to know $E[y_{1i} - y_{0i}]$. We also want to know $E[y_{1i} - y_{0i}|D_i = 1]$, the average effect of treatment on treated, ATT. One may also be interested in the average effect on the untreated, ATU: $E[y_{1i} - y_{0i}|D_i = 0]$. However, the data only provides $E[y_{1i}|D_i = 1]$ and $E[y_{0i}|D_i = 1]$ is not observed—it is the counterfactual. We cannot simply compare the outcome for the treated and the non-treated as the unemployed who apply for retraining courses (or are chosen by the administration of such courses) may be the more able ones—those who would do well even in absence of retraining. This identification problem is solved by randomization: take the $D = 1$ group and randomize into *treatment* ($R = 1$) and *control* ($R = 0$) group. Then construct the experimental outcome: $E[y^*_{1i}|D^*_i = 1, R_i = 1] - E[y^*_{0i}|D^*_i = 1, R_i = 0]$.[3]

2.2. Simultaneous Equations Reminder

Simultaneous Equations are unique to social science. They occur when more than one equation links the same observed variables. Identification issues arise. Solution: IV to find variation in the x with simultaneity bias which is not related to the variation in the ϵs, i.e., use $\widehat{x}$ instead. Theory or intuition is often used to find an "exclusion restriction" postulating that a certain variable (a potential instrument) does not belong to the equation in question. Read introduction to Angrist and Krueger (2001) and the provided WSJ article.

Consider the *structural* demand and supply system

$$\begin{aligned} q_D &= \alpha_0 + \alpha_1 p + \alpha_2 y + \varepsilon_D \\ q_S &= \beta_0 + \beta_1 p + \qquad + \varepsilon_S \\ q_D &= q_S \end{aligned}$$

where S stands for supply, D stands for demand and p is price and y is income. Both algebra and a S x D graph suggest that $E[\epsilon_D p] \neq 0$.We can solve for and estimate the *reduced form*

$$\begin{aligned} p &= \pi_1 y + \upsilon_p \\ q &= \pi_2 y + \upsilon_q \end{aligned}$$

but we can't go back from two πs to 5 αs and βs. In our setup, one can identify β_1 by *instrumenting* for p using y which is excluded from the supply equation, but α_1 is very difficult to get at.

2.3. Causal or Descriptive Evidence

We usually run a least-squares regression of y on the causal x, controlling for several other variables. What do we mean by x affects y? To speak of causality in econometrics, we consider the identification problem in terms of structural versus reduced form equations.[4] Often, we focus on the effect of one *causal* variable

[3] This can be used as a benchmark for the accuracy of sample selection techniques that we need when we have no experiment, see Section 3.2.2.

[4] In Statistics, one of the key definitions of causality has to do with the timing (order) of events (see Granger causality).

(for which we have an exogenous source of variation) and use other regressors as *control* variables. The causal variable typically captures some treatment (policy, training program, education, etc.). If the treatment is not randomly assigned, we typically use some instrument to focus on experiment-like exogenous variation in the causal variable. The ultimate goal of econometrics is to provide policy evaluation. That is to answer "what if" questions (estimate the counterfactual).[5]

An alternative use of regression analysis is as a descriptive statistical tool (see, e.g., Deaton, 1997, p. 63). The regression function is simply the conditional expectation of one variable given another. There is no behavioral meaning to a conditional mean such as

$$E[y|x] = \int_{-\infty}^{\infty} y dF(y|x).$$

We can comfortably speak of x as being a *determinant* of y when we have a theoretical model suggesting x causes y and we assume the model is true and/or have credible exogenous variation in x. In what follows, we will focus on what it means to have credible exogenous variation in x.

In any case, you need variation in x to estimate a coefficient. Where does it come from? In an "ignorant" research design, you simply take a dataset and estimate a coefficient using whatever variation there is in the data. But parameter estimates are directly the outcome of the potentially many sources of variation in our data. Some of these may be endogenous, and at least some of these you should be able to understand and focus on in the estimation, e.g., using the IV method.

Example 2.3. *Card (1993) estimates returns to schooling, which may be affected by ability bias: It is not clear whether you have high wages because you got good education or whether your wages as well as your education are high because you were born with high IQ. He uses proximity to college as an instrument for education.*

Different sources of variation lead to different interpretation and often different estimates of the coefficients.[6] Below, we discuss some examples of where IVs comes from. But first, we say what one does in any case:

[5] What would have happened to car accidents had we not lowered max speed to 50 km/h? What would happen if we shorten criminal sentences? What would have happened if unemployed were not offered retraining courses?

[6] The wage-unemployment relationship across regions reflects compensating wage differentials, while the same relationship reflects equilibration within regions over time.

2.4. Control for X

2.4.1. Regression

Of course, before you start worrying about the sources of identification for your variable of interest, you should control for other variables that are correlated with your causing variable. If you fail to find all of these, you need an IV.

Example 2.4. *Returns to education, ability bias and IQ test scores.*

When is controlling for X enough to identify a causal effect? When is *selection on observables* plausible? When is assignment to treatment as good as random, conditional on X?

Example 2.5. *For example if applicants to a college are screened based on X, but conditional on passing the X test, they are accepted based on a first-come/first-serve basis.*

Example 2.6. *When applying for a green card, you may get extra 'points' for coming from a particular country or having high education, but conditional on these factors, the cards are assigned in a lottery.*

2.4.2. Matching

There are also other techniques of controlling for X. Consider estimating the effect of some treatment on outcomes (i.e. effect of training on re-employment chances). The idea is to compare outcome for individuals from the treatment and control[7] groups for each value of X. Then average the difference in the outcomes using the distribution of X for treatments to obtain the estimate of the overall treatment effect on those who got the training.

The difference from a regression approach is in the weights attached to the difference in outcome for each value of X. Often, the feasible way to implement this strategy is to condition on the unidimensional probability of treatment $P(X)$ rather than on the multi-dimensional set of covariates X.

[7] Controls did not get training, for example because they did not apply.

2.5. Exogenous Variation (IV)

You want to estimate β in $y = X\beta + \varepsilon$ but $E[\varepsilon|X] \neq 0$ because of endogeneity or measurement error. A valid instrument Z is correlated with X but not with ε. Where do you get such a variable? One solution is to find a "natural" experiment (more correctly quasi-experiment) that generates such variation.

Example 2.7. *Angrist (1990) studies the effects of military on earnings. He uses Vietnam-era draft lottery as an IV explaining whether or not a given individual served in the military.*

Example 2.8. *Changes in wage structure, which occur in a supply-demand framework: "Women, War and Wages" by Acemoglu, Autor and Lyle. First, establish that there is a treatment—variation in draft causes differences in female labor supply. Second, ask whether there is an effect—of female labor supply on wage dispersion.*

Example 2.9. *Card (1993) estimates returns to schooling, which may be affected by ability endogeneity bias, using proximity to college as an instrument for education and tests for exclusion of college proximity from the wage equation. To do this he assumes that college proximity times poverty status is a valid instrument and enters college proximity into the main wage equation. You may think of the distribution of student distance from college as providing a quasi experiment that the regression is using. Ideally, you want to drop students randomly from helicopter. Is this case close enough?*

What if the effect of x on y differs across groups of the population (parameter heterogeneity)? Whose effect are we estimating if we ignore this parameter heterogeneity? Recall that IV uses only part of the original variation in x — that predicted by the IV; hence, we are estimating the effect of x on y for the subpopulation whose behavior is well explained by the instrument (the compliers). In general, it can be shown that IV estimates are weighted averages of group-specific (or individual-specific) effects where higher weight is given to those groups whose x is better explained (predicted) by the instrument. So the IV estimate is the treatment effect on specific groups–it is a "local" effect—this is the **Local Average Treatment Effect** interpretation of IV estimates.

Example 2.10. *Angrist and Krueger (1991) use quarter of birth and compulsory schooling laws requiring children to enrol at age 6 and remain in school until* *their 16th birthday to estimate returns to education. First, they show that there is a relationship between quarter of birth and educational attainment (Figure 1) so that the estimated return is essentially a re-scaled difference in average earnings by quarter of birth. Note that this approach uses only a small part of the overall variation in schooling; in particular, the variation comes from those who are unlikely to have higher education. (The IV appears valid precisely because quarter of birth does not affect earnings and education of those with at least a college degree, because these people are not constrained by the compulsory schooling laws.)*

Example 2.11. *Similarly, one may think of the Angrist (1990) estimate of the effect of military service as corresponding to the effect of the service on those drafted using the Vietnam-era lottery, but not those (majority) soldiers who volunteered.*

Note, that this is a general problem of all estimation. The only difference is that IV selects a specific part of variation (we know who identifies the effect) whereas OLS can be thought of as weighted average of many sources of variation, some potentially endogenous. The IV approach captures the average effect of treatment on those who change status (of the endogenous variable) in response to a change in the instrument—the "compliers".

2.6. Group-Level Variation and Identification

Often variation of interest in x does not occur across individuals but across groups of individuals (firms, regions, occupations).

Remark 1. *When using individual-level data with group-level variation in the variable of interest, one needs to* correct standard errors *to admit the actual number of degrees of freedom (dimension of the variation of interest).*[8]

When comparing the outcome y across groups, one may be worried that there are differences in average level of unobservables across the groups. Consider studying the wage effects of union/non-union status or of sex segregation (concentration of women in occupations). What if unobservable productivity of union workers differs from that of non-unionized ones? What if unmeasured occupational characteristics (preferences) differ across highly 'male' and 'female' occupations?

[8] This is done by using the `cluster` option in Stata.

When we have panel data available (workers or firms over time), we can compare changes instead of comparing levels. How does the female fraction of workers in an occupation change in our data? Mainly for those who switch from one occupation to another.

Consider the effect of a union dummy (0/1 variable) in levels and in first differences:

$$y_{it} = UNION_{it}\beta + \alpha_i + \epsilon_{it}$$

$$y_{it} - y_{it-1} = (UNION_{it} - UNION_{it-1})\beta + \triangle\epsilon_{it} \tag{2.1}$$

and note that only those who switch status between t and $t-1$ are used in the estimation.

