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ABSTRACT

Understanding whether labor market discrimination explains inferior labor market outcomes for many groups has drawn the attention of labor economists for decades – at least since the publication of Gary Becker’s *The Economics of Discrimination* in 1957. The decades of research on discrimination in labor markets began with a regression-based “decomposition” approach, asking whether raw wage or earnings differences between groups – which might constitute prima facie evidence of discrimination – were in fact attributable to other productivity-related factors. Subsequent research – responding in large part to limitations of the regression-based approach – moved on to other approaches, such as testing direct predictions of the Becker model using data on discriminatory tastes, or using firm-level data to estimate both marginal productivity and wage differentials. In recent years, however, there has been substantial growth in experimental research on labor market discrimination – even though the earliest experiments were done decades ago. Some experimental research on labor market discrimination takes place in the lab. But far more of it is done in the field, which makes this particular area of experimental research unique relative to the explosion of experimental economic research more generally. This paper surveys the full range of experimental literature on labor market discrimination, places it in the context of the broader research literature on labor market discrimination, discusses the experimental literature from many different perspectives (empirical, theoretical, policy, and legal), and reviews what this literature has taught us thus far, and what remains to be done.

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1. Introduction

Understanding whether labor market discrimination explains inferior labor market outcomes for many groups has drawn the attention of labor economists for decades – at least since the publication of Gary Becker’s *The Economics of Discrimination* in 1957. The question is of obvious importance for public policy, as the answer can help policymakers determine how much to emphasize efforts to combat discrimination in trying to bring about greater inequality between racial and ethnic groups and between men and women, and to improve the circumstances of other groups that suffer economic disadvantages, such as the disabled.

The decades of research on discrimination in labor markets began with a regression-based “decomposition” approach, asking whether raw wage or earnings differences between groups – which might constitute prima facie evidence of discrimination – were in fact attributable to other productivity-related factors. Subsequent research – responding in large part to limitations of the regression-based approach – moved on to other approaches, such as testing direct predictions of the Becker model using data on discriminatory tastes, or using firm-level data to estimate both marginal productivity and wage differentials.

In recent years, however, there has been substantial growth in experimental research on labor market discrimination – even though the earliest experiments were done decades ago. The growth in the experimental research on discrimination likely in part reflects the growth of experimental research in economics generally. However, it is also a specific response to continuing challenges in drawing definitive conclusions from non-experimental research about the role of labor market discrimination. Some experimental research on labor market discrimination takes place in the lab. But far more of it is done in the field, which makes this particular area of experimental research unique relative to the explosion of experimental economic research more generally.

My goal in this survey is to cover the full range of experimental literature on labor market discrimination, to place it in the context of the broader research literature on labor market discrimination, to discuss the experimental literature from many different perspectives (empirical, theoretical, policy, and

legal), and to review what this literature has taught us thus far, and what remains to be done. There are points of overlap with other recent research reviews. Anderson et al. (2006) provide a brief review of laboratory experiments on discrimination covering a small set of studies on race or ethnicity, excluding studies that ask subjects to make decisions about fictitious workers. Pager (2007) provides an overview of a small number of field experiments of race discrimination, focusing on differences between the clear evidence of race discrimination from these studies and the suggestion from some analyses of secondary data that race no longer matters much in labor markets. Riach and Rich (2002) and Rich (2014) provide summaries of many field experiments on discrimination in labor markets; the thoroughness of these reviews enables me to avoid a detailed cataloging of results from many studies, and instead to discuss experimental research on labor market discrimination in the context of the broader literature and debates about discrimination, to focus to some extent on methods, and to delve into what I view as the most important issues that these studies confront or still have to confront.¹ In addition, there has been a large number of experimental studies done in the last year or two, which figure prominently in some parts of this review. But clearly these other reviews can be usefully read along with this survey to get an even fuller overview and understanding of what economists have learned and still might learn from experimental research on labor market discrimination.

2. The Facts We Are Trying to Understand

The raw data that motivate most of the research on discrimination are generally in the direction consistent with discrimination – e.g., lower pay for groups thought to experience discrimination in the labor market. But untangling whether discrimination in fact generates these differences is difficult – which explains the explosion of experimental research on labor market discrimination. Here I briefly describe many of the stylized facts that underlie the discrimination literature, drawing on other work. I defer until

¹ Two reviews published in this journal are further afield, yet related. Harrison and List (2004) provide a general review of field experiments, focused largely on defining a typology of experiments, highlighting what they can teach us in comparison to other kinds of studies, and illustrating the kinds of questions they can answer. There is little overlap although one section of their paper has a few references to audit or correspondence studies of discrimination. Lang and Lehmann (2012) provide what is largely a survey of theoretical models of race discrimination, with some emphasis on drawing empirical implications. Again, the overlap is minor, although their discussion connects with the material discussed later on distinguishing between taste and statistical discrimination. In addition, there is a brief discussion of audit/correspondence studies that touches on some of the points I cover in detail.

Section 4 discussion of non-experimental work testing discriminatory versus non-discriminatory explanations of these differences, except to touch on some simple partial correlations when available.

The experimental research focuses on hiring, and thus might be thought to be most relevant for understanding group differences in employment and unemployment. However, the segregation evidence for women discussed below shows that hiring discrimination may also lead to lower earnings. Moreover, models show that discrimination in hiring can lead directly to lower earnings for groups that experience this discrimination.

2.1 Race, Ethnicity, and Sex

In the United States, there has been a persistent raw difference in the earnings of black and white men – a bit over 20 percent over the past decade, and a shade larger for full-time, year-round workers (Lang and Lehmann, 2012). Some factors explain part of this gap; in particular, Neal and Johnson (1996) showed that controlling for age and performance on the Armed Forces Qualifying Test (AFQT) diminishes the race gap for young men substantially. But other factors cut in the other direction, such as schooling (conditional on the AFQT, blacks get more schooling). Lang and Lehmann also note that earnings differences between black and white women have historically been lower and sometimes even reversed. Wages of Hispanics also lag substantially behind those of whites, with about 32 percent lower median weekly earnings for full-time workers (Kochhar, 2008). The gap is about twice as big for foreign-born as native-born Hispanics.

Pay differences between men and women in the United States have fallen since the beginning of the 1980s, but still persist, with the gap in annual earnings for full-time, year-round workers recently narrowing to around 25 percent (Blau and Kahn, 2007). Some samples now show women getting more education, but men have tended to study more lucrative fields. Lower work experience of women also plays a role, but this difference has declined as well.

Employment (and unemployment) gaps between blacks and whites are pronounced in U.S. data. Lang and Lehmann (2012) report a difference in the labor force participation rate of black versus white men aged 25-54 of 7.8 percentage points in 2008, and an unemployment rate difference of 4.6 percentage

points. The unemployment rate differential appears unrelated to education, and there is a large earnings differential (which, in contrast to wages, also reflects hours and employment differences that persist when controlling for AFQT scores (Johnson and Neal, 1998)). Hispanics have participation rates that slightly exceed those of whites, but unemployment rates are nearly a third higher (Kochhar, 2008). Women continue to have lower employment rates than men; for example, their labor force participation rate in 2013 was lower by 12.5 percentage points. However, their unemployment rates are slightly lower than men's are.²

There is similar *prima facie* evidence of discrimination in other countries, for groups that are the focus of experimental work on discrimination. As but one example, there are lower employment rates for indigenous people and immigrant groups in Australia, although these become smaller and insignificant with controls for language proficiency and schooling (Booth et al., 2012). Many studies of Europe (some in the references) document lower earnings and employment for minorities or immigrants.

Discrimination in hiring may affect where a person works, with important implications for earnings if women are hired into lower-paying occupations or firms (e.g., Groshen, 1991).³ There is also substantial workplace segregation by race and especially by Hispanic ethnicity in the United States (Hellerstein and Neumark, 2008). Segregation could be driven by skill; this study finds that education explains very little of workplace segregation by race, but that language skills explain about a third of Hispanic-white segregation. The racial segregation does not help explain lower black wages, as blacks actually work in higher-paying establishments. However, Hispanics tend to work in slightly lower-paying establishments.

2.2 Age

Older workers generally earn *more* than younger workers. However, it has long been suggested that older workers have difficulty finding new jobs. They have longer unemployment durations than middle-aged workers in U.S. data, and prior to the Age Discrimination in Employment Act (ADEA) explicit age restrictions in hiring were common. For example, in five cities in states without anti-age

² See <http://www.dol.gov/wb/stats/recentfacts.htm> (viewed August 19, 2015).

³ Occupational segregation may be more reflective of choice than discrimination; but firm-level segregation is harder to interpret as reflecting choice.

discrimination statutes, nearly 60 percent of employers imposed upper age limits (usually between ages 45 and 55) on new hires (U.S. Department of Labor, 1965). During and after the Great Recession unemployment durations rose particularly sharply for older workers, as did ADEA claims filed with the U.S. Equal Employment Opportunity Commission (EEOC) (Neumark and Button, 2014).

2.3 Evidence on Other Groups Covered in the Experimental Literature

A number of studies document labor market differences for other groups that have been the focus of the experimental literature on discrimination. A number of papers document earnings penalties for gay men relative to straight men, but earnings premia for lesbian relative to straight women (see Burn, 2015). For example, data from 2012 indicate an earnings penalty for gay men of about 8 percent, with demographic, occupational, and other controls (Burn, 2015).⁴ Many studies have documented earnings penalties associated with lower physical attractiveness (Hamermesh, 2011). There is also evidence of wage penalties for obesity, even in longitudinal data with individual or family fixed effects (Baum and Ford, 2004; Lundborg et al, 2014). Wage penalties for obesity could also reflect factors similar to “lookism” (Caliendo and Gehrsitz, 2014). Ameri et al. (2015) document that the employment rate for the disabled is less than half that of the non-disabled (33 percent versus 74 percent in 2012), their unemployment rate is about double (14.7 percent versus 7.2 percent in 2013), and that they are paid less.

Finlay (2007) discusses descriptive evidence related to a criminal background, which points to lower wages and employment for those who have been incarcerated. However, the evidence is less clear conditional on individual fixed effects, consistent with negative selection into incarceration and less consistent with a causal effect. Pager (2003) also notes the strong and consistent negative relationships between incarceration and labor market outcomes, and that it is difficult to establish causal relationships, given the role of unobserved heterogeneity.

For at least some of these categories of workers the differences in earnings and other outcomes can surely be less attributable to discrimination, for one of two reasons: the defining feature of the group may not be completely exogenous; or the defining feature of the group may in fact be associated with lower

⁴ Since gays are identified for couples, based on living arrangement and relationship, the comparison is to married straight men.

productivity. Of course, the discrimination literature more broadly considers the argument that differentials associated with sex, race, or ethnicity may reflect productivity differences rather than discrimination. There may be a difference, however, for groups for which a productivity difference is in a sense *inherent* in the defining group characteristic, such as an employer concerned about liability for hiring a convicted felon, hiring a disabled worker in a job in which the disability lowers productivity, or who regards an employee's looks as contributing to their productivity.

3. Explanations Based on Models of Discrimination

In this section, I briefly discuss theoretical models of labor market discrimination. The goal is to focus on what these models predict about possible findings from experimental studies – i.e., their observable implications – and what the models imply for how to interpret the experimental evidence.

3.1 Taste Discrimination

Becker (1957) considered three models that were distinct, but unified in focusing on the market implications of discriminatory tastes of three different types of agents – employers, employees, or customers. The model of employer discrimination is the one through which the majority of empirical work on discrimination has been interpreted.

3.1.1 Employer Discrimination

In the employer discrimination model, employers dislike hiring a particular group, such as blacks.⁵ When a black is hired, an employer considers the cost to be both the wage and the disutility from hiring the worker. Thus, we think of employers as utility maximizers, with utility function

$$U = U(\pi, B) = \pi - d \cdot B, \tag{3.1}$$

where π is profits, B is the number of black workers, and d is a constant > 0 , reflecting discriminatory tastes against blacks (a simple form of discriminatory tastes). Assume the production function is

$$y = f(W + B), \tag{3.2}$$

a particularly simple form in which there are no inputs except labor, and W (whites) and B are perfect substitutes. The perfect substitutes assumption coupled with equal coefficients makes the meaning of

⁵ I use blacks and whites here to stand in for any two groups, the first of which might be disfavored. I change the reference to other groups when issues specific to those groups (e.g., men versus women or young versus old) arise.

discrimination clear, as white and blacks are interchangeable in production. We also assume that labor supply is perfectly inelastic, so wages are determined solely by the demand side. Normalizing the price of output to one, and letting w_i denote the wage of group i ($i = W, B$), the first-order conditions are

$$MP_L = w_W, MP_L = w_B + d \quad (3.3)$$

Since d is positive, these imply $w_B < w_W$, and in particular

$$w_W = w_B + d. \quad (3.4)$$

What does this imply about employment and in particular hiring, which is what the experimental research on labor market discrimination studies? If d is the same for all employers, then when $w_W = w_B + d$ employers are indifferent between hiring white and black workers, so we expect most employers to hire both. In contrast, if $w_W \neq w_B + d$, employers will only want to hire one race or the other (e.g., only whites if $w_W < w_B + d$); as a consequence, wages will adjust in equilibrium to absorb both races, until $w_W = w_B + d$. In this case, there is no reason for employers to discriminate against blacks in hiring, as the wage differential just offsets the disutility from hiring. But if, as seems more likely, d varies across employers (d_j for employer j), an equilibrium wage differential should be established such that if $d_j > w_W - w_B$ the employer hires only whites, and if $d_j < w_W - w_B$, the employer hires only blacks. In this case, in an experimental study we would expect some employers to prefer hiring whites if there is discrimination.

However, things are a bit more complicated. In this formulation employers with $d_j < w_W - w_B$ should prefer hiring blacks, so even if there are discriminatory employers, the implication for an experiment attempting to estimate a preference for black or white employees is unclear. If there is an equal wage constraint or at least a constraint on relative black and white wages (perhaps because of minimum wages), then it is more clear that we should, on net, find evidence of discrimination against blacks in hiring.⁶ Alternatively, given that most wage equation evidence points to lower wages for blacks with similar observables to whites, we might presume that there must be a relative abundance of discriminatory employers, since even for the relatively small number of blacks to be absorbed by the labor market the

⁶ Indeed, Neumark and Stock (2006) find robust evidence that sex discrimination/equal pay laws introduced in earlier decades (at the state and federal level) reduced the relative employment of black women and white women.

inframarginal employer would appear to be discriminatory.⁷

A different question is whether a finding of hiring discrimination in the kind of field experiment discussed below is actually informative about discrimination. As Heckman (1998) emphasizes, the Becker model explores the market-level consequences of the presence of some employers with discriminatory tastes for equilibrium wage differences unrelated to productivity. But because wages are set at the margin, the presence of some discriminatory employers that an experiment on hiring discrimination might detect does not necessarily imply market-level discrimination in wages. For example, in the formulation above, if there are enough non-discriminatory employers relative to black workers that the former can employ all of the latter, then d at the marginal employer will equal zero, and black and white wages will be equal.

However, in search models with discrimination the presence of even one discriminatory employer, who ends up not hiring blacks, can shift the offer wage distribution in the market, lowering equilibrium wages for blacks relative to whites (Black, 1995). Moreover, if these experiments are detecting statistical rather than taste-based discrimination, there is no reason the discrimination would not be reflected in market wages. Finally, individual-level discrimination based on many group characteristics is illegal, so field experiments on hiring discrimination help us gauge whether policy has succeeded in rooting out this discrimination, or whether there are other forms of discrimination that policymakers might want to address.

Another question is whether employer discrimination can persist in the long run, or is undermined by discrimination. If discrimination cannot persist in competitive markets, and one believes markets are sufficiently competitive, then it is natural to be skeptical of claims of discrimination based on experiments – at least from field experiments in real markets. However, the claim that competition necessarily eliminates discrimination is often overstated. Even Becker (1957) clarified conditions under which discrimination could persist, and other theoretical work added refinements (e.g., Goldberg, 1982).

3.1.2 *Employee and Customer Discrimination*

⁷ A second complication is that this formulation of the employer discrimination model implies near-perfect employment segregation, with almost all firms hiring only one race or the other; but that is not generally observed. However, if we instead assume that employers care about the relative number of black employees, rather than the absolute amount, then employers can alter the cost of hiring blacks by adjusting the relative number of blacks, and for any equilibrium wage ratio firms will be willing to hire some whites and some blacks (except for indivisibility problems).

In the customer discrimination model, customers require a lower price when buying from blacks, because of the disutility cost of the transaction. Some of the experimental literature tests for customer discrimination by asking whether hiring discrimination is more severe for jobs with customer contact.

In the employee discrimination model, whites require a higher wage to work alongside blacks. This implies the following expressions for the profits of a firm in each of three scenarios: an all-white workforce, an all-black workforce, and an integrated workforce. These are, respectively:

$$\begin{aligned}\pi_W &= f(W) - w_W W \\ \pi_B &= f(B) - w_B B \\ \pi_{WB} &= f(W + B) - (w_W + d)W - w_B B\end{aligned}\tag{3.5}$$

Here d is the wage premium that has to be paid to whites to work alongside blacks, and I use a simple specification in which they require d if they work with *any* blacks. This model predicts completely segregated workforces, with equal wages, and hence appears at first blush to be unable to explain the stylized facts, and has unclear implications for hiring discrimination experiments.

However, Arrow (1972) discusses extensions of the model that better fit the facts, and make it more likely that we would find lower hiring of blacks in a field experiment. The employee discrimination model implies, over some range, large quantity changes in response to small price changes. For example, starting from equal wages for whites and blacks, a slight decrease in the relative price of black labor should induce a firm with an all-white workforce to switch to an all-black workforce. This seems unlikely to occur, however, because of adjustment costs, and Arrow shows that once we account for adjustment costs, and presume that firms initially have predominantly-white or all-white workforces, it is easier to tell a story in which there is incomplete segregation of workforces, and blacks earn lower wages and have worse job opportunities. The same argument would apply to cases where we are considering discrimination against women, immigrants, etc., who may have arrived to or entered the labor market later.⁸

3.2 Statistical Discrimination

In the simplest formulation of the statistical discrimination model (Aigner and Cain, 1977), whites

⁸ Heckman and Payner (1989) suggest that this scenario may be quite applicable to the textile industry in the South in the last century.

and blacks are assumed to be identical inputs on average, and there is individual-level variation in productivity that is unobserved by employers. Let q denote a worker's marginal productivity, with $w(q)$ the density of q for whites, and $b(q)$ the density for blacks. Employers observe a noisy signal of q , $y = q + u$, and employers know that $E(u) = 0$, $Cov(q, u) = 0$, $E(q) = \alpha$, and $Var(u) = \sigma^2$. Employers form $E(q | y)$, and pay wages based on this. If q and u are jointly normally distributed, then

$$E(q | y) = \alpha \frac{Var(u)}{Var(q) + Var(u)} + y \frac{Var(q)}{Var(q) + Var(u)}. \quad (3.6)$$

The second ratio is the “reliability” of the signal, denoted γ . Average black and white wages are equal if $\alpha_w = \alpha_b$, so the statistical discrimination model does not generate the key stylized fact of unexplained wage differences groups. However, the model can be extended to obtain this result.