This strategy is thought of as being closer to causal evidence because it removes time constant unobservables. It relies on "movers" — but are they exogenous? Why do they move?

2.7. Difference in Differences

A simple research resign (identification strategy), referred to as "Differences" compares one group before and after the treatment (i.e., employment before and after minimum wage increase or some other sudden change in economic environment):

$$y_{it} = \alpha + \beta d_t + \varepsilon_{it}$$

where $d_{it} \in \{0, 1\}$ is the dummy for the treatment *group*. The crucial assumption is that without treatment, β would be 0 (no difference in means of y for treatment and control (before and after) groups). So estimate of beta is just mean of y after minus mean of y before. If there are changes in other conditioning variables, add $x'_{it}\gamma$.

However, there are often underlying trends and/or other possible determinants (not captured by x) affecting the outcome over time, making this identification strategy rather weak. Therefore, a very popular alternative is the "Difference in differences" design (DD), that is a before/after design with an untreated comparison group. Here, we observe a treatment ($j = 1$) and a comparison ($j = 0$) group for both the before ($t = 0$) and after ($t = 1$) time period:

$$\begin{aligned} y^j_{it} &= \alpha + \alpha_1 d_t + \alpha^j d^j + \beta d^j_t + \gamma' x^j_{it} + \varepsilon^j_{it} \\ \beta_{DD} &= \overline{y}^1_1 - \overline{y}^1_0 - (\overline{y}^0_1 - \overline{y}^0_0). \end{aligned}$$

The main threat to this method is the possibility of an interaction between group and time period (changes in state laws or macro conditions may not influence all groups in the same way). Note that we must enforce γ to be the same across j and that we consider x as control variable, while d_t^j is the causal variable.

Example 2.12. *Famous studies: Card and Krueger (1994) NJ-PA minimum wage study or Card (1990) Mariel Boatlift study. While in the NJ-PA study, the comparison group is obvious, in the immigration paper, Card must select cities that will approximate what would have happened to Miami were there no Boatlift (120 thousand Cubans arrived in Miami resulting in a 7% increase in local labor force in 4 months). These cities better have similar employment trends before the immigration influx. There is not much of an effect.*[9] *But note: each study is really only one observation.*

Remark 2. *In the impact-of-migration studies, one needs a large unexpected episode (a 'natural experiment') to identify the effect since otherwise natives' mobility and migrants' self-selection make identification difficult. Other examples are the sudden inflow of Russians into Israel in the 1990s or the rapid and concentrated inflow of Eastern Europeans into the UK labor market after 2003. (Using the diff-in-diffs method, Lemos and Portes (2008) imply that UK workers were little affected by the arrival of the 'Polish plumber'.)*

Example 2.13. *The DD method is fragile. Return to the Card's Mariel Boatlift paper. In 1994 there was a boatlift that did not happen, but the unemployment rate for blacks in Miami rose by almost 4 percentage points between 1993 and 1995 (significant). See Angrist and Krueger [HLE].*

The best situation for the DD method is when (a) the comparison group both before and after has a distribution of outcomes similar to that of the treatment group before treatment;[10] (b) $\widehat{\alpha_1}$ is not too large (otherwise there are frequent changes all the time).

The difference in differences (DD) design is the basis of panel-data estimation with fixed effects (see equation (2.1) where first differencing eliminated the fixed

[9] Recently, Bodvarsson et al. (2007) argue that lower wages due to greater supply of labor were offset in Miami by higher wages due to greater labor demand, because immigrants increased local consumption.

[10] This is important for non-linear transformations of the dependent variable (marginals differ based on the base).

effects). Using this technique, we study the effect of policy changes occurring in time as well across regions (states) of the US, Russia, etc. This is probably the most widely used approach in the last 20 years.

Example 2.14. *Consider again the union status effect on wages; see Section 2.6. Fixed effect estimation effectively takes all variables in deviation from means (first differences) corresponding to each set of fixed effects, i.e., it is using only movers.*

Example 2.15. *Gould and Paserman (2002) ask if women marry later when male wage inequality increases. They use variation across U.S. cities in male wage inequality and marriage behavior and allow for city-specific fixed effects and time trends to establish causality. To write a paper like this, start with graphs of levels and changes, then condition on other X variables, check if female wage inequality has any effect (it doesn't), and conclude. It is not clear where changes in male wage inequality come from, but one would presumably not expect these changes to be driven by a factor that would also affect marriage behavior.*

Example 2.16. *Gonzales and Viitanen (2007) use the variation in the timing of legislation legalizing divorce across European countries to identify the effect of exposure to divorce as a child; there is a significant long run effect.*

Example 2.17. *Ashenfelter and Greenstone "Using Mandated Speed Limits to Measure the Value of a Statistical Life" In 1987 states were allowed to raise speed limits on rural interstate highways above 55 m.p.h., 40 did (to 65 m.p.h.), 7 did not. You study the increase in speed (and time saved) and contrast this with the number of fatalities. Comparison groups are states that remained at 55 m.p.h. and other highways within states that went for 65 m.p.h. They estimate*

$$\ln(\textit{hours of travel})_{srt} = \beta \ln(\textit{miles of travel})_{srt} + \gamma \ln(\textit{fatalities})_{srt} + \alpha_{sr} + \eta_{rt} + \mu_{st} + \nu_{srt}$$

but there is endogeneity problem in that people adjust travel speed to reduce fatalities when the weather is bad etc. So they use a dummy for having the 65 m.p.h. speed limit as an IV. In the end they get \$1.5m per life.

Remark 3. *Note that we often used the state-time changes as IV, instead of putting the d_{it}^{j} dummies on the RHS.*

Example 2.18. *Cutler and Gruber (1995) estimate the crowding out effect of public insurance in a large sample of individuals. They specify a model*

$$Coverage_i = \beta_1 Elig_i + X_i\beta_2 + \varepsilon_i$$

As usual in U.S. research design, there is variation in state-time rules governing eligibility. Eligibility is potentially endogenous and also subject to measurement error. To instrument for $Elig_i$ *they select a* national *random sample and assign that sample to each state in each year to impute an average state level eligibility. This measure is not affected by state level demographic composition and serves as an IV since it is not correlated with individual demand for insurance or measurement error, but is correlated with individual eligibility.*

Remark 4. *Today, the preferred version of the DD method is based on matching the differenced units. See Section 2.4.2.*

3. Labor Supply

3.1. Decision to Work in Theory

We start with the static (one period) model of labor supply with leisure on the x axis and consumption on the y axis and ask about substitution, income and endowment effects. Maximize $U(C, L, X)$, a quasi-concave utility function ($U_C > 0, > U_{CC} < 0, U_L > 0, U_{LL} < 0$), where C is consumption, L is leisure (any time spend not working), and X are individual attributes, subject to: $C = Y + w(T - L)$ and $L \leq T$, where w is the wage rate, Y is non-labor income, and T is the total time available. A single consumption good is taken as the numeraire (the price of consumption is one). The 'price' of one unit of not working is the wage (opportunity cost).[11]

Denote by M the full income, i.e., $M = Y + wT$. The Lagrangian of the maximization problem is:

$$L = U(C, L, X) - \lambda(C + wL - M) - \mu(L - T) \tag{3.1}$$

and the FOCs. are:

[11] Note that it's not clear in the static model over what length of time is being examined. Are we looking over a week, or a year? Also, it's not clear how an individual decides to allocate their time: by hours or by days?

$$\begin{aligned} U_C(C,L,X) &= \lambda \\ U_L(C,L,X) &\geq \lambda w + \mu. \end{aligned}$$

Focusing on the interior solution (with $L < T$ such that $\mu = 0$) we obtain[12]

$$\frac{U_L}{U_C} = MRS_L(C,L,X) = w,$$

where MRS stands for the marginal rate of substitution. We can use the first order conditions to solve for consumption and leisure choices that maximize utility: $L^* \equiv L^*(w,M,X)$ and $C^* \equiv C^*(w,M,X)$ or we can look at work rather than leisure: $H^* = T - L^*$, where H is hours worked. The reservation wage (that makes an individual indifferent between working and not working) equals $MRS_L(Y,T,X)$.

Labor economists (as well as macroeconomists) are interested in the response of time worked to changes in wages. It is easier to go through the response of leisure (because leisure is 'good').

$$\frac{\partial L^*(w,M,X)}{\partial w} = \left.\frac{\partial L^*}{\partial w}\right|_{M=current\_income} + \frac{\partial L^*}{\partial M}\frac{\partial M}{\partial w}, \tag{3.2}$$

where $\frac{\partial M}{\partial w} = T$.

Holding individual attributes X constant, we can compute $\left.\frac{\partial L^*}{\partial w}\right|_{M=current\_income}$, the Marshallian demand for leisure, which is based on assuming that income does not change, from the Slutsky equation:

$$\left.\frac{\partial L^*}{\partial w}\right|_{M=current\_income} = \left.\frac{\partial L^*}{\partial w}\right|_{U=U^*} - \frac{\partial L^*(w, C+wL^*)}{\partial M}L^*, \tag{3.3}$$

where $\left.\frac{\partial L^*}{\partial w}\right|_{U=U^*}$ is the Hicksian demand for leisure.[13] Recall that the substitution effect is negative (if the price of leisure goes up, holding utility constant, people

[12] Recall that the Lagrange multiplier represents the increase in the optimization criterion (utility U) resulting from relaxing the relevant constraint by one unit. It measures how much the constraint "hurts" (it equals zero when the constraint is not binding), which is why it is often called the shadow price.

[13] Recall that the Hicksian demand function is obtained by minimizing consumer expenditures ($C + wL$) subject to achieving a given level of utility ($U(C,L) \geq \overline{U}$). Of course, when M equals the optimal expenditures given w and $\overline{U}$, the Marshallian and Hicksian demands are equal, which can be used to derive the Slutsky equation.

buy less leisure). We also usually assume leisure is a normal good such that we obtain the standard result that the Marshallian response (elasticity) to the wage change is larger (in absolute value) than the Hicksian response. Substituting the Slutsky equation into the full response gives:

$$\frac{\partial L^*}{\partial w} = \left.\frac{\partial L^*}{\partial w}\right|_{U=U^*} + \frac{\partial L^*(w, C + wL^*)}{\partial M}(T - L^*). \tag{3.4}$$

The response depends on the substitution effect, the income effect and the endowment effect. The endowment effect increases consumption of leisure since a rise in the wage rate makes them richer overall (the income effect is smaller than the endowment effect).