If $\gamma_B < \gamma_W$, it may be harder for employers to optimally match black workers to jobs, resulting in lower productivity and wages ex post. Alternatively, the noisier signal for blacks may deter blacks from investing in human capital, since their wages will be more tightly clustered around α , lowering $E(q)$ and therefore wages (Lundberg and Startz, 1983); or initially erroneous beliefs about differences in α can become self-fulfilling (Coate and Loury, 1993). In the case of men and women, for example, suppose that employers predict tenure, in order to make decisions regarding specific human capital investment. If they initially assume that expected tenure is lower for women than for men, they may invest less in them, giving women less incentive to stay with the firm, which leads to employers' expectations being fulfilled, and lower wages.

The statistical discrimination model is not tied to prejudice. Rather, especially for the versions of the model that can generate wage differences, it is best viewed as a model of stereotyping based on assumed group averages. Thus, the model can be directly related to many lab experiments that attempt to measure stereotyping. The model may not imply differences in hiring if wages are free to adjust to expected productivity differences. If there are constraints on wage differences between groups, statistical discrimination should generate hiring differences that work against groups with less noisy signals, some of whom cannot be paid a wage as low as their expected productivity. Thus, if the only issue is that the signal

is noisier for blacks, we do not necessarily get the prediction of fewer job offers to seemingly identical blacks. However, if matching workers' abilities to the job is important, a noisier signal does have this implication.

The distinction between statistical discrimination and taste discrimination is important for policy. In response to taste discrimination, the most natural policy response is to raise the cost to the employer of engaging in discriminatory behavior, to, effectively, try to restore equal prices for equally productive labor from different groups. In contrast, policy interventions that increase information or the reliability of information about workers may reduce statistical discrimination.⁹ This is one reason experimental research tries to distinguish between these two broad models of discrimination.

3.3 Long-Term Incentive (Lazear) Contracts

Lazear's (1979) model of long-term incentive contracts (LTICs) provides an explanation for behavior that looks like age discrimination with regard to both terminations and hiring of older workers. In this model, incentive-compatible contracts entail paying young, low-tenured workers less than their marginal product, and older, high-tenured workers more than their marginal product. The model offers an explanation of mandatory retirement, when the discounted stream of wage payments catches up to the discounted stream of marginal productivity; mandatory retirement was viewed as discrimination by the ADEA, which abolished it over time. LTICs may also deter hiring of older workers, by imposing fixed costs that can be amortized only over a shorter period for older workers (Hutchens, 1986); barriers to paying newly-hired older workers much lower wages than current older workers to create the needed incentives can lead to the same result. Finally, LTICs can encourage employers to renege on the implicit contract, terminating higher-tenure workers when their wages exceed marginal product. Does the differential treatment of older workers predicted by this model represent discrimination? Clearly statistical discrimination with regard to how long workers will stay with the firm plays a role. Moreover, differential treatment based on the Lazear model has been interpreted as discrimination from a legal perspective

⁹ As an example, Holzer and Neumark (2000) suggest that affirmative action may have precisely this effect, encouraging more investment in scrutiny of minority workers both before and after hiring, and reduced reliance on the most-readily available signals.

(Issacharoff and Harris, 1997).

3.4 *Implicit Discrimination*

The final explanation of discrimination that is also tested in the experimental literature is “implicit discrimination.” The basic idea is that discriminatory decisions – making decisions based on race, for example, when race is not actually predictive of differences in productivity – are unconscious rather than conscious. Devine (1989) provides an overview of social psychology research on the difference between conscious efforts that may actively combat stereotypes that can lead to discriminatory behavior, and unconscious actions or decisions in which these stereotypes operate even among decision makers who are not prejudiced or who deliberately try to avoid stereotypes or prejudice. Bertrand et al. (2005) describe the Implicit Association Test (IAT), which claims to measure this kind of unconscious discrimination, and argue that scores on the IAT predict other behaviors that may translate into discriminatory behavior.¹⁰

If implicit discrimination is important in hiring and other decisions, then standard policy responses that operate on conscious behavior, such as reinforcing the illegality (and potential financial consequences) of discrimination, may not be very effective. In contrast, unconscious biases may be more amenable to policy interventions (including at the employer level) that reduce the role of unconscious decision-making – such as formalizing evaluation procedures, or things as simple as allowing more time to evaluate each candidate. Some experimental research tries to assess whether implicit discrimination generates discriminatory hiring differences.

4. *Non-experimental Approaches to Testing Hypotheses about Discrimination*

Economists have used a number of non-experimental approaches to test for and estimate the extent of labor market discrimination, and to try distinguish among the different models of discrimination. I highlight the key features of these approaches, some of which illuminate what experimental methods can provide.

¹⁰ The reader can take these tests at <https://implicit.harvard.edu/implicit/education.html> (viewed September 1, 2015). For a skeptical discussion of the validity of the IAT as a predictor of discriminatory behavior (or as a better predictor than explicit measures) – in many cases in laboratory studies – see Oswald et al. (2013).

4.1 Regression Decompositions

The traditional approach to studying labor market discrimination estimates wage regressions controlling for productivity-related differences between two groups, interpreting the remaining wage gap between the groups as an estimate of discrimination, most commonly using separate regressions to allow wage differences between groups to vary with other characteristics. Suppose we are studying blacks and whites, with data on their log wages (w) and characteristics (X). By the property of linear regressions,

$$\overline{\ln(w_W)} - \overline{\ln(w_B)} = \bar{X}_W \hat{\beta}_W - \bar{X}_B \hat{\beta}_B. \quad (4.1)$$

The $\hat{\beta}$ vectors are the empirical wage structures in the presence of discrimination. Denoting the wage structure in the absence of discrimination as β^* , we can rewrite the difference in mean log wages as

$$\bar{X}_W (\hat{\beta}_W - \beta^*) + \bar{X}_B (\beta^* - \hat{\beta}_B) + (\bar{X}_W - \bar{X}_B) \beta^*. \quad (4.2)$$

The first two terms capture wage discrimination, with the first interpreted as the advantage whites get from discrimination in the wage structure, and the second as the disadvantage that blacks suffer (if they do); their sum is the “unexplained” wage gap. The third term is the gap due to differences in productivity-related characteristics. Oaxaca (1973) alternatively used $\hat{\beta}_B$ and $\hat{\beta}_W$ as β^* , which can be justified in terms of the Becker employer discrimination model, but need not be correct (Neumark, 1988).

There are a number of problems with this approach. First, the unexplained wage gap could be due to unobserved productivity-related characteristics rather than discrimination. Second, differences in other regression coefficients may not reflect discrimination; for example, differences in the coefficient on experience can reflect human capital investment decisions (Mincer and Polachek, 1974). Third, differences in the X 's can reflect responses to discrimination, such as lower experience and tenure of women because of discrimination on the job (e.g., Gronau, 1988) – sometimes termed “feedback” effects. For all of these reasons, academic research has moved away from the regression approach to testing for discrimination.

4.2 Production Function Tests to Compare Marginal Productivity and Wage Differentials

More recently, labor economists have been drawn to the possibility of being able to estimate

¹¹ Oaxaca (1973) shows why the difference in the logs of the geometric means can be interpreted as the economic magnitude of interest.

productivity differences directly, and to compare these differences to wage differences. In Hellerstein and Neumark (1999), we developed a method of using matched employer-employee data to do this. To motivate the approach, consider an economy with plants that produce output Y (with prices normalized to one), using two types of perfectly substitutable labor inputs, L_1 and L_2 , with wages w_1 and w_2 , defining $\lambda = (w_2/w_1)$. The production function of these plants is

$$Y = f(L_1 + \varphi L_2) \tag{4.3}$$

where φ is the marginal productivity of L_2 relative to L_1 . The plants are assumed to operate in perfectly competitive spot labor markets, with completely inelastic labor supply. The proportional mix of the two types of labor in each plant will be determined by the relationship between φ and λ . If $\varphi = \lambda$, then under profit maximization or cost minimization plants will be indifferent to the mix of the two types of labor in the plant. If there is a wedge between the relative marginal product and relative wage so that $\varphi \neq \lambda$, then profit-maximizing or cost-minimizing plants will be at a corner solution, hiring either only workers of type L_1 (if $\varphi < \lambda$) or only workers of type L_2 (if $\varphi > \lambda$).

The only equilibrium is when wages adjust so that $\varphi = \lambda$, and plants are indifferent between the two types of labor. Evidence that $\varphi \neq \lambda$ is therefore inconsistent with profit maximizing or cost-minimizing plants in a competitive spot labor market, and for demographic groups, a prime alternative explanation is discrimination. The Becker employer discrimination model would predict exactly this result. And if the disutility from discrimination depends on the ratio of L_1 to L_2 , then continuous variation in the composition of the workforce can be exogenously driven by variation in tastes. There can still be plant- or firm-level unobservables correlated with race, sex, etc. But as long as we estimate the production function and earnings equation at the plant or firm level, under the null hypothesis of no discrimination any biases from these unobservables should affect the estimated productivity and wage differentials similarly.

As an example, Hellerstein et al. (1999) use U.S. data, and focus on male-female differences. We jointly estimate plant-level production functions and wage equations to estimate parameters corresponding to φ and λ for various types of workers, and find a sex gap in wages that significantly exceeds the sex gap

in marginal productivity, consistent with discrimination.¹² This structural approach to estimating wage discrimination has now been used in a number of countries where matched data have become available, and enhancements to the approach have been developed (Bartolucci, 2014).

4.3 *Using Information on Discriminatory Tastes*

Charles and Guryan (2008) incorporate survey evidence on discriminatory attitudes towards blacks to test implications of the Becker model of employer discrimination for the black-white wage gap. In this model, when the relative supply of blacks is small enough given the number of non-discriminatory employers that in equilibrium there is no wage gap, the marginal employer has $d = 0$. With higher relative supply, holding the distribution of d constant, a wage gap should emerge as the marginal employer can become a discriminator. And when the relative supply of blacks is high enough that the marginal employer discriminates, then the wage gap should be larger the stronger are discriminatory tastes; but the strength of these tastes should not matter when the marginal employer is not discriminatory.

Using state-level data from the General Social Survey, Charles and Guryan find evidence consistent with these predictions. There is no effect of the average or 90th percentile of the prejudice distribution on relative wages of blacks, but there is a negative effect of tastes at the marginal employer or the 10th percentile (which are both closer to the share black).¹³ And conditional on the distribution of discriminatory tastes, the share of the workforce that is black lowers relative wages of blacks.

Kuhn and Shen (2013) and Hellester et al. (2014) capture the direct discriminatory preferences of employers who are hiring. They study internet job boards in China and Mexico, which post job descriptions that often include preferences regarding gender and age. The mere stating of these preferences indicates discrimination, but these papers do more to try to understand what might drive these preferences.

Kuhn and Shen (2013) suggest that the pattern of advertised gender preferences can be better

¹² The use of plant-level data means that identification of φ and λ comes from across-plant variation that does not necessarily reflect within-plant variation in productivity or wages. There are no individual-level productivity measures. But for wages, we also have individual-level data on workers matched to plants and verify that the across-plant variation largely reflects within-plant variation owing to variation in the share black, female, etc. In particular, estimated wage differentials using the individual-level data are not very sensitive to the inclusion of plant-level fixed effects.

¹³ To use the survey data on discriminatory tastes to approximate the marginal employer, they assume that firms are all the same size, so that if p is the percentage of blacks in the workforce, then the marginal employer is at the p th percentile of the distribution of discriminatory tastes, because all the blacks can be employed in p percent of the firms.

explained by screening costs than by variation in discriminatory tastes alone. They find that roughly equal numbers of ads express a preference for men or women, and that these preferences can switch for different jobs at the same firm. Moreover, stated sex preferences decline for higher-skilled jobs, for which there are likely to be fewer applicants and hence lower screening costs, and for which it may be more important to find the most-qualified applicant.

Hellester et al. (2014) find similar results for age preferences. They also find a “twist” in employers’ gender preferences, which shift away from women and towards men as (preferred) age rises from 18 to 45. One potential explanation, for which they find some evidence based on additional information on stated preferences for visually attractive candidates and – in the Mexican data – requests for photos, as well as based on whether the occupation requires customer contact, is that looks are valued more highly among younger than among older women. In contrast, leadership, and preferences regarding marital status of workers, appear to drive up the relative preference for men as age rises.

4.4 Tests of Statistical Discrimination

A few studies test for statistical discrimination, sometimes also trying to distinguish between statistical and taste discrimination. These studies illustrate the strong informational assumptions required, regarding what is observed by firms but potentially correlated with unobserved productivity, how the information available to firms changes over time, and what is potentially known to the researcher but not the firm. This challenge carries over to experimental research, although it has not been recognized as explicitly.

Altonji and Pierret (2001) model the evolution of wages as workers gain experience to test for statistical discrimination based on race. Their evidence indicates a negligible black-white difference in wages when experience is zero, but a black-white gap that increases with experience. However, when they include the AFQT and its interaction with experience, the negative black-experience interaction diminishes. The authors interpret this evidence as inconsistent with statistical discrimination based on race. Instead, they suggest, blacks are less productive, but firms comply with the law, so that absent individual-level information on productivity firms do not impute lower productivity to blacks when they are first hired. But

over time, they obtain information that they can legally use to pay black workers less, which is why the black penalty grows with experience, and also why this effect diminishes when the AFQT-experience interaction (capturing information employers obtain over time) is included. Note that this interpretation hinges critically on assumptions about what information employers have available, and when.

Foster and Rosenzweig's (1993) work testing statistical versus taste discrimination also illustrates the importance of assumptions about information. They have data from the Philippines that includes demographic characteristics and wages paid for time-rate and piece-rate work. Given that piece-rate work should capture differences in productivity, we might test for taste discrimination against women by regressing time-rates (w) on a piece-rate measure of productivity (μ), and a dummy variable for women (F):

$$w = \alpha + \beta\mu + \gamma F + \varepsilon, \quad (4.4)$$

and indeed their data show that w is lower for women ($\hat{\gamma} < 0$), conditional on μ .

However, if employers do not know individual-level productivity when they set time rates, but just that μ is lower on average for women than for men, then the estimated coefficient on μ is biased towards zero, and the average difference can load onto the female dummy variable, making statistical discrimination look like taste discrimination. If we could measure expected productivity, $E(\mu)$, then if there is no taste discrimination but only statistical discrimination, the coefficient on $E(\mu)$ should equal one, and the coefficient on F should equal zero. But if we still find a negative coefficient on F , conditional on $E(\mu)$, then this would point to taste discrimination. Since there is no proxy for $E(\mu)$, Foster and Rosenzweig instead instrument for μ , assuming that μ is the sum of $E(\mu)$ and a random forecast error. Valid instruments are variables that employers use in forecasting μ , but that do not affect wage directly – precluding, for example, taste discrimination based on these variables. The authors use height, age, and education. The resulting IV estimates do not show any evidence of lower wages paid to women conditional on $E(\mu)$, consistent with all of the initial difference in the OLS estimates being attributable to statistical discrimination.¹⁴

¹⁴ I used a U.S. dataset with measures of starting wages, current wages, and employers' current performance rating of workers to test statistical versus taste discrimination as explanations of race and sex differences in starting wages (Neumark, 2002). This imposes similar requirements for assumptions about information as in Foster and Rosenzweig,

Finlay (2007) studies statistical discrimination in hiring from groups (such as blacks) more likely to have a criminal background. Rather than relying on strong assumptions to test for statistical discrimination, he captures a change in information available about workers. Specifically, he estimates how the advent of criminal background checks may have enabled employers to better distinguish among individual blacks, reducing statistical discrimination against blacks without a criminal record.

5. *Legal Issues*

A great deal has been written about anti-discrimination laws, and their potential effects on labor market outcomes for protected groups of workers as well as other groups. Here, I discuss issues relevant to motivating, understanding, or interpreting experimental studies of discrimination – and in particular field experiments – which focus on hiring discrimination.

5.1 *Overview of Laws, Enforcement, and Case Law*

Title VII of the Civil Rights Act prohibits discrimination in hiring and other conditions of employment, on the basis of race, ethnicity, religion, and sex, whereas the ADEA protects older workers and the Americans with Disabilities Act (ADA) protects disabled workers.¹⁵ Anti-discrimination laws are enforced by the EEOC and the courts. A party wishing to pursue a discrimination claim must first file a charge with the EEOC (or, in states with parallel discrimination statutes, at the state level). The EEOC may dismiss the charge if it does not think there was a violation of law, in which case the complainant may still pursue a civil action in court, and sometimes the EEOC files suit. Among other remedies, pecuniary damages can be awarded depending on the law and on conditions required for awarding them.

Case law plays a strong role in determining the types of cases that may be brought and the burden of proof. The plaintiff's burden is to prove that the action of the employer was taken on the basis of a

but I also had additional information on subsets of workers (identified as probationary) for whom statistical discrimination was more likely.

¹⁵ I not aware of any legal definition of discrimination based on “lookism,” perhaps because looks are partly endogenous. In partial contrast, the EEOC has suggested that morbid obesity can be considered a disability entitled to protection under the Americans with Disability Act (ADA) (see <http://www.bna.com/eec-commissioner-feldblum-n17179876987/>, viewed August 18, 2015).

U.S. states also have laws that cover the same protected groups – sometimes with stronger provisions – and define other protected groups based on, for example, marital status, sexual orientation, and even weight (in Michigan). European Union law largely defines the same protected groups as U.S. law (including, however, sexual orientation). I think it is fair to say that workforce protections are stronger in the United States, in part because of class actions.

protected category (e.g., race), which does not require that race was the sole factor, but was the determining factor. This can be proven in one of two ways. The first is to prove “disparate treatment” – that an employer intentionally treated someone less favorably because of the protected group status, such as posting an ad stating a preference for young applicants. If plaintiffs cannot establish direct evidence of intent to discriminate, the plaintiff can try to establish a prima facie case for discriminatory intent (which may rely in part on statistical evidence), ruling out the most likely non-discriminatory explanations of the action. The burden of proof then shifts to the employer to offer a legitimate non-discriminatory explanation. Finally, the plaintiff can rebut the employer’s explanation, most commonly by trying to prove that the non-discriminatory explanation is false. Alternatively, plaintiffs can try to prove “disparate impact.” Rather than proving discriminatory intent, such cases require that an employer’s policy that may appear neutral in fact impacts a protected group adversely, and that the practice cannot be justified by “business necessity.”¹⁶

Disparate impact cases generally rely on statistical evidence, in part because such cases have to establish differences in how the practices in question impacted different groups, and in part because it is not necessary to prove discriminatory intent. As a result of establishing patterns of behavior affecting many workers at a firm, disparate impact cases sometimes support class action lawsuits, where a suit on behalf of a large number of plaintiffs is tried on the basis of the merits of the case for a small number of plaintiffs. In such cases, the damages can be very large.

5.2 Anti-Discrimination Laws and Hiring

We do not expect anti-discrimination laws to root out all discrimination. Nonetheless, these laws may be quite ineffective – or worse – when it comes to discrimination in hiring. Enforcement of anti-discrimination laws in the United States relies on the legal process, and hence on potential rewards to plaintiffs’ attorneys. In hiring cases it is difficult to identify a class of affected workers, inhibiting class action suits and thus substantially limiting awards. In addition, economic damages can be small in hiring

¹⁶ An instructive example is provided by the relationship between age and fitness requirements for jobs. These requirements may appear neutral because they are not based on age, but in many cases are apt to disadvantage disproportionately older workers or job applicants. Courts have generally found them allowable only if they are absolutely necessary for the specific tasks to be performed (Piette, 1995).

cases because one employer's action may extend a worker's spell of unemployment only modestly. (Terminations, in contrast, can entail substantial lost earnings and benefits, and, for older workers, significant pension accruals.) And it could be worse: If anti-discrimination laws fail to reduce discrimination in hiring but make it harder to terminate protected workers, these laws could actually deter hiring of protected workers by imposing a higher termination cost (Bloch, 1994; Posner, 1995).

As a result, it is possible that evidence of lower hiring from protected groups does not reflect discrimination but instead – perversely – efforts to combat discrimination. I regard it as unlikely that the overwhelming evidence pointing to hiring discrimination from the field experiments I review is attributable to higher transaction costs stemming from anti-discrimination laws. First, much of the evidence arises even in settings where discrimination laws are weaker or even not applicable, and hence are unlikely to impose termination costs that could alter hiring behavior. Moreover, laboratory experiment evidence of discrimination (which I find less convincing generally) has the advantage that it is not driven by real employers who might be responding to the unintentional incentives that could, in principle, be created by anti-discrimination laws. That said, the evidence on whether anti-discrimination laws increase hiring of protected groups is scant, and mixed. The difficulty of rooting out hiring discrimination using anti-discrimination laws, and the prospect that these laws may increase hiring discrimination in some circumstances,¹⁷ help motivate interest in testing for discrimination in hiring.

5.3 Defining Discrimination

A couple of issues arise with regard to the relationship between legal definitions of discrimination and how labor economists think about discrimination. First, although economists are interested in distinguishing between statistical and taste discrimination, both are illegal under U.S. law. The Code of Federal Regulations (29, § 1604.2) defines as illegal discrimination “The refusal to hire an individual because of the preferences of coworkers, the employer, clients or customers ...” But it also states “The principle of nondiscrimination requires that individuals be considered on the basis of individual capacities

¹⁷ For example, Neumark and Button (2014) find that, in normal economic circumstances, state age discrimination laws that are stronger than the ADEA appear to boost hiring of older workers, but during and after the Great Recession they may have had the opposite effect – perhaps because of uncertain future labor demand coupled with the higher termination costs that stronger state laws impose.

and not on the basis of any characteristics generally attributed to the group.”¹⁸

Second, in the case of age discrimination and disability discrimination, the law is potentially or explicitly more nuanced than simply mandating equal treatment of otherwise identical workers, which can have implications for both the design and the interpretation of evidence from field experiments. Age discrimination laws allow for some ways in which age can legitimately affect labor market outcomes (Neumark, 2003). And disability discrimination laws, at least as they have been interpreted, do not require exactly equal treatment.¹⁹ Most importantly, employers are required to provide “reasonable accommodation” for disabled workers, thus explicitly allowing for the possibility that hiring a disabled worker will entail higher costs. Given that we do not know *ex ante* what would be deemed a reasonable accommodation, it can be tricky to interpret evidence of differential treatment of the disabled in a field experiment as discrimination. For example, it is possible that the disability “conveyed” in the study would have entailed accommodation viewed as beyond reasonable, although Ameri et al. (2015) discuss evidence that most accommodations are inexpensive.

5.4 *Standing*

Finally, although it does not directly bear on the evidence from field experiments, it is worth noting that U.S. courts allow organizations that conduct audit or correspondence studies to file claims of discrimination based on the evidence they collect. In support of these efforts with regard to both labor market and housing discrimination, the EEOC refers to case law from the early civil rights movement, when groups’ motivation was only to test the law (U.S. Equal Employment Opportunity Commission, 1996).²⁰ The EEOC discusses the well-established standing of testers in housing discrimination cases

¹⁸ I have not found as explicit a prohibition of statistical discrimination in the European Union (EU). Article 2 of the EU’s Directive 2000/43/EC prohibits both “direct” and “indirect” discrimination, but these appear to line up, respectively, with disparate treatment and disparate impact in the U.S. context (see <http://eur-lex.europa.eu/legal-content/en/ALL/?uri=CELEX%3A32000L0043>, viewed December 2, 2015). However, other material suggests that the prohibition on statistical discrimination is covered by direct discrimination (OECD, 2013, p. 195).

¹⁹ See <http://www.eeoc.gov/policy/docs/accommodation.html> (viewed November 1, 2015). Burgdorf (1997) discusses the extent to which enforcement agencies and the courts have interpreted and implemented disability discrimination law in a manner that gives special treatment to the disabled, rather than simply prohibiting discriminatory treatment.

²⁰ For example, the EEOC cites *Pierson v. Ray* (1967), in which the Supreme Court “held that a group of Black clergymen who were removed from a segregated bus terminal in Jackson, Mississippi had standing to seek redress,” ruling that the plaintiffs “had been discriminated against by being ejected from the terminal, despite the fact that the plaintiffs’ sole purpose was to test the law rather than to actually use the terminal” (U.S. EEOC, 1996, p. 2).

under Title VIII, based on *Havens Realty Corp. v. Coleman* (1982), the parallels to employment discrimination under Title VII, and cases in which damages have been awarded in employment testing. The EEOC also references *Fair Employment Council of Greater Washington, DC v. BMC Marketing Corporation* (1994), in which standing was limited because individual testers could not show future harm, as well as the implications of Civil Rights Act of 1991, which allows for damages; according to the EEOC, the 1991 act would have given the testers in this case standing had the testing occurred after its passage. Consistent with these rulings, there are organizations that conduct audit or correspondence studies of discrimination, and file charges seeking damages.²¹

6. *Laboratory Experiments*

Lab experiments have been used to test for discriminatory decision making, to try to understand the nature of discrimination, and to study behaviors that can lead to discriminatory outcomes. There a broader research literature on laboratory experiments that relates to discrimination in exploring alternative explanations of group differences in outcomes, like recent work on gender and competitiveness (e.g., Gneezy et al., 2003). There is also an extensive psychology literature measuring stereotypes about different groups (e.g., Kite et al., 2005). I restrict attention to research that specifically addresses whether outcomes differ because of how a group is treated in a laboratory setting meant to mimic potential labor market discrimination. Thus, I exclude research on gender and competitiveness, and the general literature on gender and bargaining except where it tries to address labor market outcomes directly. And I cover research on stereotypes only to the extent that this research ties the stereotypes participants hold to actions that could plausibly be tied to labor market outcomes. My goal is not cover every study, but to illustrate the kinds of studies that have been done, to discuss the issues that arise, and to identify important questions. And where survey articles are available (with respect to field experiments as well) I reference and discuss them, rather than going back to the individual studies, and offer my perspectives on the larger literature

²¹ As recent examples, “Freedom to Work” conducted a correspondence study of hiring discrimination against LGBT candidates at Exxon Mobil, and filed a charge with the Illinois Department of Human Rights (http://www.freedomtowork.org/?page_id=483, viewed November 2, 2015); and the Equal Rights Center files numerous complaints based on evidence from tests for discrimination in housing (see http://www.equalrightscenter.org/site/DocServer/2013_Annual_Report.pdf?docID=2521, viewed November 2, 2015).

they cover.

There are standard objections regarding laboratory versus field experiments, mostly having to do with external validity, and capturing realistic stakes for participants.²² The latter issue may arise in a particularly complex way in research on discrimination, because the market-level effects of competition may be the driving force that imposes costs for engaging in discriminatory behavior, implying that it may be hard to mimic the costs of discrimination in the laboratory. In fact, though, a good deal of the existing research does even less to incentivize behavior, imposing no costs on participants to make discriminatory choices in selection of employees in hypothetical scenarios.

6.1 *Simulated Personnel Decisions (Vignette Studies)*

Vignette studies seek to study discrimination in personnel decisions made by employers or managers. They present participants with hypothetical scenarios regarding selecting job candidates for hiring, or selecting employees for training, promotion, etc., and often ask about attitudes towards these workers or other supplemental information that might help explain their decisions. As in the field experiments discussed below, protected-group status (age, sex, etc.) is manipulated in these studies for otherwise identical job candidates, which is what makes vignette studies experiments.

It is easy to criticize vignette studies for dealing in hypotheticals. However, for personnel decisions regarding existing workers (rather than new hires), it is difficult to conceive of a corresponding field experiment, such as selecting otherwise identical employees for training or promotion decisions. The only exception would be, perhaps, an experiment run internally at a company that was sufficiently large to be able to present fictitious workers to other managers.²³ The closest laboratory experiments have gotten to this is to administer vignette studies to managers likely to be actively involved in making similar decisions, such as managers participating in training programs. To a large extent, vignette studies simply test for discrimination in decision making, without attempting to determine the nature of discrimination. However, some of the studies present evidence that could be viewed as testing statistical versus taste discrimination.

²² On the second issue, see, e.g., Niederle and Vesterlund (2007) and Flory et al. (forthcoming).

²³ Baert et al. (forthcoming) try to study promotions in the context of a correspondence study field experiment, but this entails hiring at new employers into jobs at a higher level than the present one, which is different from internal promotions.

There are scores of vignette studies in the literature. Here I touch on some that are cited frequently and that appear to be most closely related to hypotheses that arise in the economics of discrimination.²⁴

6.1.1 *Sex*

An early vignette study (then called an “in-basket exercise”) was administered to male bank managers participating in a management training program (Rosen and Jerdee, 1974). The managers were given hypothetical scenarios requiring selecting bank employees for promotion or development, and the solution of a supervisory problem. The study found significantly lower willingness to promote females, whether into complex or simple jobs, and significantly lower selection of females for development (attending a professional training conference). In supervisory problems, a lengthy report from a supervisor described a problem and requested that the employee be terminated or at least transferred; the choices of participants ranged from terminating the employee, to arranging a transfer or “softer” solutions. Termination was more likely to be recommended when the supervisor was male and the workplace issue concerned performance (with the evidence less clear when the issue was personality). All of this evidence is consistent with sex discrimination in decisions that are related to key labor market outcomes. The authors interpret the evidence as confirming the importance of sex stereotypes in this discrimination, but the basis for this conclusion is not clear.

Researchers have considered the external validity of these vignette studies, albeit in the limited sense of asking whether outcomes using professionals as subjects instead of students yield different results. In an early meta-analysis, Olian et al. (1988) found a similar (negative) effect size of female for studies using the two kinds of subjects, although for some reason a higher variance of effect sizes for professionals. This may partially mitigate concerns that outcomes for student subjects are not reflective of real-world decision making, although what professionals do in the lab versus the real world remains an open question.

The studies reviewed in this section are dated, and these types of studies appear to be less common in more recent literature. It is possible, of course, that sex discrimination overall has declined enough that

²⁴ For example, Singer and Sewell (1989) discuss many studies regarding selection decisions based on age, which follow up on the studies I discuss below. However, many of these studies pertain to cognitive issues (the impact of other information calling attention to age) or sociological issues (the role of differences in the status of jobs considered).

the issue is regarded as less important. Nonetheless, more recent experimental research has explored questions that intersect with gender. For example, Correll et al. (2007) conduct a laboratory experiment and a field experiment (discussed later) of the motherhood penalty, motivated by evidence they cite that much of the unexplained contemporaneous sex gap in wages is attributable to motherhood. In the lab experiment, undergraduate subjects evaluate same-sex pairs of married applicants who differ in whether they are parents. For the female pairs, ratings of competence and commitment are significantly lower for mothers, as are recommendations for hiring (47 percent versus 84 percent, significant at the 5-percent level) and recommended salaries. For the male pairs, the recommended hiring rate is a bit higher for fathers (73 versus 62 percent, significant at the 10-percent level), as is recommended salary. The difference in evaluations of competence and commitment account for about 40 percent of the hiring differential within female pairs.²⁵ Finally, there was no evidence of lower ratings or selection for hiring between non-parent men and women (based on between-pair comparisons).

6.1.2 *Ethnicity*

More recently, vignette studies have been applied to ethnic discrimination, coupled with additional evidence on potential determinants or at least correlates of discrimination. Blommaert et al. (2014) present student participants in the Netherlands with resumes signaling Moroccan, Turkish, or Dutch ethnicity via name and parents' country of birth (all applicants were born in the Netherlands), as well as sex, education, and experience. Participants rated applicants (from zero to 10) on suitability for the job, and selected three they would invite for an interview. Minority applicants received lower suitability ratings, although the effect was very small (0.05 of the zero-10 ranking).²⁶ For selection for interview, the odds ratio for selecting native Dutch applicants was 1.14, marginally significantly different from one.

Participants were surveyed on personal and background characteristics. Indeed, the authors argue that this ability to interview/survey experiment participants is an advantage of laboratory versus field experiments of discrimination – although later I will discuss field experiments that try to collect

²⁵ To try to incentivize participants, the subjects were told that a real communications technology company was soliciting their views on hiring a marketing head since young people are heavy consumers of the company's product.

²⁶ This difference is reported as statistically significant, which I suspect is because the standard errors are not clustered at the participant level; there were 272 participants who evaluated 8,704 resumes.

information from recruiters. Blommaert et al. find less discriminatory behavior in selection for interviews by those who report more positive inter-ethnic contacts (higher quality, not higher frequency), as well as those with higher education. The first result might provide some support for a taste-based understanding of ethnic discrimination in this experiment, although there is no way to know whether positive inter-ethnic contacts reduce discriminatory tastes, or are simply correlated with weaker discriminatory tastes.

6.1.3 Age

Rosen and Jerdee (1977) conducted a vignette study of age discrimination similar to their earlier sex discrimination study, based on a survey administered to *Harvard Business Review* subscribers. They provided hypothetical scenarios and asked subscribers how they would respond to managerial complaints about workers, decisions about career development, and promotions. Age of the worker in question was manipulated (32 or 61). In response to complaints about a worker, respondents indicated greater difficulty in changing the behavior of older workers, and were more likely to recommend that the work be reassigned from an older worker than from a younger worker, and conversely more likely to recommend an encouraging talk with a younger worker. In response to a request for a career development opportunity regarding production technology, older workers were viewed as less likely to want to keep up with technology, and the decision was more likely to be approved for a younger worker. And in response to promotion to a position requiring creativity and innovation, older workers were viewed as less likely to succeed, and were less likely to be recommended for promotion.

Similar results are reported in Rosen and Jerdee (1976), using as subjects, instead, undergraduate business students. The similarity of results suggests that, at least for business students, findings are externally valid. However, 28 years later, a replication of this study by Weiss and Maurer (2004) found relatively little evidence of negative stereotypes or adverse hypothetical decisions for older workers. This could be attributable to more older workers in the workforce, and changes in attitudes and behaviors over three decades. It would be preferable to know about changed attitudes and behavior among subjects like subscribers to the *Harvard Business Review* (as in Rosen and Jerdee, 1977). Although these papers provide suggestive evidence that age-related stereotypes are likely to affect managerial decisions, the analyses do

not tie individual responses regarding stereotypes to the recommendations of those respondents.²⁷

6.1.4 *Looks and Obesity*

Hosoda et al. (2003) provide a meta-analysis of laboratory studies of discrimination based on looks or “attractiveness” and selection for promotion, performance evaluation, hiring, and other outcomes. Attractiveness is typically manipulated by including a picture, although there are differences, such as having subjects rate mock videotaped job interviews (Riggio and Throckmorton, 2006). Hosoda et al. conclude that “Physical attractiveness is always an asset for individuals” (p. 451). The effect size is large (although a bit hard to interpret), and the effect does not appear to vary with the sex of the applicant, the sex type of the job, the provision of job-relevant information, or whether subjects were students or professionals. However, the effect size declines over time (across studies), and is largest for choice of a business partner and smallest for performance evaluation.

Rudolph et al. (2009) provide a similar type of meta-analysis of studies on discrimination against obese job applicants. The authors note obesity can be associated with unattractiveness, moral or emotional impairment, unhappiness, higher absenteeism, and lower acceptance by co-workers, emphasizing the issue of whether studies capture differences in discrimination or productivity. The manipulation of weight is sometimes done using computer morphing programs (Polinko and Popovich, 2001) or theatrical prostheses (Pangitore et al., 1994) to make the same “applicant” appear as both average weight and obese. The evidence from this meta-analysis points to adverse effects for evaluation and selection for hiring.

6.2 *Statistical vs. Taste Discrimination*

Some laboratory studies try to estimate whether providing additional information about applicants reduces differences in selection between groups. Such evidence might be interpreted as testing between statistical and taste discrimination, although this interpretation is made much more explicit in field experiments taking this approach, and hence gets more attention when I discuss field experiments.

Heilman (1984) presents evidence on the effects of information about applicants on selection of

²⁷ Krings et al. (2011) study selection by age and stereotypes regarding age and “warmth” and competence (older workers are rated higher on the former, and lower on the latter). The authors found lower selection of older workers, and that this bias was mediated by age-related stereotypes regarding competence.

women versus men. She documents inconsistent findings in vignette studies regarding whether providing additional information about individual applicants reduces adverse outcomes for women. Heilman suggests that because sex is a salient characteristic for evaluators, additional job relevant information will reduce the role of sex, but information not relevant to the job can worsen outcomes for women by increasing the salience of sex. This is tested in an experiment on selection of a recent college graduate for a managerial position. The “job-relevant” information treatment is success in courses in economics and business. For the “low-relevance” treatment, similar success is reported in biology and political science. (In the third treatment, neither type of information is given.) The results are consistent with her conjectures. In particular, only the provision of job-relevant information eliminates the lower selection of women, potentially consistent with statistical discrimination.

The role of job-relevant information in reducing group differences is considered in three meta-analyses. Tosi and Einbender (1985) report that, *across* studies, sex differences in selection and other outcomes are smaller when subjects are given more information about applicants. Davison and Burke (2000) focus on ratings of both sexes for “opposite-sex” jobs and similarly report that lower ratings for opposite-sex jobs are diminished when more information is provided. The evidence that information affects ratings of both males and females in this way suggests that the findings may reflect the reduced salience of sex rather than statistical discrimination per se (which we might expect to disadvantage women). In contrast, the Hosoda et al. (2003) study also reports across-study results, finding that providing more job-relevant results about applicants does not influence group differences in outcomes based on attractiveness.

Three points can be made about these studies. First, we might expect less statistical discrimination based on looks, since it not clear what unobserved productivity differences employers might associate with attractiveness. Second, it is hard to know how to interpret evidence that more information fails to reduces group discrimination (as in Hosoda et al., but more generally), since we do not know whether the information provided pertains to qualifications about which employers make assumptions based on group averages when discriminating statistically; this is a general problem with testing taste versus statistical

discrimination to which I return later. Finally, it is clearly more desirable to study variation within than across studies, to isolate the effects of the provision of information.