Note the difference between this result and that from conventional demand theory, which only includes income and substitution effects (which are both negative if the good with a price increase is a normal good). The conventional case concerns goods that are consumed, not sold – there are no endowments. Here, an individual not only consumes leisure, but also may 'sell' it for a wage, in order to obtain other consumable goods. The main point of the static model is to show that the response from a wage change is ambiguous – there is both a positive and a negative effect, and it is not clear which one dominates, and under what circumstances. If the price of leisure increases, people should work more (consume less leisure), but they also get richer, which makes leisure more attractive. Of course, the response is not ambiguous for an individual who is currently not working. An increase in the wage will lead to a zero or positive increase in hours worked. Individuals that work for only few hours a week are likely to have the substitution effect dominate because the less an individual works, the smaller the combination of the conventional income and endowment effects (the smaller $T - L^*$).

This is useful to know in practice, because many empirical studies suggest that movements in labor supply are principally owing to variations in the participation rate, and that the elasticity of the supply of female labor, especially that of married women, is greater than that of men. For prime age men, most empirical evidence indicates that the income effect dominates (men tend to work less when their wages increase). For women and others with currently lower labor force participation, the response to an increase in wage tends to generate more of a response towards working more hours.[14]

[14] Furthermore, research suggests that the wage elasticity of labor supply decreases with labor force participation, both in time and across countries (Blau and Kahn, 2007; Alesina and Ichino, 2007). As more and more women participate in the labor force, fewever are probably on the

How are the empirical results estimated? The general empirical equation for estimating the overall Marshallian labor supply elasticity (corresponding to the static one-period model, using cross-sectional data) is:

$$\ln H_i = \beta_0 + \beta_1 \ln w_i + \beta_2 X_i + v_i, \tag{3.5}$$

where β_1 is the elasticity of hours worked with respect to wages. Clearly, there may be many factors that affect both wages and hours worked such that estimating Equation (3.5) must deal with this endogeneity problem (omitted variables problem). Further, it is not clear how to deal with those with $H_i = 0$. We will look at these technical issues in the next section.

The simple static model allows for differences in responses to wage changes to be driven purely by differences in tastes (because the indifference curves take on different shapes). It assumes that wages are parameters, which don't change when other characteristics change, but clearly this is not the case. To make the model realistic, one needs to incorporate into the budget equation the effects of taxes ($wH(1-\tau)$), transfers (get welfare if $H \leq 0$), and fixed costs of working (one time costs associated with going to work, $H > 0$, i.e., transportation costs, eating out, day care costs, etc.).[15] Since the budget constraint is no longer continuous, it's a pain to derive the first order conditions. It's a lot easier to consider how these changes affect the budget constraint graphically.

Example 3.1. *Of course, one can use a natural experiment to avoid the more 'structural' estimation of Equation (3.5) that we review in the next section. For example, Eissa and Liebman (1996) follow the participation rates of single women with children who became entitled to earned income tax credits (EITC). A natural control group for a DD research design are single women without children since their labor demand presumably moves in the same way as the labor demand for single women with children.*

In theory, the response to a transfer or tax depends both on how the revenue is raised and how taxes are used. Even if we focus on situations where revenue raised is used to make transfer payments to other individuals, the effect of a tax on individuals is still ambiguous because of the opposing direction of substitution

margin between participating and not; therefore, fewer can be "lured" into the labor force by wage increases.

[15] Other extensions include overtime work or rationing. In the next step, introduce home production, transfers within families, and allocation of (non-)market time.

and income/endowment effects (similar to the effect of changing wages). The bottom line is that the aggregate response is a priori unknown—it's an empirical question.

Exercise 3.1. *Of course, introducing an unconditional guaranteed welfare income (G) unambiguously reduces the incentive to work. A less extreme example is a negative income tax program (NIT): introduce income taxes so everybody gets only $(1-\tau)wH$ and introduce a subsidy S for those who work ($H > 0$), such that $S = G - \tau wH$ if $G > \tau wH$, and $S = 0$ otherwise. Non wage income is left out. (In practice, Y is hard to assess.) $wH = G/\tau$ is the break-even income level when the income supplement stops. Describe the effect of the NIT program on hours worked graphically and also sign the effect using the Slutsky equation. Consider those working as well as those not working before the program is introduced. Is the effect ambiguous?*

3.1.1. Life Cycle Labor Supply

The static (one-period) labor supply model is not a good guide to understanding intertemporal decisions (reaction to shocks: trading work today if wages are unusually high for leisure tomorrow)[16] or retirement decisions. To think of utility over time, we typically assume that it is temporally separable, which allows us to write it as $\sum_{t=1}^{T} U(C_t, L_t, t)$.[17] The budget constraint now must allow for savings; it follows the evolution of assets over time. The model suggests the use panel data to run the following equation:

$$\triangle \ln H_{it} = \beta_0 + \triangle\beta_1 \ln w_{it} + \triangle\beta_2 X_{it} + \triangle v_{it}, \qquad (3.6)$$

where $\triangle$ stands for the first (time) difference, which eliminates the individual fixed effect.[18] Here, β_1 is the elasticity of labor supply with respect to a transitory change in w. Estimating the effects of permanent shifts in wage profiles is more complicated.

Remark 5. *Recent research of labor supply (and permanent-income hypothesis) asks about how difficult it is for workers to understand policy changes in tax*

[16] Another related issue is investment into training (we return to this point in Section 4).

[17] This rules out, e.g., habit persistence. It implies that $U_L/U_C = w_t$ in every period.

[18] The theory suggests that λ_t can be broken down to an individual fixed effect and a common age effect, which comes from interest rates over time. If the interest rate is constant then β_0 is the result of first differencing this age effect $\beta_0 t$.

schedules. (Is there a lump sum change or a change in marginal tax rates? Do people correctly understand their marginal tax rates or do they incorrectly respond to average tax rates using some form of "schmeduling"? Etc.).

3.2. Empirical Analysis of Labor Supply

How do we take the labor supply or human capital models to data? In this section we will (i) introduce the standard parametric sample selection technique, for which James Heckman got the Nobel price, and (ii) use the example of labor supply estimation to highlight the need for exogenous variation—IV.

Consider estimating a labor supply equation (regression) for women: $\ln H_i = h_i = \beta' x_i + \delta w_i + u_i$. Or consider the effect of education on wages for women: $\ln w_i = \beta' x_i + \gamma S_i + \epsilon_i$. The problem with estimating these equations is that many women do not work (corner solution, not a tangency solution). You can only measure the impact of S on w for those women who work—that is select themselves into the sample of working women. Whether or not a woman works, depends on the wage she could get when working. There could be unobservables affecting both the wage and the labor supply equation. How can we work with data where many women have zero hours and wages?

3.2.1. Tobit

OLS is inconsistent no matter whether we include or exclude the zero observations. So, we need to build an econometric model. To start, assume a model for observed hours worked h using the concept of an underlying unobservable *latent* variable h_i^* (desired hours):

$$\begin{aligned} h_i^* &= \beta' x_i + \delta w_i + u_i \text{ with } u_i \sim N(\mu, \sigma^2) \\ h_i &= h_i^* \text{ iff } h_i^* > 0 \\ h_i &= c \text{ iff } h_i^* \leq 0, \end{aligned}$$

and use Tobit MLE based on the normality assumption:

$$L = \prod_{h_i^* > 0} \frac{1}{\sigma} \varphi \left(\frac{h_i - x_i' \beta - \delta w_i}{\sigma} \right) \prod_{h_i^* \leq 0} \Phi \left(\frac{0 - x_i' \beta - 0}{\sigma} \right).$$

Where does the participation decision come from? An early modelling approach of Heckman (1974) proposes two behavioral functions: the market wage function w_i and the reservation wage function w_i^R. If $w_i > w_i^R$, a women i decides to work (participate) and her hours h_i adjust so that her marginal value of time equals w. Of course w_i^R is not observable and we see w_i only for women who work. Heckman (1974) builds an estimable model by assuming (a) equations for h and w with a bivariate normal distribution of error terms u and v, and (b) no fixed costs of entering the labor force so that one can obtain w_i^R from $h = 0$ condition. Participation decision: do *not* work if

$$w_i < w_i^R \Leftrightarrow \theta' z_i + v_i < -\frac{\beta' x_i}{\delta} - \frac{u_i}{\delta} \Leftrightarrow -\frac{u_i}{\delta} - v_i > \theta' z_i + \frac{\beta' x_i}{\delta}.$$

Now, we are ready to estimate the model using MLE:

$$L = \prod_{\text{work}} |\mathrm{J}| \, f(\underbrace{h_i - \beta' x_i - \delta w_i}_{u_i}, \underbrace{w_i - \theta' z_i}_{v_i}) \prod_{\text{no work}} \Pr\left(-\frac{u_i}{\delta} - v_i > \theta' z_i + \frac{\beta' x_i}{\delta}\right)$$

Remark 6. *This model is very restrictive in asking the structural model to deliver w^R from the $h = 0$ assumption. As a result, Tobit leads to significant overestimation of some elasticities. In other words, the hours of work decision made when the woman is in the labor force appears distinct from her labor market participation decision.*

Remark 7. *Also notice the complete reliance on (and the sensitivity to departures from) the Normality assumption.*

Remark 8. *The basic Tobit model is restrictive in constraining the coefficients and the xs affecting the extensive and intensive margins to be the same. We can relax the Tobit likelihood and split it into two (independent) parts: (i) 0/1 probit for whether the woman works, and (ii) a truncated normal regression of the wage when working. Further, we can allow different explanatory variables to enter each of the separate two likelihoods. But the disturbances from the two separate equations are likely dependent, which is why we need a sample selection model:*

3.2.2. Sample Selection: Heckman's λ

There is a better way of dealing with the sample selection problem. Heckman (1979) considers a two-equation behavioral model:

$$\begin{aligned} y_{i1} &= x_{i1}' \beta_1 + u_{i1} \\ y_{i2} &= x_{i2}' \beta_2 + u_{i2}, \end{aligned}$$

where wages y_{i1} are observed only for women who work (have utility from working $y_{i2} > 0$).

Note that the expectation of data on y_{i1} you observe depends on the selection rule which determines that y_{i1} is observable:

$$\begin{aligned} E[y_{i1}|x_i, y_{i2} &> 0] = x_{i1}^{'}\beta_1 + E[u_{i1}|selection\ rule] = \\ &= x_{i1}^{'}\beta_1 + E[u_{i1}|y_{i2} > 0] = x_{i1}^{'}\beta_1 + E[u_{i1}|u_{i2} > -x_{i2}^{'}\beta_2]. \end{aligned} \tag{3.7}$$

We have an omitted variable problem: x_{i2} enters the y_{i1} equation. Are u_{i1} and u_{i2} independent?

If we assume that u_{i1} and u_{i2} are jointly normal with correlation σ_{12} and variances σ_1^2 and σ_2^2 respectively, we can derive a formula for $E[u_{i1}|u_{i2} > -x_{i2}^{'}\beta_2]$, which we will call Heckman's lambda:

$$E[y_{i1}|x_i, y_{i2} > 0] = x_{i1}^{'}\beta_1 + \frac{\sigma_{12}}{\sigma_2}\frac{\phi(x_{i2}^{'}\beta_2/\sigma_2)}{\Phi(x_{i2}^{'}\beta_2/\sigma_2)} = x_{i1}^{'}\beta_1 + \sigma\lambda(x_{i2}^{'}\beta_2). \tag{3.8}$$

One way to use this approach for the estimation of returns to education is to first run probit on labor force participation and obtain $\widehat{\lambda}$, then run the wage regression to get the effect of education on wages $\widehat{\beta}$ (and $\widehat{\sigma}$).

Remark 9. *While we can numerically identify σ_λ from β_1 even when $x_{i2} = x_{i1}$ because λ is a non-linear function, there is need for exclusion restrictions (IVs, variables in x_{i2} not included in x_{i1}) in order to avoid identification by functional form (i.e. by distributional assumption implying nonlinearity in xs).*

Remark 10. *Recent work is relaxing the assumption of joint normality of disturbances.*

Now, let us return to Heckman (1974) and rework this problem using the sample selection technique as in Heckman (1979). Consider 3 equations: w_i, w_i^R, and h_i (not 2 equations because here w_i^R does not come from $h = 0$). Moreover, here, w is taken as an endogenous variable:

$$\begin{aligned} h_i &= \delta w_i + x_i^{'}\beta + \sigma\lambda(z_i^{'}\widehat{\gamma}) + u_i \\ w_i^R &= x_i^{'}\phi + e_i \\ w_i &= \theta' z_i + v_i. \end{aligned}$$

We need to correct the hours equation for sample selection into labor force (only observe h for those who work). This correction comes from a comparison of behavior equations governing reservation wages w_i^R and market wages w_i which leads to a 0/1 participation estimation, as before, depending on $r_i^{'}$, which is the collection of RHS variables from both w_i^R and w_i equations. To see this, consider the participation decision: do not participate if:

$$\theta' z_i + v_i - x_i^{'}\phi - e_i < 0 \Leftrightarrow \underbrace{\theta' z_i - x_i^{'}\phi}_{r_i^{'}\gamma} + \underbrace{v_i - e_i}_{\varepsilon_i} < 0$$

Running a Probit on this delivers $\widehat{\gamma}$.

Second, you need to instrument for w_i which is likely endogenous. The first stage regression where you predict $\widehat{w}_i$ also needs to have a selection correction in it:

$$\widehat{w}_i = \theta' z_i + \sigma^* \lambda^*(\cdot)$$

Finally, you can estimate

$$h_i = \delta \widehat{w}_i + x_i^{'}\beta + \sigma\lambda(z_i^{'}\widehat{\gamma}) + \varepsilon_i.$$

Remark 11. *There is serious need for exclusion restrictions: you need an exclusion restriction for running IV for w_i (that is a variable predicting wages but not hours) and you need another exclusion restriction to identify the selection correction in the first-stage wage equation (a variable affecting participation, but not wages).*

Remark 12. *If the unobservable selection threshold is time constant we can use a fixed effect panel data model to deal with it. See Sections 2.6.*

3.2.3. Other Approaches

Difference in Differences Researchers sometimes avoid the estimation of more structural economic models of labor supply by using natural experiments. Eissa and Liebman (1996) follow the participation rates of single women with children who became entitled to earned income tax credits (EITC). A family is eligible for EITC is a) earned income is below a particular amount (about $28,000 in 1996) and b) there is a child in the family (under 19). In 1987, the subsidy rate (and maximum) was increased from 11% to 14% (from $550 to $851). A natural control group for a DD research design are single women without children since their labor demand presumably moves in the same way as the labor demand for single women with children.

Table 1 shows that the characteristics of the two groups are different so Eissa and Liebman focus only on single women with less than high school. The main result (in Table 2) is that labor force participation rises by 4 percentage points after the reform. However, Table 5 shows no response in annual hours. Of course, the analysis says nothing about the overall costs of the program (to the taxpayer).

Brewer et al. (2006) suggest that a similar UK program (WFTC) increases the lone-parent employment rate by 10 p.p. copmare to "no program" scenario (see IZA Policy Paper No. 3 for further references on this literature at www.iza.org).

Ashenfelter et al. (2010, NBER WP No. 15746) ask about the long-run elasticity of male labor supply using a simple, natural experiment, in which men can change their hours of work, and in which wages have been exogenously and permanently changed: the case of New York City taxi drivers who faced two exogenous permanent fare increases. Their estimates suggest a relatively small elasticity of about -0.2, implying that income effects dominate substitution effects in the long run.

Income Elasticity, not Hours Research on the effects of labor taxation has typically focused on the impact on hours of work and labor force participation. More recently, however, a literature has emerged that focuses on the impact on taxable income and other measures of income. One reason for this new direction is that taxes may affect individual behavior along a number of margins in addition to the effects on hours of work and participation, such as work effort and job mobility. This literature estimates the elasticity of taxable income with respect to (1-marginal tax rate). It typically adopts IV strategies in "differenced" specifications, where changes in income are regressed on changes in tax rates, which are IV-ed. See, Holmlund and Soderstrom (2007, IZA DP no. 3088) for a recent study.

Household Production and Leisure An interesting line of research (on household production and other issues) uses time-diary data. Burda, Hamermesh and Weil (2007, IZA DP no. 2705) use data from 25 countries to show that there is a negative relationship between real GDP per capita and the female-male difference in total work time per day – the sum of work for pay and work at home. In rich northern countries on four continents, including the United States, there is no difference – men and women do the same amount of total work. This is not the consequence of differences in market wages (price of time), as women's total work is further below men's where their relative wages are lower, and it is not associated with marital status. To explain the facts the authors offer a theory of social norms, which is consistent with evidence based on the World Values Surveys.

Another recent paper using (US) time use survey data is Connolly (JOLE, 2008). She shows that on rainy days (when enjoyment of leisure is lower), men shift on average 30 minutes from leisure to work. This gives the intertemporal elasticity of labor supply of around 0.01, in line with the rest of the literature.

Lately, experimental data is being used to supplement survey-data-based exercises. Fehr and Goette (AER, 2007) argue that the reason why most previous studies on intertemporal labor supply found very small substitution effects is that there are constraints on workers' labor supply choices. They therefore conduct a randomized experiment where workers are free to choose hours worked and find a large positive elasticity.

A recent summary of the empirical literature on labor supply and taxes is provided by Meghir and Phillips (2008, IZA DP. no 3405). They argue that a structural approach is desirable and discuss the "new Tax Responsiveness" literature which uses the response of taxable income to the marginal tax rate as a summary statistic of the behavioural response to taxation. Underlying this approach is the unsatisfactory nature of using hours as a proxy for labour effort for those with high levels of autonomy on the job and who already work long hours, such as the self employed or senior executives. The conclusion is that hours of work are relatively inelastic for men, but are a little more responsive for married women and lone mothers. On the other hand, participation is quite sensitive to taxation and benefits for women.

Ex Ante Evaluation of Policies In practice, governments often change taxes and benefits systems in absence of credible ex-post micro-data based evaluations. At the very least, the government should have an idea of how the *interaction* of benefits and taxes affects workers' work incentives. If I stop working, the combination of benefits and taxes implies some net replacement ratio (NRR). Once I start working for a low wage and consider increasing my wage by exerting more effort, I am taxed not only due to a progressive income tax schedule, but also because with higher wage, I lose some of the (family) social support benefits I used to get. This implies some Marginal Effective Tax Rate (METR), which differs across different wage levels.

4. Human Capital

This section is so far presented only in slides. Some suggested reading follows.

Mincerian returns: Card (2000), Heckman and Krueger (2003). School quality: Card and Krueger (1992a), Heckman et al. (1996), Angrist and Lavy (1999). Wage growth and labor mobility: Topel and Ward (1992). Models of life cycle human capital accumulation, learning, matching, and sorting: Neal and Rosen (2000). Relative supply of labor and wage inequality: Card and Lemieux (2001). Recent increases in inequality at the top of the income distribution:[19]. Other-than-wage returns to education: Oreopoulos and Salvanes (2009, NBER WP No. 15339): schooling makes one enjoy work, make better decisions about health,[20] marriage (delay it[21]), and parenting. It also improves patience, trust and social interaction.

Hanushek and Woessmann (NBER WP No. 14633): home-country cognitive-skill levels strongly affect the earnings of immigrants on the U.S. labor market in a difference-in-differences model that compares home-educated to U.S.-educated immigrants from the same country of origin. Countries that improved their cognitive skills over time experienced relative increases in their GDP growth paths. From a policy perspective, the shares of basic literates and high performers have independent significant effects on growth that are complementary to each other, and the high-performer effect is larger in poorer countries.