A related problem of thinking about the role of information in interpreting studies like these arises in Baert and De Pauw (2014), who conduct a laboratory experiment on discrimination in Belgium against applicants with Turkish (versus Flemish) names, using students as subjects. They find that respondents were likely to rate minority applicants as having the required productivity for the job (indeed, more likely than non-minorities), although respondents rate the minority group population overall as less qualified. The authors interpret this as inconsistent with statistical discrimination, because the overall lower group average for the minority group is not reflected in evaluation of the candidates. However, the experimental design presents native and minority candidates with identical resumes, so subjects may have enough information that they do not impute the group level averages to the applicants they assess. That is, thinking back to the Aigner and Cain (1977) framework, we have no way of knowing whether subjects perceived the signals of applicant quality as sufficiently noisy that they would put a lot of weight on group averages of the applicant populations. As a result, it is hard to see how the evidence can be informative about statistical discrimination. Moreover, since the study's results provide no evidence of (hypothetical) employer discrimination in the first place (there is no difference in selection for hiring), it is hard to interpret this study as testing alternative explanations of such discrimination.

In my view, however, the external validity problem is particularly severe with regard to these kinds of tests for statistical discrimination. To infer something about real-world statistical discrimination from whether providing additional information on hypothetical applicants affects group differences in selection requires assuming that the laboratory conditions regarding information actually mimic real-world hiring. In the case of Heilman's analysis, for example, it seems unlikely that employers *would not* know field of study to begin with, so the experimental treatment of no information about coursework and how the outcome changes when this information is provided may not provide evidence on how employers' actual decisions are changed by the provision of information that might reduce sex stereotypes.

In contrast, Mobius and Rosenblat (2006) conduct an experimental study of the "beauty premium"

that more successfully distinguishes among explanations of this premium, including taste discrimination and statistical discrimination. The experiment involves solving mazes, for which there is no difference in ability based on looks. “Workers” have to estimate how many mazes they can solve, and “employers” have to estimate how many mazes workers can solve (both types are incentivized to estimate correctly). Each worker is matched with five different employers, and the interaction is manipulated to include no visual or oral (by telephone) information, one type information or the other, both types, or face-to-face interaction. Wages paid to workers are based on some of the employer estimates, and come out of employers’ endowments. Payments are arranged so that the employer first evaluates or interacts with the worker, and is then told which estimates will contribute to a worker’s earnings. This provides evidence on taste discrimination, since an employer might sacrifice earnings to pay a higher wage to an attractive worker.

The authors decompose the beauty premium into a number of sources. They find that one contributor is overconfidence of better-looking workers, and another is overestimation of ability of better-looking workers by employers.²⁸ The latter is a form of statistical discrimination, albeit based on incorrect stereotypes because better-looking people are in fact no more productive in their setting. There is no evidence of taste discrimination, although the authors caution that this kind of result may not carry over to real-world labor markets, where the utility gain from interacting with a good-looking person over an extended period may matter much more than in the lab – a question, again, of realistic stakes for participants.

6.3 Tests for Implicit Discrimination

There has been little or no work trying to test whether implicit discrimination can account for group differences in labor-market-related outcomes in laboratory experiments.²⁹ Rosen and Jerdee (1977) found that although respondents exhibited age discrimination in decisions and held age stereotypes, a large share favored policy reforms that would help older workers, such as complete vesting of pensions and the elimination of mandatory retirement. They suggest that these two types of responses/behavior are

²⁸ Less related to this paper, they also find that better-looking workers have better oral skills that raise their wages, controlling for confidence.

²⁹ Dovidio et al. (2002) survey experimental psychology research on implicit discrimination.

discrepant, and could imply that respondents in the role of managers do not consciously discriminate, but respond to unconscious stereotypes – which sounds like an early appeal to implicit bias. However, this inference may not be supported. First, not all respondents supported policies to help older workers (60-80 percent did), and the others may have driven the age differences in personnel decisions and stereotypes. Second, the responses concern policy reforms that are not closely related to the personnel decisions respondents were asked to make.

6.4 *Discrimination in Bargaining Experiments*

There is experimental research on sex differences in bargaining outcomes (e.g., Eckel, 2008; Solnick, 2001), which of course could influence pay. Some of the work on bargaining also pertains to taste versus statistical discrimination (Fershtman and Gneezy, 2001). Here, though, I restrict the discussion to a bargaining experiment that more explicitly tries to capture features of labor markets.³⁰

Dittrich et al. (2014) study multi-stage, alternative-offer bargaining, which they believe is more likely to generalize to labor market outcomes than simple bargaining experiments, and they explore differences depending on whether males and females are playing the role of employees or employers. Their experiment is framed as a firm bargaining with a worker. The firm needs a task done, for which it earns a fixed amount, and proposes a wage to the worker, the remainder from which is the firm's earnings. The worker has an outside option of zero, and the employer has a positive outside option that is less than its total earnings if it reaches an agreement. An initial offer is made, alternatively by the employer or the employee, and then there are rounds of counteroffers. After the third round, if there is no agreement reached there is an exogenously imposed probability of the negotiations ending, in which case each gets their outside option. Pairings can be either mixed- or same-gender, and those bargaining sit across from each other but communicate only via a fixed script.

³⁰ There are also field experiments on bargaining, but perhaps not surprisingly given the needs of this research, these experiments focus on consumer transactions. Examples include sex differences in bargaining over taxi fares in Peru (Castillo et al., 2013), race, age, and sex differences in bargaining over sports cards (List, 2004), and sex and race differences in bargaining over car prices (Ayres and Siegelman, 1995). These studies do not pertain to the labor market, so I do not cover them. But it is interesting to note that they often point to statistical discrimination as the source of differences in outcomes (e.g., assumptions about the valuation of a taxi ride in the Castillo et al. study). One might suspect that taste-based discrimination would be more important in labor markets because employment entails a long-term relationship.

There are two key findings. First, males acting as employees negotiate higher wages than females (but as employers, there are not differences in outcomes). Second, male employers make higher initial wage offers to male employees than female employees, whereas female employers do not behave in this way. The authors interpret this finding as wage discrimination. Third, the differences emerge because of differences in initial offers (of male employees generally, and male employers to male employees) rather than subsequent bargaining, suggesting that sex differences in bargaining skills are not critical.

7. Natural Experiments

A handful of studies on discrimination in labor markets can be considered natural experiments. In some, variation in the race of who evaluates workers is effectively random, giving us information on how different workers are evaluated depending on the characteristics of the evaluator. And in one the evaluation mechanism exogenously changed from evaluators knowing the sex of applicants to sex-blind evaluations, which is informative about discrimination prior to the advent of sex-blind selection.³¹

By way of analogy, suppose that, as in a traditional non-experimental study of wage discrimination, we had data on workers' wages, race, sex, and human capital controls. However, suppose we also had repeated observations on the race of the supervisor who was setting wages, and this varied for reasons exogenous with respect to pay. Alternatively, suppose we had evidence that in some cases pay was set without the supervisor knowing the worker's sex, again for exogenous reasons. These are not realistic scenarios, yet we would likely regard them as natural or at least "quasi" experiments. The handful of studies I discuss in this section, which are summarized in Table 2, have the same flavor.

7.1 Sports

Kahn (2000) describes how detailed data in the world of sports on performance, work histories, "supervisors," and pay can be used to study discrimination. A few recent papers have pushed the envelope in this area of research to consider natural experiments. These studies perhaps do not get at the most fundamental questions, such as who gets hired and how much they are paid. But they use clever research designs that provide clean evidence on how workers are evaluated, and how this affects performance. And

³¹ Indeed, as discussed in the next section, there are a couple of field experiments where a similar change was made.

they also – some studies argue – present evidence on implicit discrimination.

Parsons et al. (2011) study the evaluation of Major League Baseball pitchers by umpires, looking at pitches near the edges of the strike zone where there is more uncertainty as to whether a pitch is a ball or a strike. They assemble a large dataset in which pitchers and umpires appear multiple times, and are matched randomly – based on both the data and the institutional assignment of umpires to games. They can therefore estimate models conditional on pitcher and umpire fixed effects and obtain causal estimates of how the race or ethnicity of the umpire and the player affects strike calls. Their evidence indicates that strikes are more likely to be called when the umpire and pitcher are of the same race/ethnicity. Moreover, umpire evaluations affect pitcher performance. A different-race pitcher is less likely to pitch to the edges of the strike zone where they will experience this disadvantage, and this strategic response affects the pitcher’s success adversely, because a pitch to the middle of the strike zone is easier to hit.

The study also exploits variation in monitoring of umpires – either explicitly through the use of technology or implicitly based on crowd size and the strategic importance of the pitches. They find that the bias in umpiring disappears under more intensive monitoring, and appears only (and more strongly) in the low-monitoring environments. Parsons et al. interpret the evidence as indicating that when the “cost” of discrimination is higher, umpires engage in less of it.

A study on calling of fouls by referees in the National Basketball Association (Price and Wolfers, 2010) provides evidence of a similar nature. The authors know the racial composition of the three-person umpiring crew, and the race of players (they focus on blacks and non-blacks). Using similar analyses as Parsons et al. (although with no evidence of explicit monitoring, and perhaps less clear evidence of implicit monitoring), they find that fouls are more likely to be called on players whose race deviates more from that of the umpiring crew, and that this bias appears to affect players and teams adversely. Interestingly, this bias mainly affects white players, either via worse treatment by black referees (in contrast to what we think generally might happen in the labor market), or favorable treatment by white referees.

Finally, Gallo et al. (2013) study whether referees award yellow cards after fouls in English Premier League soccer games. They focus not solely on race, but on what they term “oppositional identity”

based on players' race and foreign origin from poorer countries. The authors try to test for implicit discrimination by asking whether yellow cards to those of oppositional identity are more common when the decision is more rushed (because of the position of the foul on the field), or more ambiguous (e.g., contrasting ambiguous fouls called for hard plays, versus unambiguous fouls called for excessive celebration). The authors find more evidence of fouls called on these players in rushed or ambiguous instances. I leave it to soccer experts to judge whether the authors correctly interpret the role of position of the foul on the field. However, I find testing for implicit discrimination based not just on race but on broader oppositional identity somewhat unconvincing, since race is obviously directly observable in a flash, whereas country of origin (and that country's GDP) might take more mental processing; in fact, the study does *not* find the kind of evidence it interprets as implicit discrimination when it focuses only on race.

The evidence in the Parsons et al. and Price and Wolfers studies, that biases in evaluations can affect performance, means that performance measures in discrimination studies cannot necessarily be treated as exogenous. And if discrimination leads to worse performance (as seems likely in these contexts, but not necessarily in all contexts),³² regressions of pay or related measures on performance and group membership will understate discrimination. This is, in a sense, another manifestation of the kinds of feedback effects that have been discussed in non-experimental research on discrimination.

7.2 *Orchestras*

Goldin and Rouse (2000) study the impact of the switch to blind auditions for major orchestras on the selection of female auditionees. The variation in this study arises from the adoption by orchestras, over time, of blind auditions where the musician plays behind a screen and other steps are taken to ensure that the musician's identity is not known when selection decisions are made. The authors find that the selection of females increased because of blind auditions, suggesting that there was discrimination against women prior to the adoption of blind auditions.³³ The contribution of this study to understanding discrimination in

³² In contrast, think of the Charlotte Whitton quote: "Whatever women do they must do twice as well as men to be thought half as good," (famously followed by "Luckily, this is not difficult").

³³ The use of blind auditions is chosen by the orchestra, and hence the variation is not really "natural." More substantively, the authors are careful to rule out orchestras that hire more women ex ante adopting blind auditions (or adopting them earlier) – analogous to testing the parallel-trends assumption in a quasi-experimental panel data

the labor market is more substantial than the sports studies, because the audition process relates directly to hiring. Indeed, Goldin and Rouse cast their study as complementary to the kinds of audit or correspondence studies of discrimination discussed in the next section of the paper.

The study includes a cautionary tale about how either natural or experimental variation can induce behavior that eliminates *reverse* discrimination. In particular, there is type of situation in which blind auditions disfavored women – a semi-final stage when affirmative action (which cannot take place when auditions are blind) may have been used to increase the pool of women. Below, I discuss a couple of field experiments where adopting sex- or race-blind procedures seems to have reduced hiring opportunities for women or minorities, presumably by reducing affirmative-action-like favoritism. In the Goldin and Rouse study, however, this presumably unintended consequence at one stage of the process was less important than the larger overall impact of blind auditions in increasing the selection of women.

8. *Field Experiments on Hiring Discrimination*

The most extensive body of experimental research on labor market discrimination is audit or correspondence (AC) studies of discrimination in hiring. There are lengthy discussions of the basic methods elsewhere (Fix and Struyk, 1993), and a few survey papers or meta-analyses that provide broad overviews of the findings for some groups. I focus on key issues in the design of field experiments and analysis of data from them, to help the reader understand how these studies have developed and to try to identify unresolved or potentially promising research questions. I also provide a more detailed overview of results for groups that have not been the focus of the bulk of the existing studies and hence of the prior surveys.

8.1 *Basic Methods*

AC studies of the labor market are field experiments used primarily to address the question of discrimination in hiring. In audit studies, fake job candidates (“testers”) of different races, ethnicities, etc., who are sometimes actors, are sent to interview for jobs (or in some early studies, apply by telephone). The

analysis. The study also illustrates the importance of controlling for worker heterogeneity (via a fixed effects analysis that evaluates outcomes for workers under different treatments), because the adoption of a more neutral screening procedure that reduced sex discrimination increased the likelihood that less-qualified female musicians auditioned.

candidates have similar resumes and are often trained to act, speak, and dress similarly. Correspondence studies, in contrast, use fictitious job applicants who exist on paper only (or now, electronically), and differ systematically only on group membership. The response captured in correspondence studies is a “call-back” for an interview or a closely related positive response. In contrast, the final outcome in audit studies is actual job offers. The earliest AC studies of discrimination were Daniel (1968) – an audit study – and Jowell and Prescott-Clarke (1970) – a correspondence study.³⁴ AC studies have also been used to study discrimination in housing markets and consumer markets. My focus is on labor markets and the unique issues that arise in these markets.³⁵

It is easy to see the appeal of experimental AC studies. Non-experimental regression-based approaches to testing for and measuring discrimination use data on the groups in question in a population, introducing regression controls to try to remove the influence of group differences in the population that can affect outcomes. AC studies, in contrast, create an artificial pool of labor market participants among whom there are supposed to be no average differences by group. This is clearly a potentially powerful strategy, because if we have, e.g., a sample of blacks and whites who are identical *on average*, then in a regression of the form

$$Y = \alpha + \beta B + \varepsilon, \tag{8.1}$$

where Y is the outcome and B is a dummy variable for blacks, ε is uncorrelated with B , so that the OLS estimate $\hat{\beta}$ (or simply the mean difference in Y) provides an estimate of the effect of discrimination on Y .

Of course, most of the regression studies focus on wages, whereas AC studies focus on hiring. If an employer is free to pay a lower wage to blacks, for example, then in the context of the Becker employer discrimination model, why discriminate in hiring? One common interpretation is that, as discussed earlier, there is an equal wage constraint – perhaps due to a minimum wage, or because anti-discrimination laws are more effective at rooting out wage discrimination than hiring discrimination. Alternatively, in the

³⁴ Schwartz and Skolnick (1962) did an early version of a correspondence study of the effects of criminal background. But this study did not focus on a characteristic usually associated with discrimination (e.g., race).

³⁵ The application to housing markets originated with the U.S. Department of Housing and Urban Development’s Housing Market Practices Study, in 1977). For an overview of some of the existing work, see <http://www.urban.org/features/exposing-housing-discrimination> (viewed November 22, 2015). For recent research on consumer markets, see Doleac and Stein (2013) and Zussman (2014).

simple model, employers with stronger discriminatory tastes than the marginal employer will discriminate in hiring.

For some of the discussion that follows, a more formal framework is useful.³⁶ Suppose that productivity depends on two individual characteristics (standing in for a larger set of relevant characteristics), $X' = (X^I, X^{II})$, so that productivity is $P(X')$. X^I is what the firm observes, and X^{II} is unobserved by firms. It is simplest, for now, to think of Y as continuous, such as the wage offered, although in fact in AC studies we should think of it as latent productivity leading to a decision to hire/call-back or not. Define discrimination as

$$Y(P(X'), B=1) \neq Y(P(X'), B=0). \quad (8.2)$$

Assume that $P(.,.)$ is additive, so

$$P(X') = \beta_I X^I + X^{II},^{37} \quad (8.3)$$

and

$$Y(P(X'), B) = P + \gamma B. \quad (8.4)$$

Discrimination against blacks implies that $\gamma < 0$, so that blacks are paid less than equally productive whites.

In AC studies, researchers create resumes that standardize the productivity of applicants at some level. Denote expected productivity for blacks and whites, based on what the firm observes, as P_B^* and P_W^* . Y is observed for each tester, so each test – the outcome of applications to a firm by one black and one white tester – yields an observation

$$Y(P_B^*, B = 1) - Y(P_W^*, B = 0) = P_B^* + \gamma - P_W^*. \quad (8.5)$$

Given that the AC study design sets $P_B^* = P_W^*$, we should be able to estimate γ easily from these data, by simply running a regression of Y on the dummy variable B and a constant.³⁸ However, there are potential complications.

³⁶ This is a simplified version of Neumark (2012).

³⁷ Because X^{II} is unobservable, its coefficient is normalized to one.

³⁸ Moreno et al. (2012) present an interesting analysis using observational data from a government job intermediation service in Peru. They are able to obtain detailed data on applicants for jobs and on characteristics of jobs. They refer to this as a “pseudo-audit” study, because it uses actual rather than fictitious applicants. They also refer to it as a field experiment, although strictly speaking there is no experimental variation.

Nearly all studies used matched pairs of testers (or even triplets or quadruplets), usually justified as a means of controlling for heterogeneity across employers (indeed one can include employer fixed effects in the regression models estimated).³⁹ In a recent paper, Phillips (2015) suggests that some existing studies appear to understate discrimination because of positive spillovers that arise from matched-pair comparisons in which the quality of the artificial applicants submitted influences employers' evaluations of other applications submitted (although the bias can go in either direction).⁴⁰ Such spillovers are unlikely if the number of artificial applications is small relative to the number of applications the employer receives, but typically researchers do not know the latter number. For researchers concerned with this issue, the problem can be avoided by foregoing matched-pair designs – for example, randomizing race for each applicant independently within pairs – so that the “treatment” of being assigned a black name is not correlated with the applicant pool composition (as affected by the researcher).

With matched-pair designs, some researchers discard pairs of testers in which neither gets a call-back or interview, and measure “net discrimination” as the difference between the number of pairs where the white is favored over the black versus the opposite, divided by the number of tests with at least one positive response (e.g., Riach and Rich, 2002). However, recent work often translates the results of AC studies into differences in how many jobs a member of a group has to apply to in order to get a call-back or interview (e.g., Bertrand and Mullainathan, 2004; Rich, 2014), for which the number of pairs in which neither gets a response is clearly relevant. Moreover, the full data seem critical to gauging discrimination. For example, suppose that there are 100 tests in a study, and in 97 neither tester gets a positive response, in two the white tester does, and in one the black tester does. Discarding the pairs with no offers implies net discrimination of 33.3 percent, whereas a more reasonable interpretation would be that it is only slightly

³⁹ A recent exception is Weichselbaumer (2015a), who studies discrimination in Germany against women with Turkish names wearing headscarves (in Germany, photos are included). She sends only one application to each employer, for two reasons: first, she argues that correspondence studies have gotten enough publicity in Germany that employers may look for closely matching applications; and second, job applications in Germany require extensive amounts of information, including detailed school reports, making it hard to match within pairs. The basic statistical analysis is the same, but there can be differences in the clustering of observations and/or the fixed effects that can be included.