5. Job Search

So far we did not discuss how leisure is spent and how jobs are found by workers (remember the matching of workers with jobs in the first class?). Now, we briefly discuss how people find and accept job offers:

Job search theory assumes that unemployed individuals know the wage (offer) distribution they are facing. They exert effort (incur search costs) to get to see a wage offer (which arrives with some probability). In this (dynamic programming, Bellman equation) setup, one can show that the optimal strategy for a worker is to set a reservation wage, which can depend on how long s/he has been searching

[19] http://faculty.chicagogsb.edu/joshua.rauh/research/reality_economy_24Jul07_NYT.pdf

[20] Although see, e.g., NBER Working Paper No. w16013, for some caution on this argument.

[21] Powdthavee and Adireksombat (2010, IZA DP No. 5019) use a nationwide change in the compulsory schooling law in the UK in 1947 to estimate that completing an extra year of schooling increases the average age at first marriage by approximately 3 years for men and almost 2 years for women.

(credit constraints) as well as on all of the parameters of the job search environment (probability of job destruction, probability of offer arrival, etc. In each period, the unemployed compare the value of continuing search with the value (function) of accepting a wage offer.

In the simplest stationary setup with one wage draw (offer) per period, no labor supply, work forever once offer is accepted, no search intensity, and no risk aversion, one can write the value of searching for one more period as

$$V_t(w_t) = b + \beta \max \left\{ \frac{w_t}{1-\beta}, E[V_{t+1}] \right\}$$

such that the optimal stopping rule (reservation wage) is $\frac{w^*}{1-\beta} = E[V_{t+1}]$. Now,

$$E[V_t] = b + \beta \Pr[w_t > w^*] \frac{E[w_t | w_t > w^*]}{1-\beta} + \beta \Pr[w_t < w^*] E[V_{t+1}].$$

Stationarity suggests that $E[V_t] = E[V_{t+1}] = V$ such that

$$V = \frac{w^*}{1-\beta} = b + \beta[1 - F(w^*)] \frac{E[w_t | w_t > w^*]}{1-\beta} + \beta F(w^*) \frac{w^*}{1-\beta},$$

which gives w^*. At the end of the day, the theory generates a hazard rate (probability of accepting an offer and leaving unemployment), which depends on the parameters of the search process, including the key policy parameters: Unemployment Insurance (UI), i.e., the level of benefits and the duration of benefits, aka entitlement length.

One would like to quantify the UI effects on duration of unemployment (search effort, choosiness) as well as on accepted wages. The empirical literature suggests that there is some UI disincentive effect (esp. of entitlement length) on unemployment duration.[22] Effects on accepted wages (which can be positive) need to be explored more.

Equilibrium job search models.

6. Labor Demand

And Minimum Wages, Monopsony. See the slides.

[22] There are numerous technical estimation issues, some related to identification, other related to estimation of duration (hazard) models, where unobservable heterogeneity (even that uncorrelated with explanatory variables) leads to inconsistency.

7. Compensating Wage Differentials

In theory, equalizing wage differentials (or the return to risk, the risk premium) can be estimated using a hedonic wage regression $\ln(w_{ij}) = \beta' X_i + \gamma X_j + \delta risk_j + \epsilon_i$, where X_i are the observable workers' characteristics, X_j are the observable job (occupation) characteristics (other than injury hazard), and $risk_j$ is a measure of the occupational safety risk; δ captures equalizing wage differential. The risk measure can be self-reported (measured with error, but reflecting individual risk preferences) or ex-post industry- or occupation-wide.

Unfortunately for this strategy, unobservable worker characteristics (preferences) influence both their wages and the riskiness of the jobs they choose (and the return to risk that they face). Measures of compensating wage differentials therefore suffer from an endogeneity problem and are biased. To deal with unobservable heterogeneity, researchers have used fixed-effect models (occupation switchers) or proxies for the unobservable risk attitudes (smoking). Most studies worry only about the downward bias in compensating wage differentials resulting from omitting some workers characteristics, but the estimates can also be upward biased because workers who self-select into risky occupations might be more tolerant towards risk, more skilled in coping with risk or both (Shogren and Stamland, 2002). If I am the most risk-loving person in the labor market, I enjoy substantial rent because the risk premium is determined by the risk-dealing ability of the marginal worker (the worker in the risky occupation with the lowest ability to deal with risk among those choosing this occupation). In other words, I would be willing to work in a risky occupation for much lower (even negative) risk premium.

Example 7.1. *Viscusi and Hersch (2001) ask whether heterogeneity among workers in their attitude towards risk affects the market opportunities they face. They use smoking intensity as a proxy for the attitude towards risk and find that smokers choose riskier jobs, receive less compensation for the riskiness of their jobs, and get lower wages in zero-risk jobs. This is consistent with smokers facing a flatter offer curve that lies entirely below the offer curve for nonsmokers. Smoker also apparently attach a significantly smaller value to a statistical job injury than nonsmokers do.*

Example 7.2. *Bell et al. (2002) relate wages and unemployment in several data dimensions: (i) aggregate time series, (ii) cross-sectional compensating wage differentials as a no-moving equilibrium, (iii) regional equilibration (changes in both variables in a regression conditional on time and regional fixed effects).*

Remark 13. *Measures of compensating wage differentials can also be used to estimate the* value of a statistical life *(VSL), which is often used to evaluate different governmental programs and policies aimed at improving the citizens, workers, drivers, etc. safety.*

8. Discrimination

8.1. Theory

Becker's 1957 book The Economics of Discrimination started an enormous literature.[23] Discrimination is when members of a minority are treated differently (less favorably) than members of a majority group with identical productive characteristics (X), i.e., when $\gamma < 0$ in the following equation:

$$\ln w_i = \alpha + \beta' x_i + \gamma M_i + \epsilon_i, \tag{8.1}$$

where M is an indicator of minority status.

Remark 14. *There are several caveats to this definition of discrimination:*

- *What if productivity does depend on M? What if customers will pay more to see a white actress or a black athlete?*[24]
- *Is the production technology β truly exogenous? (Could fire-fighting equipment be adjusted to allow women to become fire-fighters? Presumably fire-fighting equipment used in Japan demands a smaller physical stature.)*
- *What if there is pre-market discrimination or there are expectations of future discrimination, which would both tend to reduce x for members of the minority group. (Examples: poor schools, or a rational belief among minorities that education will not be rewarded by the market.)*

[23] This section draws heavily from David Autor's lecture notes.

[24] These things do happen. See IZA DP no. 3987 by Lawrence Kahn, who surveys work on discrimination in basketball. In the 1980s, there was much evidence of discrimination against black NBA players, but it went away in the 1990s. This is consistent with evidence on changes in fan preference for white players.

8.1.1. Taste-Based Discrimination

In Becker (1957), employers have a 'taste for discrimination,' meaning that there is a disamenity value to employing minority workers. (Discrimination comes directly out of the utility function.) In this case, minority workers may have to 'compensate' employers by being more productive at a given wage or, equivalently, by accepting a lower wage for identical productivity.

Let a denote majority group membership and b denote minority group membership. Employers will maximize a utility function that is the sum of profits plus the monetary value of utility from employing members of particular groups. Let d be the taste parameter of the firm, which Becker called the "coefficient of discrimination." So, firms will maximize

$$U = pF(N_b + N_a) - w_a N_a - w_b N_b - dN_b.$$

Employers who are prejudiced ($d > 0$) will only hire b group members if $w_a - w_b \geq d$. The optimal number of workers hired at each firm is determined by the solutions to $pF^{'}(N_a) = w_a$ and $pF^{'}(N_b) = w_b + d$. Now, let $G(d)$ denote the CDF of the prejudice parameter d in the population of employers and aggregate across firms in the economy to obtain the market demand functions for each type of worker. Market clears at wages that equate demand with supply for each type.

Remark 15. *The main point of this setup is that a wage differential $w_b < w_a$ will arise if and only if the fraction of discriminating employers (or discriminating jobs) is sufficiently large that the demand for b workers when $w_b = w_a$ is less than the supply. In other words,* discrimination on average does not mean discrimination at the margin. *If there are enough non-discriminating employers, then discrimination is not 'visible'. (In this case, minority workers don't work for discriminating employers.)*[25] *On the other hand, if the share of prejudiced employers is sufficiently large, then some b group members will work at $d > 0$ employers, and this implies that $w_b < w_a$. In this case, the strength of prejudice* at the margin *(that is d for the marginal employer of b workers) is what determines the size of the wage gap.*

Remark 16. *Also note that with free entry (or constant returns to scale), discriminating employers may be competed out of business. In a competitive market,*

[25] The presence of discriminatory tastes create incentives for segregation. It is potentially Pareto improving for minority workers to work in their own businesses and similarly for majority workers — then no one bears the cost of the distaste.

each worker must earn his marginal product. In a competitive equilibrium (with free entry, in the long run), discriminating employers must fund the cost of their distaste out of their own pockets; they cannot pass the cost onto the minority worker.[26] *In the short run (partial equilibrium), minority workers must 'compensate' employers by accepting a lower wage for equivalent productivity.*

8.1.2. Statistical Discrimination

Since Phelps (1972) and Arrow (1973), most of economic research (starting with Aigner and Cain, 1977) has focused on the statistical theory of discrimination rather than taste-based discrimination. The premise of the statistical discrimination literature is that firms have limited information about the skills of job applicants (but hold no animus against racial groups). This gives them an incentive to use easily observable characteristics such as race or gender to infer the expected productivity of applicants (if these characteristics are correlated with productivity). Statistical discrimination is the solution to a signal extraction problem.

If an employer observes a noisy signal $\widetilde{\eta}^i$ of applicant's i true productivity η^i and also has prior information about correlates of productivity (let's say a group-specific mean of productivity $\overline{\eta}_a$ and $\overline{\eta}_b$), then the expectation of applicant productivity should place weight on both the signal and the mean (in fact, both are 'signals').