⁴⁰ See also related evidence in Weichselbaumer (2015b). Although based on small sample, she finds weaker evidence of discrimination in a paired-application design, which she conjectures may be because employers may detect the test and avoid or reduce discrimination.

harder for a black to get a positive response than a white. Standard practice has become to report estimates from linear probability or probit models (marginal effects), retaining all the observations; this approach does not require matched pairs.

8.2 *Data and Design Issues*

There are a number of decisions researchers have to consider in designing an AC study. One key decision is whether to do an audit or a correspondence study. A major advantage of audit studies is that actual job offers are observed, which are more convincingly tied to actual labor market outcomes, although, perhaps surprisingly, audit studies have not proceeded to the stage of wage offers.⁴¹

A second issue in this choice is that call-back rates could be influenced by factors that make them less reliable indicators of ultimate hiring. Suppose that a correspondence study uses high-skilled white and black candidates. Employers may know that the representation of high-skill blacks in the population is lower. Hence, if they want to hire some blacks, they may make a disproportionately high number of call-backs to blacks, expecting more competition for them from other employers, which could boost call-back rates for blacks and obscure evidence of discrimination.⁴² However, a set of audit studies on ethnic discrimination by the International Labor Organization (ILO), discussed in Riach and Rich (2002), separate discrimination at the selection for interview and job offer stage, and find that most (around 90 percent) of the discrimination occurs at the selection for interview stage, suggesting that call-backs for interviews constitute the key part of the hiring process to study.⁴³ Moreover, nearly all correspondence studies find evidence of discrimination against minorities, so at worst call-back studies could be understanding racial and ethnic discrimination, rather than obscuring it. In contrast, the evidence of sex discrimination from correspondence studies is weaker and more ambiguous. However, it is unlikely that higher call-backs per female applicant relative to intended hiring are responsible for this evidence, because skill distributions are

⁴¹ Another potential advantage noted by Pager (2007) is that in audit studies testers can be debriefed regarding their treatment during the interview, although this can inject a subjective element.

⁴² Of course, this can also happen for job offers in audit studies, since not all offers are accepted.

⁴³ Similarly, in my audit study of sex discrimination in restaurant hiring (Neumark, 1996), we were able to compare call-backs resulting from candidates dropping off resumes in the morning when managers were not there and there was little personal interaction, to results for job offers based on the actual interviews. The evidence pointed to discrimination at both the invitation for interview and job-offer stages.

more similar between men and women. Nonetheless, there are still open questions of what we learn from differences in call-back rates in correspondence studies, relative to job offers and other labor market outcomes.

On the other hand, correspondence studies have many advantages. One is the much lower cost per application, which permits researchers to obtain larger samples, with which they can explore a richer set of questions. As examples, the large-scale Urban Institute audit studies of discrimination against blacks and Hispanics (Cross et al., 1990; Turner et al., 1991) had hundreds of observations. In contrast, Bertrand and Mullainathan's (2004) correspondence study of black-sounding names had over 5,000 observations, and Neumark et al.'s (2015) correspondence study of age discrimination had over 40,000 observations. Both correspondence studies (and especially the latter) use a number of different variations of resumes to test alternative hypotheses about discrimination.

Correspondence studies also let researchers avoid experimenter effects that can lead to bias in audit studies. For example, Heckman and Siegelman (1993) point out, in their evaluation of the Urban Institute audit studies, that part of the training of testers was "a general discussion of the pervasive problem of discrimination in the United States" (p. 216), raising the possibility that testers took actions in their job interviews that led to the "expected" result. Similarly, Pager (2003) reports that black testers posing as having criminal records may have had negative psychological reactions that affected their performance in interviews. Correspondence studies avoid this problem, effectively creating blind testers.

More generally, in audit studies the influence of small differences between testers from different groups can be amplified by the nature of the study design. For example, Heckman and Siegelman note that Hispanic testers in one of the Urban Institute test sites had facial hair. Any productivity difference associated with facial hair might seem trivial compared to the productivity-related characteristics employers care about. But when the research design matches on many productivity-related characteristics, the importance of these trivial features can be magnified.⁴⁴ This problem is avoided in correspondence

⁴⁴ It is possible that $E(X_B^{II} - X_W^{II} | X_B^I = X_W^I)$ – the bias with testers matched on X^I – is greater than $E(X_B^{II} - X_W^{II})$ – the expected difference in randomly matched pairs from the population. However, if X^I and X^{II} are positively correlated, which seems plausible, then standardizing on X^I implies that the expected difference in X^{II} between matched black and

studies because resume characteristics are randomized.⁴⁵ For the preceding reasons, the correspondence-study method has come to dominate field experiments on labor market discrimination, and hence much of the ensuing discussion of methods focuses on correspondence studies.

The second key design issue – which really undergirds the quality of the research project – is the construction of applicants and their resumes. One decision concerns which kinds of jobs to study. Typically, these studies focus on lower-skill, entry-level jobs, for three reasons: first, the ways in which job applications are made and responses collected (e.g., by email) are more common for low-skill jobs;⁴⁶ second, websites now list numerous low-skill jobs to which researchers can apply; and third, for higher-skill jobs there is likely a greater likelihood that candidates would be known to prospective employers or that employers could easily learn something that reveals the applicant as fictitious.⁴⁷ Aside from missing higher-skill jobs, correspondence studies likely miss jobs found through informal contacts and referrals, which are important job search methods (Ioannides and Datcher Loury, 2004).

A key goal in constructing applicant resumes is to make them realistic for the jobs being sought. Recent studies have used websites on which resumes are posted as the basis for the resumes used in the study (e.g., Bertrand and Mullainathan, 2004; Neumark et al., 2015).⁴⁸ The devil really is in the details here; Neumark et al. (2015) provide a recent discussion of many of the potential issues. And there is computer code available to input resume entries and create resumes for correspondence studies that are

white testers is on average *smaller* than for a randomly selected pair.

Pager (2007) suggests that the problem of failure to match on productivity-related characteristics may be overstated, because “it is a mistake ... to assume that the researcher is at a necessary disadvantage relative to the employer in identifying productivity-related characteristics” (p. 115). This argument hinges on researchers having strong incentives to match on the same characteristics that are important to the low-skill employers included in these kinds of studies.

⁴⁵ One way to test for the influence of uncontrolled tester characteristics in audit studies is to use multiple pairs of testers and explore the robustness of the findings across different pairs of testers, perhaps using tests of the homogeneity of results for the different pairs prior to pooling the data (Heckman and Siegelman, 1993). For example, in my audit study of sex discrimination in restaurant hiring (Neumark, 1996), the two female testers were quite different looking, and one had more success in getting offers at high-price (and high-pay) restaurants, so I verified the robustness of the overall results to using only pairs with the more successful female tester.

⁴⁶ On the other hand, Zschirnt and Ruedin (2015) note that correspondence studies might not cover the very lowest-skill jobs where written or on-line applications are not used.

⁴⁷ There are exceptions to a focus on low-skill jobs. In a recent study of discrimination against the disabled, Ameri et al. (2015) used fictitious applicants who were either novice or experienced accountants, and had call-back rates in the 5-8 percent range, only a shade lower than, for example, the 6-10 percent range for lower-skilled sales and administrative jobs in Bertrand and Mullainathan (2004).

⁴⁸ Sometimes actual resumes posted on job-search websites are used (e.g., Siddique, 2011).

randomized along many dimensions with respect to the groups being studied (Lahey and Beasley, 2009).

In correspondence studies, group membership has to be signaled on the resume. This is probably easiest with regard to sex, since most first names are not gender-neutral, or with respect to age, since many resumes list year of high school graduation (Neumark et al., 2015). Race is not listed on resumes, so Bertrand and Mullainathan use typically black and white names based on birth certificate data. This introduces the possibility that employers associate productivity differences with black-sounding names (from their title, “Lakisha and Jamal” versus “Emily and Greg”). Bertrand and Mullainathan try to rule this out by controlling for neighborhood quality.

Other characteristics are even less natural to signal on resumes. For example, studies of discrimination based on sexual orientation (e.g., Drydakis, 2009) have included a line indicating volunteer work on behalf of the gay community or a gay organization on some resumes, versus similar work for a different kind of organization (e.g., environmental) to avoid activism or volunteering per se signaling something to employers. In their study of disability, Ameri et al. (2015) disclose one of two disabilities in a cover letter.

There are potentially interesting questions as to how different ways of signaling group membership may affect outcomes, perhaps most importantly by unintentionally providing information on productivity differences. The issue of black-sounding names was already discussed. As another example, Figinski’s correspondence study of military reservists (2013) wrestles with whether selection into the military, or past activations, are associated with productivity-related factors. Similarly, Ameri et al. (2015) try to choose disabilities that are unlikely to affect productivity (suggesting that one of the disabilities they study – Asperger’s syndrome – may even be associated with higher productivity in accounting).

8.3 *Differences in Unobservables*

A correspondence study can preclude systematic differences between groups in observables. But there can still be differences in employers’ assumptions about unobservable differences between groups. I discuss, in turn, differences in levels and differences in variances, which raise different issues.

8.3.1 Differences in Means of Unobservables

As noted above, the group difference in outcomes we estimate in a correspondence study is

$$Y(P_B^*, B = 1) - Y(P_W^*, B = 0) = P_B^* + \gamma - P_W^*, \quad (8.6)$$

where $P_B^* = E(\beta_l X_B^I + X_B^{II} | X_B^I, B = 1)$, and similarly for P_W^* . Assuming randomization, and with $X_B^I = X_W^I = X^I$, this reduces to $\gamma + E(X_B^{II} | X^I, B = 1) - E(X_W^{II} | X^I, B = 0)$, implying that we only identify γ if $E(X_B^{II} | X^I, B = 1) = E(X_W^{II} | X^I, B = 0)$. But employers may have different expectations about the mean of X^{II} for blacks and whites, conditional on what they observe, which a labor economist would label statistical discrimination.

Thus, absent any method of distinguishing between γ and $E(X_B^{II} | X^I, B = 1) - E(X_W^{II} | X^I, B = 0)$, if the job application process used in a correspondence study mimics the process used in the real world, correspondence studies identify the combined effects of taste discrimination and statistical discrimination. In contrast, if the study provided much less information about applicants than employers usually have, then the statistical discrimination that is identified as part of the sum $\gamma + E(X_B^{II} | X^I, B = 1) - E(X_W^{II} | X^I, B = 0)$ might have little to do with real-world statistical discrimination. On the other hand, when a correspondence study includes a rich set of applicant characteristics, it becomes less likely that statistical discrimination plays much of a role in group differences in outcomes. These issues come to the fore in studies that try to distinguish between statistical and taste discrimination.

8.3.2 Differences in Variances of Unobservables

A second problem is that employers may assume different *variances* of the unobservable across groups. With data on hiring – the focus of AC studies – it is most natural to think of applicants having to exceed some productivity threshold with sufficiently high probability, and in such models different variances of the unobservable can matter. I refer to this problem, described in Heckman and Siegelman (1993) and Heckman (1998), as the “Heckman critique” of AC studies. The possibility of a difference in the variance of unobservables is in fact a central feature of some models of statistical discrimination (Aigner and Cain, 1977). Although this model was applied to race and sex differences, unequal variances of the unobservable may be more plausible in comparing other groups, such as older versus younger workers. The human capital model predicts that earnings become more dispersed with age as workers

invest differentially in human capital and these differences accumulate (Mincer, 1974). Because this variation is unlikely to be conveyed on the resumes used in correspondence studies, it could imply a larger variance of unobservables for older versus younger applicants.

To isolate the problem, consider the best-case scenario where $E(X_B^H|X^I, B = 1) = E(X_W^H|X^I, B = 0)$ – i.e., there is no statistical discrimination regarding levels. But the standard deviations of the unobservables, denoted σ_B^H and σ_W^H , need not be equal.⁴⁹ Assume the applicant is called back (hired) if there is a sufficiently high probability that their productivity exceeds a given threshold. In this case, $\sigma_B^H \neq \sigma_W^H$ generates bias in the estimate of discrimination; worse, perhaps, we cannot necessarily even sign the bias.

To see this intuitively, suppose that the research design standardizes X^I at a low level, denoted X^{I*} . Employers care about how likely it is that the sum $\beta_I X^I + X^H$ exceeds some threshold. Given the low value X^{I*} , this is more likely for a group with a high variance of X^H . Thus, even in the case of no discrimination ($\gamma = 0$), the employer will favor the high-variance group. Conversely, if standardization is at a high level of X^{I*} , the employer will favor the low-variance group. Because researchers do not have information on the population of real applicants to the jobs studied, there is no definitive way to know whether X^{I*} is high or low relative to the actual distribution, and hence no way to sign the bias.

I proposed a solution to this problem that separately identifies the relative variances in the unobservables and the discrimination coefficient, γ (Neumark, 2012).⁵⁰ The intuition behind the solution stems from the fact that a higher variance for one group (say, whites) implies a smaller effect of observed characteristics on the probability that a white applicant meets the standard for hiring. Thus, information from a correspondence study on how variation in observable qualifications is related to employment outcomes can be informative about the relative variance of the unobservables, and this, in turn, can identify the effect of discrimination. Based on this idea, the identification problem identified by the Heckman critique is solved by invoking an identifying assumption – specifically, that the effect of applicant characteristics that affect perceived productivity and hence call-backs have equal effects across groups –

⁴⁹ I assume homoskedasticity within groups and hence suppress conditioning on X_B^I and X_W^I .

⁵⁰ To reiterate, in this section I assume $E(X_B^H|X^I, B = 1) = E(X_W^H|X^I, B = 0)$, to simplify. Without this assumption, references to γ in the remainder of this section should be read as references to $\gamma + E(X_B^H|X^I, B = 1) - E(X_W^H|X^I, B = 0)$ – i.e., the sum of taste and statistical discrimination.

along with the testable requirement that some applicant characteristics affect the call-back probability (since if all the effects are zero we cannot learn about σ_B^H/σ_W^H from these coefficient estimates).

In a probit specification, for example, we know that can only identify the coefficients of the latent variable model for productivity relative to the standard deviation of the unobservable. In this case, we effectively have two probit models, one for blacks and one for whites. If we normalize σ_W^H to one, then for a characteristic (Z) that affects the call-back rate, we identify its coefficient (δ_W) relative to σ_W^H , or δ_W/σ_W^H . However, if we assume that $\delta_W = \delta_B$, then we do not need to impose the normalization that $\sigma_B^H = 1$, but instead can identify σ_B^H/σ_W^H from the ratio of the coefficients on Z in the probit for whites versus blacks, which in turn allows us to identify γ . The estimation can be done using a heteroskedastic probit model, which also permits an approximate decomposition into the effect of race via γ and its effect via the difference in variances of the unobservable. Finally, when there are *multiple* productivity-related characteristics that shift the call-back probability Z_k ($k=1, \dots, K$), there is an overidentification test because the ratio of coefficients on each Z , for whites relative to blacks should equal σ_B^H/σ_W^H .

It is actually rare that correspondence studies include variables that shift the call-back probability, because these studies typically create one “type” of applicant for which there is only random variation in characteristics that are not intended to affect outcomes. However, a handful of studies have this information – including Bertrand and Mullainathan’s (2004), whose data I used to illustrate this approach. Moreover, going forward studies can be designed to include this kind of quality variation.

This discussion also raises the issue of what we are trying to measure in AC studies. To this point, I have focused on γ , which I think of as the structural effect of race capturing the potential discounting by employers of black workers’ productivity à la Becker (and possibly statistical discrimination about the mean of X^H). But we just saw that employers could treat blacks and whites differently in hiring because of different variances of the unobservable. Is the latter a meaningful measure of discrimination, in which case we might not want to eliminate it?

I think there are two reasons why the coefficient γ is the object of interest. First, to the best of my knowledge, differential treatment based on assumptions (true or not) about variances are not viewed as

discriminatory in the legal literature. Second, and probably more important, the taste discrimination (and possibly “first-moment” statistical discrimination) that AC studies capture in γ generalizes from the correspondence study to the real economy. In contrast, the “second-moment” statistical discrimination is an artifact of how a correspondence study is done – in particular, the standardization of applicants to particular, and similar, values of the observables, relative to the actual distribution of observables among real applicants.⁵¹ If, instead, a study used applicants that replicated the actual distribution of applicants to the employers in the study, there would be no bias – in the setting described here – from the different variances of the unobservable; that is, differential treatment is an artifact of the study design.

8.4 *Testing the Nature of Discrimination*

Many recent AC studies try to distinguish whether taste or statistical discrimination underlies their findings of hiring discrimination. The approach is generally to add information to resumes, and if doing so diminishes the differences in callback or job offer rates between groups, the evidence is interpreted as indicating that employers must have been statistically discriminating – i.e., assuming group differences with respect to the types of information provided included in the “high-information” treatment.

There are two potential problems with this approach. First, we do not know, *ex ante*, on what unobserved characteristics employers might be statistically discriminating. If we add information that is not the basis for employers’ statistical discrimination, then a null finding of no change in offer or call-back rates is uninformative. Second, a reduction in the difference between offer or callback rates from adding information to the resumes does not necessarily imply statistical discrimination, because the experiment – and in particular the “low-information” treatment – may not mimic actual employer hiring. To take an extreme case, suppose we compare results using resumes with no information except the group identifier to resumes with other information (like education and job histories) that employers would typically have, and suppose that the callback or offer rate difference between the groups diminishes. This does not imply that, in the real world, members of the disadvantaged group suffer from statistical discrimination, because employer would actually have the information on education and job histories. A valid test requires that we

⁵¹ That is not to say there cannot be discrimination based on second moments with, for example, risk averse firms.

know what information is typically *not* provided in the job application process, on the basis of which employers statistically discriminate, and examine the effect of adding that information.

To see that there is no “general” result that adding information to an AC study is informative about whether statistical discrimination generates group differences in hiring or call-back rates, using the notation from earlier suppose that

$$E(P|B = 1) < E(P|B = 0) . \tag{8.7}$$

Then in an audit study where employers know only race (B), in the model for hiring

$$Y = \alpha + \beta B + \varepsilon , \tag{8.8}$$

the estimate β reflects both taste discrimination (γ) plus the expected productivity differential. If the resumes instead also provide information on X , and

$$E(P|B = 1, X) = E(P|B = 0, X) , \tag{8.9}$$

then the estimate of β reflects only taste discrimination, and hence the effect of adding X to the resumes might be interpreted as providing evidence of statistical discrimination. But if real-world employers know X , then this evidence is only an artifact of using an unrealistic baseline “low-information” treatment that excludes X . Conversely, suppose we know that employers have information on X but not on Z , and the low-information treatment includes X while the high-information treatment also includes Z . If $E(P|B = 1, X) < E(P|B = 0, X)$, but $E(P|B = 1, X, Z) = E(P|B = 0, X, Z)$, then adding Z reduces group differences from statistical discrimination. But if $E(P|B, X, Z) = E(P|B, X)$ – that is, if Z is uninformative about productivity – then adding Z has no impact on estimated group differences. Thus, the validity of this kind of test for statistical discrimination in AC studies hinges in part on how well the job applications in the low-information treatment capture the information employers actually have, and – less knowable – the extent to which the high-information treatment conveys the signals of interest to employers.

Aside from the focus on statistical discrimination, a few studies try to test Becker’s model of customer discrimination, by comparing call-back rates across jobs with different degrees of customer contact. (I am not aware of attempts to test for employee discrimination, which would require information on the composition of the tested employers’ workforces.) Finally, a couple of recent studies, discussed

below, use creative means to try to examine whether implicit discrimination accounts for findings of discrimination in AC studies.

8.5 *Race, Ethnicity, and Sex*

8.5.1 *Summary of Findings*

An extensive survey of results from field experiments through 2000 is provided in Riach and Rich (2002), and Rich (2014) surveys field experiments conducted since then, both focusing on the standard demographic categories. I do not provide a detailed study-by-study discussion of the extensive evidence on race, ethnic, and sex discrimination that these surveys cover. Rather, I draw out the main lessons from the surveys, and focus on issues that may pose challenges to the conclusions from these studies and motivate further research.⁵² In subsequent sections, I turn in more depth to some of these issues and to evidence on other groups, especially when that evidence raises new issues that interesting areas for additional research. The studies I discuss are summarized in Table 3.

Riach and Rich cover eight AC of race or ethnic discrimination (which they refer to with the catchall “race”) in the United Kingdom, six studies in other European countries, five studies in the United States, and one each in Canada and Australia. Some of these studies report more than one analysis (e.g., for both interviews and job offers, or for different ethnic groups). The key result is that every single comparison in these studies is in the direction of discrimination against the minority group, and most of the estimates of discrimination are statistically significant.⁵³ Although surely one can quibble with some details of the studies,⁵⁴ it seems difficult to argue with the conclusion that “In view of the number of studies involved and their geographical extent this is compelling evidence of enduring and pervasive racial discrimination in employment” (Riach and Rich, 2002, p. F499).

⁵² Riach and Rich (2002) also provide a detailed response to many points raised in the Heckman and Siegelman (1993) chapter critiquing the Urban Institute audit studies, and discuss many “nuts-and-bolts” issues that researchers planning to conduct an audit or correspondence study might find useful. (See also Pager, 2007).

⁵³ The significance tests are based on the net discrimination measure that excludes from the denominator tests with no positive responses. This has no effect on the sign of the estimated discrimination, and an ambiguous effect on statistical significance (increasing the estimate by using a smaller denominator, but reducing the sample size).

⁵⁴ For example, in the ILO studies the minority applicant always make the first contact (in this case, by phone), and the test is stopped as soon as one applicant is rejected, which is more likely to be the minority applicant. Most studies randomize the order of application in each test, and I am unaware of other studies where the test was stopped after one rejection. (In correspondence studies, there usually is not a rejection, but simply no call-back.)

The survey of more recent studies in Rich (2014) – covering a larger number of studies and more countries – also documents near-uniform evidence of hiring discrimination against racial and ethnic minorities, with only a handful of exceptions. One notable finding across a number of studies (in Australia, Belgium, France, Germany, Italy, and Sweden) is the consistent evidence of labor market discrimination against applicants of Middle Eastern origin (often immigrants).⁵⁵

The findings on studies of sex discrimination are more ambiguous. Riach and Rich summarize results for five studies – three in the United States, and one each in Australia and Austria. An interesting dimension of these studies is that some of them distinguish between results for more traditionally male-versus female-dominated fields or sectors. These studies appear to provide clear evidence of discrimination against women in male-dominated fields, and also in higher-pay or more-senior jobs within a sector (e.g., high-price and higher-pay restaurants in the United States in my 1996 study), but also discrimination against men in female-dominated jobs. Moreover, some studies fail to find evidence of sex discrimination in more sex-integrated jobs.

The additional studies surveyed in Rich (2014) confirm both findings. Indeed Rich suggests that some of the strongest evidence of discrimination arises for men applying to typically female jobs (although this is largely based on net discrimination measures computed relative to tests for which one or both testers received a positive response).⁵⁶ Emphasizing the absence of consistent evidence of discrimination against

⁵⁵ There is no clear way to explicitly signal religion in these studies. However, a recent study by Weichselbaumer (2015a) studies discrimination against Turkish women in Germany based on whether they wear a headscarf, which is likely to signal a religious Muslim. Compared to women with German names, and women with Turkish names who do not wear a headscarf (in the photos included), Turkish women with a headscarf experienced much lower call-back rates (roughly three times the penalty for a Turkish name).

Two correspondence studies in India – where apparently caste and religion can be signaled by name – find mixed evidence. In software jobs, Banerjee et al. (2009) find no evidence of discrimination against lower castes, or between Hindu and Muslim upper-caste applicants. But in call centers they find discrimination against lower castes, which they suggest may be attributable to employers wanting workers with better English, American telephone etiquette, etc., on the basis of which they statistically discriminate against lower-caste applicants. Siddique (2011) finds some evidence of discrimination against lower castes in white-collar jobs, concentrated on female applicants in front office or administrative jobs. She also finds some evidence that caste-related differences in call-back rates vary with the sex and religion of recruiters (inferred from the name of the contact person listed in the ad, who may not be the decision maker), and suggests that since expectations regarding the different groups of applicants should be the same across recruiters, variation in call-back rates with recruiter characteristics is more likely to reflect taste discrimination.

⁵⁶ And based on data from correspondence studies of sex discrimination in Sweden, Carlsson (2011) concludes that the relationship between gender bias in call-backs and the female share in the occupation is quite weak. He also finds that the gender of the recruiter and the proportion female at the firm (based on matching to firm-level data) does not affect the relative treatment of female candidates, which he interprets as ruling out “in-group favoritism” (p. 90).

women, in a study of China, Zhou et al. (2013) found statistically significant evidence of discrimination against men in most jobs covered, including many jobs that would not be viewed as typically female.

A recent meta-analysis by Zschirnt and Ruedin (2015) also provides summary measures, in a more consistent form, of correspondence studies of racial and ethnic discrimination in OECD countries. The overlap with Riach and Rich (2002) and Rich (2014) is substantial, and the simple summary statistics therefore, not surprisingly, convey the same near-uniformity of the findings of hiring discrimination against ethnic minorities. What is unique in this study, however, is the attempt to quantify (via meta-analysis) how the estimates of discrimination in these studies vary with a number of study characteristics, to attempt to shed light on hypotheses about what drives relatively stronger or weaker evidence of this discrimination, and to try to understand the nature of discrimination. Most of this part of the analysis pertains to trying to test for statistical versus taste discrimination, which I take up below. However, another interesting finding the authors report is that they find no association between GDP growth or unemployment rates and measured discrimination, in contrast to the hypothesis that employers are more likely to engage in discrimination in slack labor markets (e.g., Biddle and Hamermesh, 2013).⁵⁷

8.5.2 *Testing for the Nature of Discrimination*

Based on the consistent evidence of hiring discrimination across so many groups, Riach and Rich (2002) suggest that it is unlikely that statistical discrimination underlies most of the findings. As they put it:

“Given the very diverse cultural, social, religious and educational backgrounds of the various Asian, Arab and African-descendant groups who have encountered the employment discrimination in these studies, it would be a superficial generalization to hold that the recorded disinclination to hire them arises because the various ‘indigenous’ white populations have, on average, superior employment characteristics. The common characteristic of Asians, Arabs and African-descendant groups is that they are not white... White immigrant groups (for example, Italians, Greeks) encountered discrimination in some studies, but never at a level comparable to non-whites” (p. F499, F503).

⁵⁷ Baert et al. (2015), in a study of discrimination against Turks in Belgium, find less ethnic discrimination in what they term “bottleneck” occupations, where vacancies take longer to fill (and which tend to be higher skilled). This does not necessarily speak to changes in discrimination with the business cycle. It could, for example, be related to the importance of a better match in some occupations than others, generating both longer-duration vacancies and less tendency by employers to discriminate – paralleling the evidence on Chinese job boards indicating that, for higher-skilled jobs, employers are less likely to express preferences based on worker demographics (Kuhn and Shen, 2013).

Of course, we cannot decisively rule out statistical discrimination based on this observation. Nonetheless, the argument is somewhat compelling.

Similarly, the findings on sex discrimination are hard to square with statistical discrimination. The usual argument is that employers engage in statistical discrimination against women based on a higher likelihood of leaving the firm, taking on family responsibilities, etc. It is not clear, then, why the evidence from AC studies points to discrimination against men in typically-female jobs, and vice versa, rather than discrimination against women generally, although it is in principle possible that these stereotypical female behaviors are for some reason more compatible with female jobs, and similarly for male behaviors and characteristics and male jobs. But that would seem to lead to a rather tortured argument about statistical discrimination, and in this case, coupled with the evidence on race and ethnicity, Occam's Razor might well argue for a simpler taste-based discrimination model – adapted in the case of hiring discrimination by sex to have more to do with violating stereotypes or norms than disutility from hiring women.

Some AC studies try to test for statistical versus taste discrimination more directly, by manipulating the amount of information provided. Rich (2014) concludes that the evidence is inconsistent across studies, with some finding that including more information eliminates group differences (e.g., Kaas and Manger, 2011), whereas many do not. As explained above, however, these tests may not be informative, and the same hypothesis can generate different results in different studies. Other studies report evidence that authors argue are not consistent with either model. For example, Bertrand and Mullainathan (2004) suggest that their evidence of similar magnitudes of race discrimination across occupations is hard to reconcile with statistical discrimination because factors such as the importance of unobservable skills and the ease of observing relevant characteristics should vary across occupations. But they also note results that are hard to reconcile with some taste-based models – for example, the similarity of results across occupations with different degrees of customer or co-worker contact. Rich (2014) claims that, across studies, measured discrimination does not vary with customer contact, although no formal analysis is presented.

Carlsson and Rooth (2012) combine data from their correspondence study in Sweden with

municipal-level survey information on attitudes towards immigrants. In evidence they interpret as consistent with taste discrimination, they find that the call-back rate for Middle Eastern minorities was lower in municipalities where respondents had more discriminatory attitudes. However, the attitudinal question refers to the value of immigrants in contributing to the Swedish population. It is not entirely clear what this means, but is possible that the productivity of the immigrant population relative to non-immigrants differs across municipalities, in which case the reported differences in attitudes could reflect these population averages, and the relationship with call-back rates could reflect statistical discrimination. Attitudinal questions focused more on discriminatory tastes or preferences would likely be more convincing.

Zschirnt and Ruedin's (2015) meta-analysis focuses on statistical versus taste discrimination, presenting three types of evidence. First, they suggest statistical discrimination should be lower for second-generation than for first-generation immigrants, because more information is available about the former; for example, more of the qualifications (like schooling) are local. They find some evidence of lower discrimination against second-generation immigrants, although the estimates point to substantial discrimination for both groups and the difference is not large. In addition, there are other reasons measured discrimination may change across immigrant generations, such as increased proficiency in the native language, which may not be adequately controlled in a correspondence study. And it is not clear why taste discrimination cannot decline for more assimilated second-generation immigrants.

Zschirnt and Ruedin suggest that evidence of stronger measured discrimination against "more distant and visible minority groups" (p. 5) would be indicative of taste discrimination. Their analysis suggests the lowest call-back rates for Arabs and those of Middle Eastern origin, followed by Indians, Pakistani, and Bangladeshi as well as Chinese, with Turks having call-back rates that are closest to natives, but still lower. Again, though, it is difficult to distinguish taste from statistical discrimination, as group differences or availability of information about these groups may also differ.

Third, the authors note that in German-speaking countries, far more-detailed information is included as part of job applications (like diplomas and transcripts), and hence it is less likely that employers

need to resort to statistical discrimination. Thus, smaller measured discrimination against minorities in these countries would be consistent with statistical discrimination. Their evidence points to lower measured discrimination for minorities in the German-speaking countries, although the remaining difference in German-speaking countries, despite the extensive information provided, would suggest that taste discrimination remains important. Echoing the earlier discussion, however, this source of variation is only informative if the studies in each country provide the information employers typically have. Another limitation of this evidence is that it does not condition on the ethnic group, whereas a difference in the German-speaking countries for the same ethnic group relative to natives would be more informative about the difference between the two sets of countries owing to differences in the application process. The authors also suggest that much weaker evidence of hiring differences in the public sector is consistent with statistical discrimination, given that the public sector uses more formal and detailed applications. Of course, the public sector may simply engage in less discriminatory behavior as a matter of policy.

A recent example of the “information-treatment” test for statistical discrimination is Oreopoulos (2011), who studies discrimination against immigrants in a correspondence study in Canada. Some of his analyses are potentially more informative because they implement this test across jobs and skills for which we might expect statistical discrimination to be less important or more important, while the other limitations of this test that I have described are, in a sense, held constant. One approach is varying whether an applicant with a foreign name shows Canadian education and experience or foreign education and experience. The latter may provide a noisier signal, although the domestic education or experience could have different value in terms of productivity. Oreopoulos also compares results for which relatively more or less of the required skills might be inferred from immigrant status. For example, in sales jobs language might matter a lot, but in computer programming it should not. He does not find consistent evidence pointing to statistical discrimination from adding information related to important skills. For example, listing English or French fluency increases call-back rates for foreign-educated and experienced applicants but not foreign-named applicants. And he does not find the expected relationship between information about language skills and the skills required on the job, even though some recruiters who responded to a

survey said that a foreign name is viewed as a signal of a lack of critical language skills.

Finally, Rooth (2010) tried to test for implicit discrimination in the context of a correspondence study, by going back to the recruiters from his earlier correspondence studies of discrimination against Arab-Muslim men in Sweden and administering the IAT to some of them. The evidence shows that those with a higher IAT (more discriminatory) were less likely to call back Arabs. This result is valuable, as it appears to provide some validation of the IAT in the real world.

Rooth then includes three explicit discrimination measures in the call-back model. The evidence indicates that recruiters have explicit discriminatory attitudes. For example, in the data from one experiment, 54 percent say that they prefer hiring a Swedish male to an Arab Muslim male. Rooth finds that these explicit measures do not account for the lower call-back rates associated with a higher IAT. He concludes from this evidence that “there are recruiters who implicitly discriminate, but who would not explicitly do so” (p. 529).

This conclusion may not be warranted, however, because the explicit measures are unconditional. For example, the hiring preference measure asks “which group, native Swedes versus Arab-Muslim, they prefer when hiring people,” and the performance stereotype questions has alternatives like “Swedish men perform much better at work than Arab-Muslim men” (p. 527). The responses can therefore reflect actual differences in productivity, rather than how a recruiter would treat two candidates with equal qualifications. The unconditional nature of the explicit discrimination measures seems likely to lead to understatement of the effects of these measures, because recruiters who would prefer a Swede unconditionally might not have that preference conditionally (on the characteristics for which the study controls), so the explicit measures may misclassify recruiters with regard to their explicit biases in the conditional experiments. That is, the correlation of the IAT with explicit *conditional* discrimination measures might be much higher. Nonetheless, the idea of combining data from AC studies with the IAT is very creative, and future work could likely make more definitive use of such information.

Finally, in a recent paper, Bartoš et al. (2014) propose a model of statistical discrimination that endogenizes effort devoted to learning about applicants (and hence in a sense might be viewed as blending

statistical and implicit discrimination). They consider costs and benefits of acquiring information about applicants to model how much attention employers will pay to applications from different groups and in different settings. The model predicts that in highly selective markets, employers will pay less attention to applications from groups with lower qualifications. The authors test this prediction in correspondence studies using Czech and German data with native and ethnic minority names. In the Czech study, they measure “attention” by modifying the research design to force employers to click on hyperlinks in emails to see resumes (having seen the name that signals minority status in the email), and then in the resume embedding “learn more” buttons (like clicking on “Experience” to see more about the applicant’s job responsibilities). The authors find evidence of discrimination against minorities (Roma and Asians) similar to the many studies already discussed. They also find evidence that employers pay more attention to the non-minority resumes, in terms of both opening resumes and looking at more information – although these differences are generally not significant in their very small sample.

In the German study a different design is used, in which employers have to open a resume, but then an error is generated and they have to take additional effort to try to see the resume. In this case, no information on call-backs is obtained, since the resume is never provided, and the authors obtain information only on information acquisition, for which they find less effort to get resumes of minority (Turkish) applicants.

8.5.3 *Studies Addressing the Heckman Critique*

My 2012 paper applied the method for addressing the Heckman critique to the data from the Bertrand and Mullainathan (2004) study of black-sounding names, exploiting data on skills included on some of their resumes – which do help predict call-backs. The estimated overall marginal effects of race from the heteroskedastic probit model indicate callback rates that are lower for blacks (black-sounding names) by about 2.4-2.5 percentage points, relative to a 9.65 percent call-back rates for whites. The estimated variance of the unobservable is larger for blacks (although the difference is not statistically significant). As a result, decomposing the marginal effect, the effect via the level of the latent variable, which captures discrimination as conventionally defined in AC studies, is larger than the marginal effect

from the probit estimation, ranging from -5.4 to -8.6 percentage points. The effect of race via the variance of the unobservable, in contrast, is positive, ranging from 2.8 to 6.2 percentage points (although this effect is not statistically significant). The point estimates imply that, in these data, the bias from different variances of the unobservable leads to understatement of race discrimination (presumably reflecting both the higher variance for blacks and standardization of the resumes at a low quality level).

Carlsson et al. (2013) re-examine data from four previous studies of the Swedish labor market by some of the co-authors, each of which includes some form of the data required to implement this method. Their re-analysis does not lead to large changes in the estimates of discrimination, and sometimes the estimated discrimination (against those with Arabic names, and in favor of women) becomes smaller – which contrasts with the findings from the Bertrand and Mullainathan data. Baert et al. (2015) implemented this method in a study of discrimination against Turkish school-leavers in Belgium, using information on distance from the worker’s residence to the workplace and other application characteristics to identify the heteroskedastic probit model, and report that this correction does not alter the conclusions. They find a higher variance of the unobservable for Turks, and a larger estimate of discrimination, consistent with a low level of standardization; again, the difference in variances is not statistically significant.⁵⁸

Two of the three studies that use this method find a higher variance of the unobservable for minorities, consistent with early models of statistical discrimination. And in these cases, the estimates point to understatement of discrimination when the variances of the unobservables are assumed to be equal. And none of the studies indicate that ignoring the Heckman critique leads to strong overstatement of discrimination. On the other hand, in each case considered, equal variances are not rejected, sometimes owing in part to imprecise estimates of the effect of group membership on the variance of the unobservable. Yet the precision of the estimate of discrimination does not decline, so there is little cost to estimating the more flexible model to recover unbiased estimates of discrimination.⁵⁹

⁵⁸ Baert (2015) does a similar analysis for a field experiment on sex discrimination, and finds no effect of this correction on estimates of discrimination.

⁵⁹ This same kind of finding is reflected in applying this technique to age discrimination, discussed below.

8.6 Age

8.6.1 Summary of Findings

A number of studies have applied AC methods to age discrimination. In general, this work follows the paradigm used in studies of discrimination against other groups, such as blacks or women, making applicants identical (up to random variation) in all respects except age. However, there is an issue of how to treat potential age-related differences in experience, discussed below.