We will only look at one case in detail: when the two groups have different means but identical variances. So, assume that $\eta^i_a \sim N(\overline{\eta}_a, \sigma^2_\eta)$ and $\eta^i_b \sim N(\overline{\eta}_b, \sigma^2_\eta)$ and that $\overline{\eta}_a > \overline{\eta}_b$. When workers apply for jobs, the employer observes the group type of the applicant $x \in \{a, b\}$ and some error-ridden signal of the applicant's productivity $\widetilde{\eta}^i_x = \overline{\eta}_x + \epsilon_i + \nu_i$, where ν is the noise around the true η^i_x (noise is Normally distributed with mean zero and σ^2_ν variance) and where $\epsilon_i = \eta^i_x - \overline{\eta}_x$. Now, the employers are doing their best to distil information from the two signals: x and $\widetilde{\eta}^i_x$. In particular, they form $E[\eta|x, \widetilde{\eta}] = \overline{\eta}_x(1-\gamma) + \widetilde{\eta}\gamma$, where $\gamma = \sigma^2_\eta/(\sigma^2_\eta + \sigma^2_\nu)$. This immediately implies that the expected productivity of b applicants is below that for a applicants, even though $\widetilde{\eta}$ is an unbiased signal of true productivity for each applicant.

Remark 17. *In this model, there is equal pay for equal* expected *productivity, but not equal pay for equal work, because 'work' is not fully observable. For some*

[26] But note that if discrimination starts with customer preferences, than it will not be competed away.

workers, there is discrimination in terms of equation (8.1), but within each group, expected productivity equals true average productivity.

Remark 18. *Of course, the less noise there is in the productivity signal for a particular group, the lower the importance of the group average productivity for employers. Similarly, if employers are risk averse, they will hate to hire from high variance groups. If the signal* $\widetilde{\eta}$ *corresponds to education and non-whites' education 'counts' less, they will not invest in schooling as much.*

Remark 19. *Unlike taste-based discrimination, statistical discrimination is not competed away in equilibrium.*

Remark 20. *A related point is that statistical discrimination is 'efficient.' That is, because statistical discrimination is the optimal solution to an information extraction problem, economists might generally say that employers 'should' statistically discriminate. It is profit-maximizing, it is not motivated by animus, and it is arguably 'fair' since it treats people with the same expected productivity identically (though not necessarily with the same actual productivity). Hence, many economists might endorse statistical discrimination as a reasonable public policy.*

Remark 21. *But statistical discrimination is illegal (in the US, EU). Why? Even though two groups have different mean productivity, when I compare two individuals from these two groups, the individual from the group with the lower mean can easily have higher productivity. But so what? Why should employers ignore useful signals and select workers in an unnecessarily noisy (random) way? You should not be punished for the fact that others in your group are not productive because if we allow your fate to be determined not just by what you do but by what people like you do, this can lead to self-fulfilling expectations that are discriminatory in nature.*

Remark 22. *The self-fulfilling nature of statistical discriminatino can be produced in a lab. Fryer et al. (2005) ran an experiment matching workers with employers, where workers are (randomly assigned to) either 'purple' or 'green' groups and decide whether to invest in education, which increases a worker's value for employers and also increases the chances of the worker passing a test. (There is one wage rate for employed workers.) Employers make their hiring decisions based on the result of the test and on the 'colour' of the worker. In the* *first round, by chance, a larger fraction of the 'green' group decides to gamble on getting an education. In a few more rounds, employers won't hire 'purple' workers because they had not invested and 'purple' workers won't invest because they're not being hired.*

Remark 23. *But consider the case of racial profiling. For example, consider screening at airports or consider anti-drug police stopping cars on the highway based on the race of the driver. The police (or airport safety) have only limited resources and they know that average criminality (terrorist threat, drug running) is higher among certain groups. The police will stop cars driven by group a or group b drivers such that they will equate the* marginal *return to stopping a driver from each of the two groups (i.e., the marginal car stopped has the same expected probability of criminality for both groups). Of course, this will mean that innocent group b drivers will be stopped (checked) a lot more often. There are more Type I errors for group b. In other words, statistical discrimination is inequitable on average, even if it is 'fair' at the margin.*

Remark 24. *Of course, statistical discrimination is 'efficient' only if the averages for each group are correctly estimated. Otherwise, we are talking not about statistical discrimination but simply about prejudice.*

8.2. Descriptive Empirics

What does the relative position of a minority group, say women, on the labor market consist of? First, we may ask about pre-market productive and other characteristics such as the level of education and the field of study (technical fields versus humanities), health status, access to health and child care, occupational and career preferences (inherited versus acquired through gender stereotyping). Second, we can ask about how women are treated on the labor market relative to men and how their career choices differ from those of men. Here, we measure the relative male/female employment gaps and the gender pay gap. We also ask about the differential propensity of men and women to be employed in specific industries, firms or occupations, coined as gender segregation.[27] (Similarly, one can look at ethnic residential segregation.) Much of the economics research on the relative position of women focuses on understanding the sources of the observed gender pay gap, in particular in connection to segregation.

[27] There are segregation indices available to summarize the overall extent of segregation (i.e., the Duncan index).

8.2.1. Oaxaca-Blinder Decompositions

Often, researchers use Least Squares regressions to explain the "accounting" sources of the difference in the outcome across two groups of workers, countries, etc. Much of this work follows Oaxaca (1973) and Blinder (1973) in decomposing the overall mean wage difference between the advantaged (men) and disadvantaged (women) into two parts: the first reflecting the difference in average productive endowments of individuals in each group and the second part due to the differences in coefficients. Following this approach, one first estimates logarithmic wage regressions separately for each gender, controlling for explanatory variables. The decomposition technique relies on the fact that the fitted regressions pass through the sample means as follows:

$$\overline{\ln w_g} = \widehat{\beta_g}' \overline{X_g}, \quad g \in \{f, m\}, \tag{8.2}$$

where f denotes females and m denotes males, $\overline{\ln w_g}$ is the gender-specific mean of the natural logarithm of hourly wage, and where $\overline{X_g}$ represents the respective vectors of mean values of explanatory variables for men and women. Finally, $\widehat{\beta_m}$ and $\widehat{\beta_f}$ are the corresponding vectors of estimated coefficients. A general form of the mean wage decomposition is as follows:

$$\overline{\ln w_m} - \overline{\ln w_f} = (\overline{X_m} - \overline{X_f})' \widetilde{\beta} + [\overline{X_m}' (\widehat{\beta_m} - \widetilde{\beta}) + \overline{X_f}' (\widetilde{\beta} - \widehat{\beta_f})], \tag{8.3}$$

where $\widetilde{\beta}$ represents a counter-factual non-discriminatory wage structure. The first term on the right hand side of equation 8.3 represents that part of the total logarithmic wage difference which stems from the difference in average productive characteristics across gender. The second term originates in the differences in gender-specific coefficients from the non-discriminatory wage structure and used to be interpreted as reflecting wage discrimination.[28]

Remark 25. *There are a number of variants of this method depending on how one simulates the non-discriminatory wage structure $\widetilde{\beta}$. Neumark (1988) and Oaxaca and Ransom (1994) suggest the use of regression coefficients based on*

[28] There have been objections to this decomposition approach. First, by focusing on the mean gap, it ignores meaningful differences in gender-specific wage distributions. Second, if characteristics which might differ between males and females are omitted in the vector of regressors, the contribution of these characteristics will be captured by the constant term and will erroneously appear in the measure of discrimination. Some extensions of this argument are made below.

pooled data including both men and women, arguing that they provide a good estimate of a competitive non-discriminatory norm.[29]

Remark 26. *Note that using $\beta_m$or β_f for $\widetilde{\beta}$ corresponds to estimating the ATU or ATT (see Section 2.1), respectively (when being a female is the "treatment").*

Remark 27. *Nopo (2004) and Black et al. (2005) and others now point out to matching as an alternative when support (X) is not perfectly overlapping (between men and women).*

8.2.2. Wage Gap Studies

Not very useful. We start with the overall 'raw' wage gap and try to measure the gender wage gap for comparable workers of different ethnicity or gender (the conditional or 'unexplained' gap). Recall from the previous section that the minority (gender) dummy corresponds to both potential discrimination and any number of important unobservable productivity or job-taste characteristics. To interpret the size of these gender wage gaps, some people reach for international comparisons. For example, one of the EU's structural indicators is the raw gender wage gap. The EU then asks whether a given country has a "better" gender wage gap than other economies. Similarly, the EU likes to compare the gap over time within a country. However, this is misleading because differences or changes in relative gender outcomes are often driven by the variation in the skill structure (observable as well as unobservable) of female employment participation.

Why should this be the case? If most low-skill (low-educated) women in a given country are not employed, but both low-skill and high-skill men are working, then the gender pay gap will be very small even if there is a substantial degree of discrimination. OECD (2002), a cross-country study based largely on the European Community Household Panel, suggests that cross-country differences in female employment rates are driven mainly by the degree of integration of less-educated, lower-paid women into employment and that such compositional effects are important for understanding international differences in the gender pay gap as well as in the extent of segregation. Countries with a higher degree of participation of less educated women in employment would therefore be expected to feature a relatively high level of gender segregation and gender wage gap.

[29] Neumark (1988) provides a theoretical justification for this approach using a model of discrimination with many types of labor where employers care about the proportion of women they employ.

Non-random selection of women into work across countries may indeed explain a large part of variation in the gender pay gap across countries. This notion is supported by the observed variation in employment gaps, from 10% in the US, UK and Scandinavian countries, to 15-25% in northern and central EU, up to 30-40% in southern EU and Ireland (Olivetti and Petrongolo, 2005).[30] If women who are employed tend to have relatively high-wage characteristics (both observed and unobserved), low female employment rates may become consistent with low gender wage gaps simply because low-wage women would not feature in the observed wage distribution.

Similarly for time changes in the gender pay gap: Hunt (2002) and Kazakova (2005) suggest that large changes in the observed gender wage gap (even after we attempt to compare comparable male and female workers) can be linked to major changes in the skill structure of female employment in East Germany and Russia, respectively.

The differences in the level and structure of female labor-market participation can then be either related to discrimination (if women are discriminated against, they may be less likely to participate in the labor market), labor-market institutions (such as high wage floors, which may prevent low-productivity workers from being employed) or be driven by country-specific culture or history.