The main studies almost uniformly find evidence of age discrimination in hiring.⁶⁰ For example, Bendick et al.'s (1997) correspondence study looks at 32 and 57 year-old male and female applicants (signaled by year of graduation). Among applications in which at least one of the two applicants received a positive response, in 43 percent of cases only younger applicants received the positive response, versus 16.5 percent of cases in which the older applicant was favored, for a statistically significant net discrimination measure of 26.5 percent.

This study had other interesting features. First, the authors sent applications to a large list of companies that they could identify (without reference to job postings). Drawing on other information sources, they could study how firm characteristics such as firm size or industry affect the outcomes of their tests. For example, they find more evidence of discrimination in the largest firms; this contrasts with other research on discrimination (e.g., Holzer, 1998), but that may be because this other research contrasts very small firms that may be exempt from anti-discrimination laws.

A second feature was to vary the extent to which resumes de-emphasized age (by focusing on skills rather than history), that highlighted experience and maturity, or that tried to combat age-related stereotypes (by adding to the cover letter a phrase about being “energetic, adaptable to the latest technology, and committed to my career”). Measured discrimination was about half (but still present) for the applications de-emphasizing age or countering ageist stereotypes. The finding on stereotypes might be viewed as evidence of statistical discrimination against older workers based on assuming they are less adept at new technology, or are likely to retire soon. In light of the earlier discussion of the potential limitations

⁶⁰ In this section, I exclude a few studies that focus on age differences but do not include age ranges covering older workers (in their 50s or 60s). See the discussion in Neumark et al. (2015).

of tests for statistical discrimination, this evidence might be viewed as relatively more compelling, since employers likely do care about these things (recall the evidence from Rosen and Jerdee, 1977) and not be able to glean it directly from job applications.

Bendick et al. (1999) perform an audit study meant to provide more evidence on age discrimination than can be obtained from a correspondence study. One goal was to capture more than just whether the callback was positive, such as whether, when both testers received positive responses, one was treated more favorably with regard to getting an interview, an opportunity to demonstrate skills, a job offer, or a job offer with higher compensation. The percentage of tests with a more favorable response for younger applicants (age 32) was 42.2 percent for age 32, versus 1 percent for older applicants (age 57), and the evidence points to favorable treatment of younger workers, among pairs offered jobs, with respect to salary and health insurance benefits.⁶¹

Another feature of this study is to document differences in outcomes at different stages of the process. Echoing the earlier conclusions from studies of ethnic and race discrimination, about three-quarters of the discriminatory difference in treatment is at the pre-interview stage. The study also reports on some assessments of phone calls and interviews by the testers, including attempts to gauge whether employers expressed age-related stereotypes, finding that this was rare. Finally, the study presents some information on characteristics of the positions offered, with some indication of less attractive positions offered to older applicants (e.g., lower commissions on sales, part-time work, etc.), although any such conclusions are based on a handful of observations.

Lahey's (2008) correspondence study focuses on women only (for reasons discussed more below). She uses five different ages ranging from 35 to 62, and finds consistent evidence of age discrimination, whether using age as a continuous variable or classifying workers as older (50/55/62) versus younger (35/45). Lahey tries to address the question of statistical discrimination by including language on older resumes intended to combat adverse stereotypes of older workers (like a statement about willingness to embrace change in the resume), information on an attendance award (to combat expectations of greater

⁶¹ Details in the paper are scant, including statistical tests for each dimension of job offers.

absences among older workers), a statement about not needing health insurance (to counter expectations of higher health insurance costs), and information about computer skills (to combat fears of technological obsolescence). There were not clear indications that resume features designed to counter negative age-related stereotypes consistently helped older applicants.

Lahey also tries to assess the importance of employer, employee, or customer discrimination. She tests for employer discrimination by assuming that a firm with a human resources department would exhibit less taste-based discrimination (although it is unclear why the department would not act at the owner's discretion), but finds evidence (not significant) suggesting a negative interaction with applicant age. She tests for employee discrimination by asking whether the age composition of the workforce in the employer's PUMA is related to variation in outcomes with age, but finds no relationship (acknowledging that this is a noisy measure and hence imprecise test). Finally, she does a related test for customer discrimination by asking whether the age composition of the population is more important in occupations with more customer contact; again, she finds no evidence of a relationship.

Three closely related studies, Riach (2015) and Riach and Rich (2006 and 2010), investigate age discrimination in applying for jobs as waiters (and, in one case, as retail managers). One paper covers England, one covers France, and Riach (2015) covers four countries, adding Spain and Germany.⁶² In all instances but one (applicants for jobs as retail managers in Riach and Rich (2010), who were female only), there is strong evidence of discrimination against older applicants. This one instance is the only one I am aware of in the research literature that finds evidence of discrimination against younger workers. However, for correspondence studies, the samples in all of these studies are quite small; there are only 300 retail manager applications, and net discrimination measure is based on 24 applicants.

8.6.2 *Specific Issues in Using Correspondence Studies to Test for Age Discrimination*

Strict application of the paradigm of AC methods to studying age discrimination requires giving older and younger applicants the same low level of experience (commensurate with the young applicants' ages), because clearly a young applicant cannot have the experience of a long-employed older worker.

⁶² In Riach (2015), the data for England and France appear to be the same as in the other two papers, even though they are not cited. And the evidence for female applicants in England (discussed below) is not reported in Riach (2015).

This could make older applicants look unusually unqualified relative to the older applicants employers usually see, which could generate a bias in AC studies towards finding evidence of age discrimination. Researchers have addressed this problem in different ways, but they may not have eliminated this bias.

Bendick et al. (1997) had older and younger applicants report 10 years of experience on their resumes. To account for the gap in older applicants' job histories, the resumes indicated they had been out of the labor force raising children (for the female executive secretary applications), or working as a high school teacher (for the male or mixed applications). Bendick et al. (1999) are vaguer, but note that all applicants reported several years of experience in an occupation related to the job to which they are applying, and older applicants also reported additional years of experience in an unrelated field. Lahey (2008) includes only 10-year jobs histories for all applicants, although she focuses on women, for whom time out of the labor force may be less of a negative signal than for men. She also cites evidence from conversations with human resources professionals who said 10-year histories are the "gold standard" for resumes and would not convey a negative signal for older applicants. Still, across all studies it is a matter of speculation whether experience in unrelated fields or time out of the labor force negatively affected employers' assessments of older applicants, generating spurious evidence of age discrimination.

Riach and Rich (2006, 2010) criticize the approach of using unrealistic resumes for older workers, and give their applicants experience commensurate with their age. Most of their findings (with the one exception noted above) still find strong evidence of age discrimination, suggesting that low experience does not drive the finding in other studies. However, in their sample resumes, the description of experience is quite cursory, and not a full job history.⁶³ Results could differ with full job histories for older workers showing experience commensurate with their age.

In Neumark et al. (2015), we provide a comprehensive examination of this question in a very large-scale correspondence study of age discrimination. We argue that the more relevant policy and legal question is relative hiring of older workers and younger workers each with experience commensurate with

⁶³ In the 2006 paper, the resumes simply say that the person has worked as a server in restaurants since about age 20. In the 2010 paper, one resume lists three jobs held since age 17, rising to Senior Waiter, and the second has a paragraph description of the career since leaving school, again with rising responsibility.

their age. With regard to policy, for example, concerns expressed following the Great Recession about long unemployment durations of older workers focused on typical older workers who lost jobs during the recession. And from a legal perspective, it is pretty clear that employers passing up inexperienced older workers for younger workers would find it easier to defend their actions as non-discriminatory.

The study drew on a comprehensive database of resumes to construct different kinds of older-worker resumes showing either experience that matched that of younger applicants, or experience commensurate with age, to see whether there was a difference. For women, perhaps consistent with Lahey's (2008) conjecture, the outcomes relative to younger applicants did not depend on whether older applicants' resumes showed matched or commensurate experience. For men, however, in the one of the two occupations in which there was some evidence of age discrimination, the results were fully driven by the low-experience resumes for older workers. Thus, there is some evidence that using low-experience resumes in age discrimination AC studies is problematic, at least for men (for whom, for older cohorts, there may be more of an expectation of a fairly continuous work history).

The second issue we emphasize is differences in the variances of the unobservables. As noted above, this problem may arise quite naturally in studying age discrimination from accumulated differences in human capital investment as workers age, although how relevant this is for the lower skilled jobs these studies cover is an open question. The correspondence study was designed to include multiple skill variables that could shift call-back probabilities, and to then use these variables, in a heteroskedastic probit model, to estimate the relative variances of the unobservables and obtain an unbiased estimate of age discrimination. Paralleling the results on experience, the findings for women were robust to doing this; nothing diminished the rather strong evidence of age discrimination against older women. For men, however, the evidence was not robust to this correction.

The findings in Neumark et al. (2015) provide decisive evidence of age discrimination against older women.⁶⁴ A recent U.S. correspondence study by Farber et al. (2015) provides corroborating

⁶⁴ We also find a hint of evidence that there is less age discrimination in states with age discrimination laws that are stronger than the ADEA. We have work in progress that extends this study to all states, to get more evidence on this question.

evidence. The study focuses more on the effect of unemployment duration than on age discrimination, but finds evidence of lower call-back rates for women aged 55-58 (compared to 35-37 and 40-42) who apply to administrative support jobs (one of the jobs in Neumark et al., 2015).

In contrast to the earlier studies, however, the innovations in Neumark et al. (2015) raise doubts about the conclusion that older men suffer much age discrimination; certainly the finding is not robust. We speculate that this could reflect two factors. One is that anti-discrimination laws in the United States do less to protect older women who may suffer from both age and sex discrimination. Because the law that protects women (Title VII of the Civil Rights Act) is separate from the law that protects older workers (the ADEA), “intersectional” claims based on stronger age discrimination against older women are difficult to bring before the courts (Song, 2013; Day, 2014). Second, older women may experience more discrimination than older men because physical appearance matters more for women (Jackson, 1992) and age detracts more from physical appearance for women than for men (Berman et al., 1981) – consistent with the shift in relative preference away from women with age that Kuhn and Shen (2013) find.

8.7 Other Groups

AC studies have been conducted for many other groups of workers – some protected by anti-discrimination laws (e.g., the disabled), and some not (e.g., the less attractive). There is typically only a small number of studies for each such group. I review some of these studies briefly, focusing more on some of their interesting features, especially those that might prompt additional research.

8.7.1 Looks and Obesity

I discuss experimental research on looks and obesity together. Although there are some observable outcomes associated with obesity that employers may care about and may be health related, it is plausible that negative visual appearance is a common factor in both of these dimensions. Indeed, Rooth’s (2009) correspondence study of obesity in Sweden explicitly combines analysis of the two dimensions. Rooth signals obesity by digitally manipulating photographs – which are often included in job applications in Sweden – to change a non-obese applicant to obese. For men and women, obesity lowers the call-back rate by about seven to eight percentage points, and the sign is in this direction for all occupations but nurses.

The study also incorporates attractiveness ratings of the photographs, including the digitally altered ones. Attractiveness is rated lower for the obese applicants, and pooling across occupations, the attractiveness rating explains the obesity differential for men, but not for women. However, the two measures are highly correlated, so it is hard to distinguish the effects of looks and obesity statistically or conceptually. Moreover, across occupations the relative magnitudes of the two characteristics of photos are not robust. The obesity penalty is larger in some jobs requiring customer contact (such as sales), which may reflect customer discrimination. But the occupation-specific estimates are imprecise, and the pattern is not consistent (e.g., a large penalty for female but not male accountant applicants).

Focusing just on obesity, Agerström and Rooth (2011) use the same procedure as Rooth (2010) to test for implicit biases against the obese, by administering the IAT to some recruiters from Rooth's (2009) correspondence study of obesity discrimination, and asking about explicit discrimination preferences and obesity-related stereotypes regarding overall work performance. The results are similar to the Arab-Muslim discrimination study. Recruiters with a higher IAT (more discriminatory toward the obese) were less likely to call back obese applicants, controlling for the explicit measures did not change this result, and the explicit measures were not significantly associated with call-backs. However, the criticisms of the interpretation of the results based on the explicit measures discussed earlier apply here as well.

Two studies focus on looks in isolation. Bóo et al. (2013) study Argentina. They use photographs that are digitally manipulated to alter what they say are validated measures of facial beauty based on horizontal distance between the eyes relative to a face's width, and vertical distance between the eyes and mouth relative to a face's length. They find that call-back rates for the attractive candidates are about one third higher, and the effect is similar for men and women. However, when they have students rank the photos, they get high correlations of rankings with the distance measures, but the subjective rankings do not predict call-backs. This raises the question of what is being manipulated in the experiment, since we might expect student ratings of looks to line up to some extent with employers' perceptions.

Ruffle and Shtudiner (2015) do a correspondence study of lookism in Israel, where including a photo is discretionary, using subjective ratings of attractiveness by a panel of working people. They find

higher call-back rates for more attractive men, but not women. They also send out applications with no photos, and among women, these get the highest call-back rate, whereas among men the no-photo applications get higher call-back rates than the non-attractive applicants. These kinds of results raise questions about which real-world applicants actually send out photos and hence about the external validity of the findings in countries where including a photo is discretionary.⁶⁵ In contrast to the Kuhn and Shen (2013) findings, for men it appears that looks matter more in jobs requiring experience than in jobs not requiring experience, suggesting that employers may be using looks as a source of information about productivity, rather than indulging tastes for good looks where skills are not so important. As in other studies, the authors test for differences based on interaction with customers. For neither men nor women does the treatment based on looks vary with customer contact.⁶⁶

Finally, Galarza and Yamada (2014) conduct a correspondence study in Peru of discrimination based on looks and race (signaled via photos and names). The photos are manipulated to be similar in terms of clothing, background, etc., and are rated by a group of professionals from various fields (they sound like academics); they include both men and women. Looking only at race (indigenous origins), the authors find strong evidence of racial discrimination in all types of jobs. However, white applicants are rated as better looking than indigenous ones, raising the question of whether the racial discrimination in call-backs reflects lookism or something else. The authors suggest that this raises the difficult question of whether looks should be judged (normalized) for both groups together, or the two racial groups separately. However, it seems that only the first approach can address the question of whether racial discrimination and lookism are confounded. Using this approach, they find a significant beauty “premium” in call-back rates, and that looks explain about half the racial gap in call-backs. In professional jobs, there is only a difference associated with looks under this approach, and no racial difference. In technical and in unskilled jobs, neither appears to matter when both are included in the model. Among the unskilled jobs (in results the

⁶⁵ The authors report some results from asking their subjective raters of looks whether they would attach the picture to an application if it was their picture, and ask employers which types of applicants tend to attach a picture to their resume and what message applicants convey by including a photo.

⁶⁶ The authors present some evidence that they interpret as suggesting that the disadvantage or lack of advantage for attractive female applicants, in their data, may stem from jealousy or envy among females who evaluate job applications.

authors only describe), looks seem to matter for those with customer contact (sales and marketing, and restaurants), but not in non-contact jobs (e.g., call centers and drivers), consistent with customer discrimination that presumably makes good-looking employees more productive in such jobs.⁶⁷ This study, like Rooth (2009), raises the question of how to interpret the effects of looks and other physical characteristics that seem to be associated with looks.

8.7.2 *Women with Family Responsibilities*

Statistical discrimination against women in labor markets may stem from employers' expectations that will leave their job, perhaps to have or care for children. Duguet and Petit (2005) and Petit (2007) report results from a correspondence study for jobs in the financial sector in France intended to test this hypothesis.⁶⁸ They do not use the information-treatment approach, presumably because it is hard to imagine a credible direct signal of future turnover or childbearing intentions. Instead, they use groups with different likelihoods of future childbearing or childrearing responsibilities. In particular, they have three types of both male and female applicants: age 25, single and childless; age 37, single and childless; and age 37, married with three children. Women in the first group are those for whom employers are most likely to expect future childbearing. Consistent with past studies, they do not find significant overall differences in call-back rates by sex. Nor, for all jobs combined, do they find differences for the three kinds of applicants. They do find, however, that for the jobs with higher qualifications or that offer training – jobs in which turnover costs may be higher – the women who are 25, single, and childless receive fewer call-backs than comparable men. In contrast, for the 37 year-old applicants there is either no sex difference or, in one case, a difference favoring women. This evidence is consistent with statistical discrimination against women with regard to future childbearing.

In the Correll et al. (2007) study discussed earlier, the evidence from a lab experiment is coupled with evidence from a correspondence study. The design of the correspondence study was similar to the lab

⁶⁷ There is also an attempt to ask whether the racial discrimination reflects looks or something else, prompted in part by the finding that whites are ranked as better looking. It is not clear, though, that these two things can be distinguished.

⁶⁸ The data are the same in the two papers, and the analysis and results nearly the same; I describe the results from the first paper.

experiment with respect to the job, with a male pair and a female pair (signaled by names) in each of which one was a parent and one was not, and similar types of marketing positions to those used in the lab experiment were targeted. The results indicated no significant difference in call-back rates for men dependent on parental status, but a call-back rate for childless women 1.8 times that for mothers. Given that parental status was signaled as being a Parent-Teacher Association Coordinator, and applicants had about seven years of experience, the applicants were reasonably interpreted as having young children. Marital status does not appear to have been indicated, so it is possible that had the non-parent females been described as single, the gap would have been even larger because an assumption about low future childbearing might also have been made.

8.7.3 *Criminals/Criminal Records*

Perhaps the least surprising result among field experiments on labor market discrimination is that applicants with criminal backgrounds get fewer call-backs. Pager's (2003) audit study of black and white applicants in Milwaukee that, among whites, call-back rates were lower by 50 percent for those with criminal backgrounds (a felony drug conviction and 18 months served), and among blacks call-back rates were lower by 71 percent. The researchers did not have the testers go beyond the first contact, so they report results for whether they were invited for interviews (call-backs). They chose an audit rather than correspondence study design – despite only studying call-backs – so that race could be signaled directly via direct employer contact with the testers.⁶⁹ Strikingly, perhaps, the call-back rate for the white tester with a criminal background record is higher (not significantly) than that for the black tester without a criminal background. However, there were only four testers, paired by race, so the comparisons by race do not come from a within-pair design.⁷⁰

Baert and Verhofstadt (2015) carried out a correspondence test in Belgium, using as applicants

⁶⁹ Like in age AC studies, there is an issue of missing labor market experience, attributable to the 18 months spent in prison. Pager addresses this by have the testers show some work experience in prison, and having those without criminal backgrounds graduate later. Still, there could be an experience differential that matters for the outcomes, although my sense is this is unlikely to explain much of the difference associated with criminal background.

⁷⁰ Pager et al. (2009) extend this study (doing it in New York) to include Latino applicants as well, using alternative assignments of testers that permit matched-pair comparisons between non-minority ex-offenders and minorities without a criminal background. The white ex-offenders have modestly higher (though not statistically significant) call-back rates than non-offender blacks and Latinos.

male school leavers applying to a number of occupations, some of whom indicated a period of juvenile delinquency (spending time in a detention center). The applicants indicating this history also reported having a certificate of good conduct. The call-back rate for control applicants who did not report a criminal background was 29 percent (four percentage points) higher. The difference is more pronounced for males, and small and statistically insignificant for females.