Mulligan and Rubinstein (2008) follow US wage inequality between as well as within genders from the 1960s and argue that selection into the female full-time full-year workforce shifted from negative in the 1970s to positive in the 1990s, and that the majority of the apparent narrowing of the gender wage gap reflects increased attachment of the most able women to the labor force. Demographic groups with high and stable female employment rates have little measured relative wage growth for women in the US. Growing wage equality between genders coincided with growing inequality within gender.

[30] This paper follows Neal (see the next section) and uses his simple alternative to estimating Heckman-style sample selection models: assume that those women who do not work, in case they did work, would have wages that would be below the median of those women with similar characteristics who actually do work. This assumption allows one to consider wages of all workers, employed or not, and measure the (conditional) median wage gap across the two genders. While the mean wage gap would be affected by the value of the assumed wage for those who do not work, median wages are not affected by the particular value, as long as wages of those not working would indeed be below the median wage, were they to work.

8.2.3. Studies of Wage Gap and Segregation

Again, not very useful. One of the most clearly established labor-market 'gender' facts is that women and men tend to concentrate in different occupations and industries. This is an important concern because those occupations and industries staffed mainly with female workers typically pay lower wages to both men and women compared to predominantly 'male' occupations and industries. The observed persistent concentration of women in low-paid groups of workers, coined gender segregation, is therefore a key explanation for the existence of the gender wage gap.

The high concentration of women in low-wage employment could be the result of gender stereotyping or discrimination. However, it may (also or alternatively) be a matter of gender-specific preferences and choice. In other words, it could be that (i) discriminating employers prevent women from working in high-wage occupations, or that (ii) 'female' occupations offer costly non-wage characteristics preferred by women such as for example flexible working hours.[31] Standard equal employment opportunity clauses aim to reduce all forms of segregation resulting from potentially discriminatory hiring, firing, and promotion practices. But standard measures of occupational segregation do not allow one to differentiate between the discrimination-related and choice-driven explanations of segregation.

There is some research trying to indirectly differentiate between the two explanations ((i) and (ii)) by estimating models of wage determination. Wage structure research on U.S. and Canadian data (Macpherson and Hirsh, 1995; Baker and Fortin, 2001) has established the existence of a 'penalty' to working in 'female' occupations and has also shown that the size of the 'penalty' decreases significantly after controlling for occupational attributes and/or unmeasured worker preferences and quality (using switchers, see Section 2.6). This would suggest that occupational gender segregation in the U.S. is to a large extent driven by preferences, not discrimination. Jurajda and Harmgart (2007) follow on the Hunt (2002) study to suggest that the 'penalty' to working in highly 'female' occupations depends on the extent of participation by low-wage women, much like the gender wage gap. Machin and Puhani (2003) show that a major part of the gender wage gap among recent school graduates in Germany and the U.K. can be explained by gender differences in the field of study. Again, does this mean that field of study preferences are genetic or stereotyped?

[31] Women are apparently happier working in female dominated workplaces (where there is greater job flexibility). Bender et al. (2005), Fernandez and Asadullah (2007).

One can also study segregation by focusing on specific occupations, such as the highly visible group of managers. The representation of women among top-level managers and their relative wage position are of high academic as well as general public interest. In the US, the share of female executives is increasing (close to 10%) and the managerial gender pay gap is narrowing (Bertrand and Hallock, 2001; Bell, 2005).

One can ask if the gender wage gap affects other outcomes. Three quarters of all violence against women is perpetrated by domestic partners. Aizer (AER, 2010) exploits exogenous changes in the demand for labor in female-dominated industries to estimate the impact of the gender wage gap on domestic violence. Decreases in the wage gap reduce violence against women, consistent with a household bargaining model.

8.3. Testing for Discrimination

8.3.1. Wage Gap Studies

Competition and Discrimination While the size of the gender wage gap (even of the conditional one) is hard to interpret, the Becker's (1957) preference-based discrimination theory suggests that discrimination should decrease in face of competition. Employers who pay male employees a wage premium (or pay women less than MRPL) in order to indulge their discriminatory tastes accrue additional costs and are unable to compete with others who do not have such preferences as product markets deregulate or open to international trade, i.e., as entrants increase competition. Therefore, firms in more concentrated (less competitive) product markets should exhibit higher levels of gender or ethnic wage gaps since rents are available for indulging such tastes.

There is some supportive evidence for this notion. Ashenfelter and Hannan (1986) show that US banks facing more competition hire more women (have a higher share of female workforce) and have lower gender wage gaps. Similarly, there is a positive relationship across manufacturing firms in the share of women on workforce and economic performance, but only in industries facing lower competition (Hellerstein, Neumark, and Troske, 2002). Similarly, the firm-level gender wage gap has gone down faster in those Hungarian industries that became more open to international trade and became less concentrated in the last decade (Lovasz, 2007).

A more complete test of Becker's theory is in Charles and Guryan (2008). They use data on racial prejudice from the General Social Survey and find support for all of the key predictions from Becker about the relationship between prejudice and racial wage gaps: relative to white wages, black wages: (a) vary negatively with a measure of the prejudice of the marginal white in a state; (b) vary negatively with the prejudice in the lower tail of the prejudice distribution, but are unaffected by the prejudice of the most prejudiced persons in a state; and (c) vary negatively with the fraction of a state that is black.[32]

Black-White Wage Gaps and Pre-Market Characteristics Neal and Johnson (1996, JPE) show that controlling for ability measured in the teenage years (AFQT, kind of an IQ measure) eliminates young adult wage gaps for all groups except for black males, for whom they eliminate 70% of the gap. They look at identically skilled teens before market entry and then again later in life and ask: What is the initial earnings gap and does it grow over time? Assuming there are no differences in tastes or costs of skill investment, one could attribute wage differences between comparable workers to discrimination. In Table 1, they show that 'pre-market' skills appear to explain a large part of racial earnings gap for currently employed workers.

Next, in Figure 1, they note that low scoring blacks are noticeably less likely to participate in labor market. To deal with the problem of selection into employment, they use median regressions. If nonparticipants have wage offers lower than the median wage for the employed group with similar observables (race, test score) and at least half of each group participate, then the median is identified. In Table 4, they therefore use median regressions to suggests that much less of the gap is explained once we condition on participation.

Finally, they ask what explains AFQT scores. There are huge racial gaps, but adding a bunch of family background, home environment, and school quality covariates reduces these considerably. This suggests that pre-market factors have a lot of potential explanatory power for this gap.

Carneiro, Heckman and Masterov (2003) return to minority-white wage gaps. They point out that others have faulted Neal and Johnson because minority children and their parents may have pessimistic expectations about receiving fair rewards for their skills and so they may invest less in skill formation. If this is the case, discrimination may still affect wages, albeit indirectly, though it would appear that any racial differences in wages are due to differences in acquired traits. In contrast to this view, Carneiro et al. find that gaps in ability across racial and

[32] They also go through an extension of the original theory suggesting that the model's main predictions can survive the effects of long run competition.

ethnic groups open up at very early ages, long before child expectations are likely to become established. These gaps widen with age and schooling for Blacks, but not for Hispanics which indicates that poor schools and neighborhoods cannot be the principal factors affecting the slow black test score growth rate. Their evidence points to the importance of early (preschool) family factors and environments in explaining both cognitive and non-cognitive ability differentials by ethnicity and race. They argue that policies that foster both types of ability are far more likely to be effective in promoting racial and ethnic equality for most groups than are additional civil rights and affirmative action policies targeted at the workplace.[33]

Statistical Discrimination Return to Altonji and Pierret (2001), which we discussed in the human-capital section. In fact, their test of the signalling hypothesis is a test of 'statistical discrimination' on education. Are employers initially using education as a proxy for unobserved (noisy) ability? They suggest that this is the case.

Next, they also ask about racial wage gaps. Do employers statistically discriminate on race as a proxy for AFQT? If so, one would expect race to be negative absent the AFQT x time measure. Once the time interaction is added, this should make the race main effect more negative, while the time interaction with race should be more positive. If race is taken as a (negative) productivity signal early in the career, it should become less important over time as actual productivity is revealed. In fact, the opposite occurs. So, there appears to be little statistical discrimination on race, employers may be obeying the legal prohibition of statistical discrimination. The race intercept is zero in year 0 of market entry—employers are not using the average productivity of the group as a signal—and they are learning about the true productivity over time—they are 'surprised'.

8.3.2. Direct Tests in Specific Settings

Discrimination in Sports and in Instant Decisions Price and Wolfers (2007) "Racial Discrimination Among NBA Referees" NBER Working Paper No. W13206 study the behavior of NBA referees to test for taste-based discrimination.

[33]Environment must influence I.Q. James Flynn established that I.Q. increased by 18 points in the US from 1947 to 2002 and such abrupt changes could not be the work of genes. If social factors can produce such changes over time, they can also lead to differences between subpopulations at any given time. Indeed, the I.Q. difference between black and white 12-year-olds has dropped by a third in the US in the last 30 years.

NBA referees and players are involved in repeated interactions in a high-pressure setting with referees making the type of split-second decisions that might allow implicit racial biases to manifest themselves. They find – even conditioning on player and referee fixed effects (and specific game fixed effects) – that more personal fouls are called against players when they are officiated by an opposite-race refereeing crew than when officiated by an own-race crew. This affects who wins.[34]

A similar recent analysis of baseball umpires by Parsons, Sulaeman, Yates and Hamermesh (NBER WP No. W13665, 2008) also shows racial player-umpire match effects, but only in games where "there is little scrutiny of umpires' behavior - in ballparks without computerized systems monitoring umpires' calls, ...".

The NBA research covers instantaneous split-second decisions. Joshua Correll at the University of Chicago has created an on-line test called "the police officer's dilemma," in which you encounter 100 pictures of black and white men, some armed and some unarmed (holding cellphones). The idea is to shoot those who are armed (as quickly as you can) and holster your gun when you see someone unarmed — and the program measures how fast you do these things. It turns out that most whites and many blacks will show racial biases in this test, shooting armed blacks fractions of a second faster than armed whites and, conversely, holstering gun more quickly when encountering unarmed whites than unarmed blacks. Similar unconscious attitudes on race, age, gender, and religion have been shown in such split-second decision tests. This applies to even the most conscientious anti-racists[35] and may correspond to cognitively build-in (hard-wired) statistical discrimination and stereotyping (powerful particularly in early childhood).

Women as Leaders Beaman, Chattopadhyay, Duflo, Pande, and Topalova (2008, CEPR DP No. DP6922) use random assignment of gender quotas across Indian village councils to investigate whether having a female chief councillor affects public opinion towards female leaders. Villagers who have never been required to have a female leader prefer male leaders and perceive hypothetical female leaders as less effective. Having a female leader does not alter villagers' taste preference for male leaders. However, it weakens stereotypes about gender roles in the public and domestic spheres and eliminates the negative bias in how female leaders' effectiveness is perceived among male villagers. Villagers rate their women leaders as less effective when exposed to them for the first, but not second, time. After 10

[34] See also http://www.bepress.com/ev/vol4/iss5/art1/

[35] See and get tested yourself at http://backhand.uchicago.edu/Center/ShooterEffect/ and https://implicit.harvard.edu/implicit/.

years of the quota policy, women are more likely to stand for and win free seats in villages that have been continuously required to have a female chief councillor.

Discrimination in Arts Goldin and Rouse (2001, AER) suggest that a change in the audition procedures of symphony orchestras–adoption of "blind" auditions with a "screen" to conceal the candidate's identity from the jury–provides a test for sex-biased hiring. The screen increases the probability a woman will be advanced and hired. More specifically, the setup is as follows: 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? First, they find that on average, women do worse on blind rounds. But this could be due to changing composition of female pool. It is possible that only the very best women competed when the game was lopsided. Indeed, estimation 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).

Racial Discrimination in Hiring Bertrand and Mullainathan (2003) is an example of an audit field-experiment study. 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?

The short answer is yes. Callback rates are lower for black sounding names. Further, black names appear to benefit less from resume enhancements (such as honors, more experience) than do whites. The authors view this as evidence against statistical discrimination. Next, discrimination based on zip-code characteristics appears quite important and does not systematically differ between white and non-white names. (Again, the authors view this as evidence against statistical discrimination.)

Beauty and Discrimination There are studies documenting that body height and BMI predict wage differences (in a predictable way). Similarly, there is a beauty premium in wages, elections, access to credit, etc. (Hammermesh, 2006 looks at elections, controlling for candidate productivity; Belot et al., 2007, show more beautiful players are less likely to be eliminated by others in a TV game; Ravina, 2008, contends more beautiful people are more likely to get a loan). Is there a consumption value basis for the beauty premium?[36]

On a related note, NBER Working Paper No. 13879 shows that there is economic value of having healthy teeth (a visible component of well-being) in the US. They use variation in access to fluoridated water during childhood and find that women who resided in communities with fluoridated water during childhood earn approximately 4% more than women who did not, but there is no effect of fluoridation for men. This effect is concentrated amongst women from families of low socioeconomic status. They find little evidence to support occupational sorting, statistical discrimination, and productivity as potential channels of these effects, suggesting consumer and employer discrimination as the residual channel.

Gender Gaps and Home Appliances Oster and Jensen (2007, NBER Working Paper No. W13305) use panel data to suggest that the increasing access to cable and satellite television in rural India is associated with improvements in women's status (higher autonomy, decreases in the reported acceptability of beating and decreases in reported son preference, higher female school enrollment and decreases in fertility.

Cavalcanti and Tavares (2008) point out that a decrease in the relative price of home appliances - the ratio of the price of appliances to the consumer price index - leads to a substantial and statistically significant increase in female labour force participation.

8.3.3. Psychological Explanations of Gender Gaps

Psychological explanations (as opposed to discrimination) for the existence of gender gaps are increasingly being tested with field data. For example, is the presence of a gender gap among talented and determined managers (or scientists) evidence of discrimination? Possibly, even if experimental research focusing on gender differences in performance in managerial tasks suggests otherwise. Gneezy, et al. (2003) suggest that women may be less effective than men in competitive environments, even if they can perform similarly well in non-competitive environments. Price (2008) suggests that a competitive graduate fellowship program, which aimed at increasing graduation rates, helped men on average, but benefited women only when a larger fraction of the group was female. Örs, Palomino and

[36] For ideas on why being beautiful shows in wages see http://people.ischool.berkeley.edu/~hal/people/hal/NYTimes/2006-04-06.html

Peyrache (2008) also study educational outcomes within a homogenous group—namely applicants to a top-ranked French business school. They show that within this group, women outperform their male colleagues in non-competitive comprehensive tests, but lag behind men in the highly competitive school admission process. Jurajda and Munich (2008) provide similar evidence for a whole cohort of Czech secondary school graduates applying to universities.

If women indeed perform worse in competitive tasks, it is not surprising that Niederle and Vesterlund (2007) imply that men are more likely to select into a competitive environment (a tournament) than women of the same ability. They contend that "women shy away from competition and men embrace it". Such gender competition performance gaps could help explain the near absence of women from top-level managerial positions, which are awarded in repeated tournaments. Differences in "taste for competition" could be innate or the result of gender roles in society. Gneezy et al. (2008) suggest that men prefer competition more in a patriarchal society (the Maasai in Tanzania) while women like competition more in a matrilineal society (the Khasi in India). Along similar lines, Booth and Nolen (2008) suggest that single-sex environments modify risk-taking preferences. Girls from single-sex schools are as likely to choose a real-stakes gamble as boys from either coed or single sex schools, and more likely than coed girls.[37] In a related line of work, Babcock and Laschever (2003) report that women may not negotiate as toughly as men on salary issues. But this would be natural if their access to these jobs is harder (if there are barrier to entry).

A related question is why there are so few female scientists. Guiso et al. (2008, *Science*) suggest that the gender gap in PISA math scores (girls do worse than boys) disappears in countries with a more gender-equal culture (think Norway vesrus Turkey). In these countries, girls do even better in reading, suggesting that even though they do as well as boys in math, their comparative advantage in humanities remains intact (don't expect more women in hard sciences).

8.4. Czech Labor-Market Gender Facts

Jurajda and Franta (2006) use a decade of Czech LFS data to show that the main reason why the aggregate employment rate in the Czech Republic was higher than that of the EU-15 in 1999 was the higher Czech employment rate of women aged

[37] See also references in Booth (Labour Economics, 2008 EALE presidential address).

25-54 with less than tertiary level of education.[38] Given the discussion above, such higher participation of low-wage women implies that the Czech gender (raw) wage gap should be somewhat higher than elsewhere. And it is, consistent with Petrongolo and Olivetti (2005) or Hunt (2002).[39]

The incidence of part-time employment remains low in the Czech Republic, thanks in part to unusually high full-time employment rates of younger Czech women (and their low fertility). Only 5% of Czech women work in part-time contracts compared to almost 26% in the EU27. The Czech employment rate gap between women with and without young children is the highest in the EU, due to the combination of high participation of women without children and low participation of women with children. This is not surprising given that only one third of kindergartens admit all applicants, while about a half of public kindergartens admits at most 40% of applicants aged 3-5. The child-care situation is even more difficult for children under 3.

The overall extent of occupational gender segregation in the Czech Republic is similar to that observed in EU-15 economies (Jurajda and Franta, 2007), thanks in part to the recent decline in occupational segregation for younger workers.

About two thirds of the Czech gender wage gap in the enterprise sector remain unaccounted for even after one controls for education, age, type of employer and the extent of gender segregation. The majority of the 'explained' part of the gap is attributable to different forms of gender segregation; both forms of segregation considered (at firm and occupation level) are important sources of overall wage differences between men and women (Jurajda, 2003). There is a significant pay gap of about 10% even among men and women working in the same firm in the same very detailed occupation. Not clear to what extent maternity leaves are at fault here.

According to Jurajda and Paligorova (2007), women are well represented in the lower-managerial ranks of Czech firms, but only about 7 percent of top-level Czech managers are women. This is actually quite comparable to US figures.[40] The overall pay gap is higher for top managers than for lower-level workers, but this appears to be due mainly to the highest paying firms having fewer female

[38] Manufacturing is responsible for an unusually high share of employment in the Czech Republic (about 10 percentage points above EU-15 and EU-27 averages).

[39] Recently, there is evidence on sample selection of women into imployment (using the SILC data): indeed, the coefficient on Heckman's lambda is positive and significant, meaning that the gender wage gap would be larger if it wasn't for the selective participation of women.

[40] Similarly, Jurajda and Harmgart (2007) find a high correlation in the 'femaleness' of occupations in East and West Germany in 1992.

top managers, rather than to a different 'treatment' of women at various firm hierarchy levels.

Overall, not much useful evidence on discrimination. The overall situation appears similar to the EU average.

9. Matching and Search Frictions

Unlike equilibrium job search models, equilibrium matching models with search frictions generate wage distributions.

Melvyn Coles (EALE 2007 lecture): Matching models and optimal UI. Moral hazard means that we cannot offer full insurance. To insure workers against unemployment, one can use UI as well as policies that increase job creation (JC) and thus shorted unemployment duration. Of course, the introduction of UI increases wages in equilibrium because UI increases the value of not working.[41]

When designing optimal UI, there are several externalities to consider ("technological," i.e., congestion,[42] thick market) and the fiscal externality: financing UI benefits through taxes lowers JC. Neither firms nor workers take the fiscal (as well as other) externality into account. Optimal UI should also reflect the welfare of everybody, not just employed workers (who are getting insured against layoffs). If your interest is to smooth consumption, UI should start high. According to Melvyn Coles (in print) UI should then decrease with unemployment duration to increase search effort and to lower the costs and the fiscal externality of UI.

10. Unions

Contracts, strikes

11. Incentives

The design of incentive systems within organizations: trade-offs between insurance and incentives, tournament theory, moral hazard in teams, and multi-tasking. Prendergast (1999), Lazear and Rosen (1981)

[41] There is a holdup problem in that to create a match between a worker and a firm, both have to invest first (into creating a vacancy, into searching).

[42] Congestion externalities, where my job search crowds out your job finding chances, are not important in a dynamic setting because my job search today means I will have a job tomorrow and I will thus make room for others.

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