Of course, lower call-backs for those with a criminal background should not necessarily be viewed as discrimination, because criminal background signals relevant information that employers not only may care about but perhaps should care about, given that they can be held liable for an employee's actions and accused of negligence in hiring, which a background check does not necessarily protect against (e.g., *Ponticas v. K.M.S. Investments*, 1983).⁷¹ In neither country is discrimination against those with a criminal background explicitly illegal, although some U.S. states are trying to increase employment protections for some workers with criminal backgrounds (Pager, 2003).⁷² Nonetheless, the results do emphasize the challenge of re-integrating those with criminal backgrounds into the labor market.

8.7.4 Gays and Lesbians

The research literature using AC methods to study discrimination against gays and lesbians is, perhaps not surprisingly, more recent. Perhaps reflecting this, the studies in this area present some interesting and unusual features, some of which – such as trying to learn about wage offers in the context of these studies – could perhaps be fruitfully emulated and extended in AC studies more generally.

Weichselbaumer (2003) conducted a large-scale correspondence study of discrimination against lesbians in Austria. Her analysis is motivated in part by the evidence of a wage penalty for gays but a wage premium for lesbians, relative to same-sex heterosexuals. Weichselbaumer discusses a number of hypotheses that might explain why lesbians earn a wage premium despite evidence that, like gays, they self-report discrimination in the workplace, and that surveys (at the time of this research) indicated negative attitudes towards both gays and lesbians, including in the workplace. First, the wage premium may be

⁷¹ Despite this, Baert and Verhorfstadt strongly interpret their results as reflecting discrimination (e.g., p. 1070).

⁷² The EEOC does warn that discrimination on the basis of criminal background can lead to racial or ethnic discrimination given high incarceration rates for blacks and Hispanics (http://www.eeoc.gov/laws/guidance/arrest_conviction.cfm#I, viewed January 18, 2016).

confined to more “masculine” lesbians, if this masculinity is valued in the workplace. Second, the adverse effects of discrimination may not be reflected in wages because few lesbians are “out” at work, whereas a correspondence study that manipulates sexual orientation would reveal discrimination. Third, lesbians may specialize less in home production, including having fewer children, boosting wages despite discrimination.

The study submitted applicants for secretarial and accounting jobs, using the detailed applications (including photographs) typical in German-speaking labor markets. Masculinity or femininity is conveyed via the photos, and the study used a group of student raters to confirm that the photos conform to these stereotypes without influencing overall ratings of desirability. The stereotypes were also reinforced via hobbies and past experience on the resumes, although it is possible that these were more strongly correlated with productivity/desirability. Sexual orientation is signaled via work for a gay rights organization, balanced in the heterosexual sample by work for other organizations.⁷³ The study successively sends two pairs of applicants to employers in such manner as to first provide information on the effect of masculinity among straight women, and then on sexual orientation. All women describe themselves as single.

Lesbian women received substantially fewer call-backs, and this was unrelated to masculinity or femininity.⁷⁴ Weichselbaumer argues that statistical discrimination is not likely to underlie the results, for two reasons. First, as later emphasized in Zschirnt and Ruedin (2015), the detailed applications in German-speaking labor markets leave less scope for this kind of discrimination. Second, because lesbians are raised in the same kinds of families and environments as heterosexuals, there is less reason to expect unobserved differences like we might assume disadvantage minorities. Third, because of the lower likelihood of having children (especially in earlier years) and of household specialization, if anything we might expect

⁷³ Baert’s (2014a) correspondence study of discrimination against lesbians in Belgium signals sexual orientation by having all applicants married, with the lesbian applicants listing a female spouse (based on first name), versus listing no spouse. Baert suggests that other studies’ use of membership or activism in a gay rights group may signal activism or even radicalism. However, the other studies usually assigned a similar kind of activism (e.g., in an environmental group) to the controls, so it is not clear why this method should accentuate discrimination against lesbians. Weichselbaumer (2015b) compares results using these two approaches (in Germany, using lesbians in “registered partnerships” to compare with married heterosexual women), and finds little difference in the call-back gap between single straight and lesbian women signaling sexual orientation via volunteer activities, and married straight and registered-partner lesbian women using marriage/partnerships to signal. Of course this design may confound the effect of differences in signaling sexual orientation and differences in the effects of orientation between single and married/registered women.

⁷⁴ Weichselbaumer (2004) uses these same data to study sex discrimination among heterosexuals, concluding that it is not driven by whether women have more masculine or feminine identities.

statistical discrimination to favor lesbians over heterosexual women.

Drydakis (2009) used a similar strategy to study discrimination against gays in Greece, although without attention to gender stereotype. Rather, to counter gay stereotypes regarding femininity, the gay and straight resumes included similar hobbies that, according to the author, implied similar masculinity. He finds strong evidence of hiring discrimination against gays, with call-back rates lower by about 25 percentage points. The author also had fictitious applicants field return phone calls to try to get information on wages offered. The paper only reports wage regressions for pairs where both applicants received wage offers, and in these cases there was no detectable wage offer difference. In Drydakis' (2011) very similar study of discrimination against lesbian women in Greece, which finds much lower call-back rates for lesbians, he reports all of the evidence on wage offers, finding a wage penalty for lesbians exceeding eight percent in the sample where only one or the other gets an offer, but only 4.8 percent in the matched sample where both get call-backs. These findings suggest that the finding of equal wage offers for gay men in the first study may reflect the selection on pairs where both men received call-backs.

The wage evidence for women contrasts with the usual finding from non-experimental data that lesbian women earn a wage premium. This could be due to differences in whether sexual orientation is known to the employer (as Weichselbaumer (2003) suggested), differences in wage offers measured by fielding phone calls versus actual accepted wage offers, or differences in wages based on gender stereotype (the resumes in this study did not particularly try to signal more masculine stereotypes).

Tilcsik (2011) presents the first large-scale correspondence study of discrimination against gay men in the United States, which includes some unique and potentially valuable features. First, it looks at variation in whether there is a law barring discrimination based on sexual orientation (looking at the seven states the study covers, as well as city and county laws).⁷⁵ Second, Tilcsik examines the association between state-level measures of attitudes towards gays and outcomes. Third, the study uses text from ads to identify those requesting stereotypically male traits, to see if gays are less likely to get call-backs for these ads – presumably because of more feminine stereotypes, although Tilcsik does not manipulate the

⁷⁵ These may be more important than state variation in age laws, since there is a national age discrimination prohibition that applies to all states.

resumes to generate variation in masculinity among applicants.⁷⁶

The study finds lower call-backs for gay applicants, with an overall call-back rate of 11.5 percent for straight applicants and 7.2 percent for gay applicants. Tilcsik's multivariate analysis suggests that there is some evidence that anti-discrimination laws reduce discrimination, although the evidence does not uniformly point in this direction, and Tilcsik is asking a lot of the data with only seven states. There is also some evidence that public support for gay rights is associated with less discrimination, although it is hard to untangle this from the effect of laws that are passed. The evidence also suggests that gays were less likely to receive call-backs from employers with ads listing stereotypically male traits.

Ahmed et al. (2013) conducted a correspondence study in Sweden of discrimination against gays and lesbians. This study signals sexual orientation via volunteer work in an organization, and there was no manipulation of gender identity. The authors focus on many occupations, and, interestingly, find evidence of discrimination against gays only in male-dominated occupations, and against lesbians only in female-dominated occupations. This evidence may imply that discrimination based on sexual orientation depends more on gender stereotypes than on sexual orientation per se, although that may be more true in a country like Sweden with less negative attitudes towards gays and lesbians than other countries (and indeed the overall evidence is weaker than in other countries).

Finally, Drydakis' (2014) correspondence study for Cyprus also focuses on both gays and lesbians. The methods and findings are similar to his studies for Greece, with regard to both hiring and potential wage offers. A unique feature of this study is its attempt to test for statistical versus taste discrimination via the provision, for some applicants, of additional information on grades, positive personality traits (e.g., efficient, organized), and reference letters attesting to work commitment, no absenteeism, etc. However, we should keep in mind Weichselbaumer's (2003) argument that there is no clear reason for statistical discrimination along these dimensions for gays and lesbians, which may explain the findings that discrimination against gays and lesbians was similar for the more- and less-informative applications.

The experimental literature on discrimination against gays and lesbians generally finds evidence of

⁷⁶ To do this, ads have to be matched to specific call-backs from employers. In Neumark et al. (2015) we discuss the difficulties of doing these matches for phone responses. Tilcsik does not indicate how this problem was handled.

discrimination, although there may be less discrimination in more recent studies (see also Baert, 2014a). Whether this has to do with study design, the country studied, or increased acceptance of gays and lesbians is an interesting question that can only be answered by comparing comparable studies over time and location, although there may not yet be enough studies to draw firm conclusions.

8.7.5 *Disabled*

There are a few correspondence studies of discrimination against the disabled. Ravaud et al. (1992) sent unsolicited applications to French companies, differentiating applicants by a sentence in the cover letter indicating that an applicant was paraplegic (i.e., in a wheelchair) as a result of an accident in 1982. Applicants were also distinguished by high or low qualifications based on education. The non-disabled applicants were much more likely to receive favorable responses – 1.78 times more likely for the higher-qualified applicants, and 3.2 times more likely for the lower-qualified applicants. Ravaud et al. argue that their approach rules out the possibility that the disability they study affects productivity directly, although ultimately there is no way to be definitive about this. In particular, they argue that the disability on which they focus has no other interactions (by which I think they mean with other types of physical problems or disease), and that by dating the accident to the end of general education, training would have occurred after the disability occurred. Moreover, the applications indicated that the disabled applicants (and the non-disabled) had a driver's license, suggesting no concerns about this kind of mobility.

Baert (2014b) studies disability discrimination in Belgium. The disabilities considered are blindness, deafness, and autism. The disabilities are noted in the cover letter and the resume, along with language about how the worker accommodates (e.g., a guide dog), and a statement that “my disability does not make me less productive.” The disabled candidates were about half as likely to receive a positive call-back.⁷⁷ As Baert notes, it is hard to know whether to interpret the difference as discrimination, given potential productivity differences, although he asserts that he assigned disability to occupations where disabled workers would be equally productive, perhaps after reasonable workplace adjustments (e.g., accountant for blind applicants, and carpenter for deaf applicants). The author seems to have information

⁷⁷ The study also focuses on the effect of a wage subsidy for hiring disabled workers.

on the reasons employers give for positive or non-positive call-backs (which apparently is common, and also legally obligatory in Belgium), and employers rarely point to the disability. But it is not clear that they would honestly reveal this information. Finally, Baert also implements the correction for differences in the variances of unobservables, using the identifying assumption that distance from the worker's residence to the workplace (which is significantly negatively associated with call-backs) has an equal effect for the different groups, and reports that this correction does not alter the conclusions.

Finally, Ameri et al. (2015) conducted a larger-scale correspondence study in the United States. They focus on positions in accounting, and two different disabilities – spinal cord injuries, and Asperger's syndrome – which the authors argue would not affect productivity in this field. Information on these disabilities was revealed in the cover letter, with all cover letters indicating volunteer work for a disability organization, and the disabled applicants signaling their disability directly, along with a statement that “my disability does not interfere with my ability to perform the skills needed in a finance environment.” The results point to significantly lower call-backs for disabled applicants (26 percent lower, on a base response rate to the non-disabled of 6.6 percent). The gap was larger for the more-skilled applicants, and small and not statistically significant for the less-experienced applicants. The type of disability made little difference.

Ameri et al. also provide evidence on the effects of anti-discrimination laws. Noting that both disabilities are clearly covered by the (ADA), they break out results based on whether private employers have fewer than 15 employees and hence are not covered by the ADA. Second, they explore whether employers were in states where state disability laws extend to smaller employers.⁷⁸ A variety of analyses confirm the apparent effect of the ADA at the 15 cutoff, but there is not consistent evidence of effects of state laws. It is also possible that large employers can more easily accommodate disabled workers.

8.7.6 *Long-term Unemployed*

After the Great Recession in the United States, unemployment durations increased, particularly for older workers (Neumark and Button, 2014). This raised concerns of “discrimination” against the long-term unemployed, even leading to President Obama proposing, in the American Jobs Act, to prohibit employers

⁷⁸ See Neumark et al. (forthcoming) for non-experimental evidence on this policy variation and hiring of the disabled.

from discriminating against unemployed workers when hiring.⁷⁹ These developments prompted the extension of AC studies to the difficulties of the long-term unemployed in finding new jobs, with particular attention paid to the question of what long-term unemployment signals to employers.

Kroft et al. (2013) conducted a correspondence study across 100 U.S. cities. They found that applicants with long-term unemployment spells were much less likely to receive call-backs. (Ghayad (n.d.) similarly documents lower hiring of those in long-term unemployment spells, in a U.S. correspondence study.) Kroft et al. also find that while employers are less likely to call back those unemployed for a longer spell, the stigmatizing effect of a long unemployment spell is weaker in slacker labor markets, and stronger in tight labor markets, consistent with employers viewing long-term unemployment as a more negative signal when it is easier to find jobs.

Eriksson and Rooth (2014) also find that fictitious applicants currently in long unemployment spells are less likely to get call-backs (in Sweden). These authors also study the longer-term stigmatizing (“scarring”) effect of unemployment, and find no adverse effect on call-backs of long unemployment spells in the past, suggesting that any negative signals or loss of skills from earlier spells are offset by more recent experience. This is a particularly nice application of the correspondence study technique, because sorting out the effect of past unemployment spells on current labor market outcomes poses a severe challenge of controlling for heterogeneity, especially in models for the length of current unemployment spells.

To be clear, the authors of these studies interpret this evidence in the context of understanding duration dependence in unemployment spells, rather than discrimination (and as a result I do not include the studies discussed in this sub-section in Table 3). Nonetheless, the application of the experimental methods from the discrimination literature is interesting. And the evidence in Kroft et al. (2013), in particular, fits to some extent into the broader issue of statistical discrimination, in that it seems to confirm that employers have imperfect information about applicants and, in this case, use external information to draw inferences about them.

⁷⁹ See file:///C:/Users/dneumark/Downloads/AmericanJobsAct.pdf (viewed May 15, 2014).

However, Farber et al. (2015), in their correspondence study on women in the U.S. labor market, do not find evidence of lower call-back rates for those with long unemployment durations. (Kroft et al. find this effect more strongly for women than for men, while Ghayad uses male applicants only.) Farber et al. suggest that the most likely source of the difference is that Kroft et al. cover considerably younger ages, and for younger applicants where the work history is less informative, employers may place more weight (in tight labor markets) on the length of unemployment duration. Yet they also note that another recent correspondence study on unemployment durations for the United States, by Nunley et al. (2014), focuses on relatively recent college graduates and finds no effect of unemployment duration on call-backs.

8.8 *Wages and Other Outcomes*

AC studies of labor market discrimination focus almost exclusively on hiring, as the preceding extensive review demonstrates. However, much of the non-experimental literature on discrimination is about wages, and in that literature the problem of group-related unobservables looms large. Can AC studies tell us more about labor market outcomes – in particular, whether wage offers or other benefits reflect discrimination? Audit studies can do this in principle, since testers actually receive job offers. Yet very few studies have tried to capture wage outcomes, and none have done so convincingly.

Bendick et al. (1999) report evidence of potentially worse job characteristics offered to older testers who received job offers, but based on very few observations. And Pager et al. (2009) report some evidence of “channeling” of black and Latino applicants, during the interview, to lower-level jobs than those for which ads were posted. As noted earlier, Drydakis’ (2009, 2011) correspondence studies on discrimination against gays and lesbians tried to get information on potential wage offers from call-backs, although it is not clear what these mean in the context of correspondence studies, in part because not all call-backs result in a job offer.

Finally, note that answering the question of wages offered in AC tests is different from the question raised by Heckman (1998) as to whether evidence of hiring discrimination necessarily implies that a discriminatory wage gap will result, which is a question about the effect of discrimination at the market level. It may be that the best answer to this question, to date, comes from Charles and Guryan’s (2008)

work on discriminatory attitudes and wage differentials. Nonetheless, it would be useful to know just how strongly the evidence on hiring discrimination from the large body of AC studies helps to explain wage gaps that, in non-experimental studies, are sometimes viewed through the lens of discrimination.

8.9 Anonymized Job Applications

An interesting and different kind of experimental method to study discrimination in hiring is to anonymize applicants and see if selection for interviews (and potentially hiring) is affected. This parallels the Goldin and Rouse (2000) study of orchestras, albeit with experimentally induced variation. This anonymization would presumably only work for selection for interviews. While past research has indicated that most of the discrimination seems to occur at this stage, that could change if the ability to discriminate in selecting candidates for interviews was eliminated, although perhaps not if it is easier to enforce non-discrimination in hiring from a pool of interviewees.

Krause et al. (2012) studied economics Ph.D. applicants to a European research institute, which on its own randomized the anonymization of demographic information before the applications went to the hiring committee.⁸⁰ After the hiring process was complete, applicants were asked for permission to use their data in the study; 65 percent agreed. The key result is that female applicants were more likely to be invited for interviews in the non-anonymous sample, whereas this advantage was erased in the anonymous sample. Taken literally, this means that there was discrimination in favor of female candidates in the non-anonymous setting, which could not occur with anonymous applications. On the other hand, the hiring committee was aware of the experiment, and this could have motivated them to be non-discriminatory in evaluating the non-anonymous applications.⁸¹ There was, however, no such pattern regarding non-Western applications, although that may be because discrimination in this case is not as much of a social taboo (perhaps because of the demographic composition of the research staff at the institute).⁸² Regardless, at this point this study could be viewed as providing additional confirmation of an absence of discrimination

⁸⁰ This kind of study echoes the call in Bendick and Nunes (2012) for more experimental research on strategies to reduce discrimination, which likely have to be undertaken by employers.

⁸¹ Behaghel et al. (2015), discussed below, refer to this as the “John Henry” effect.

⁸² Åslund and Nordström Skans (2012) present evidence on anonymized job application procedures from non-experimental data in Sweden, in which they find increased interviews and job offers for women, but only increased interviews for ethnic minorities.

against women from experimental methods.

However, a second application of this method, to hiring of minority job candidates in France, found that minorities fared worse – getting a smaller share of interviews – under anonymization (Behaghel et al., 2015).⁸³ (The hiring gap also widens, but not as much as the interview gap.) This conclusion contrasts sharply with the results from AC studies, although the authors suggest reasons why this experiment may give misleading results. In this study, the French public employment service offered firms the choice of participating, in which case they were randomized to receive standard or anonymized job applications. Among the participating firms, those that received anonymous applications interviewed fewer minority candidates. The authors suggest that this happens because, under anonymization, negative characteristics of job applicants, which are more likely to be associated with minority group membership but that firms might downweight for minorities, have a larger effect than when minority status is revealed.⁸⁴ They find evidence consistent with this interpretation, based on ratings of applicants that they had counselors from the public employment service do for non-anonymized and anonymized resumes.⁸⁵

Behaghel et al. note that the firms that agreed to participate were similar on most observables, except that they hired more minorities. When they joined the study, then, those that received anonymous applications may have been unable to continue preferential hiring of minorities. Under this interpretation, extending anonymization to all employers would have ambiguous effects, depending on to what extent the non-participating employers engage in discrimination against minorities, and to what extent anonymization prevents this. The considerations raised here suggest that while anonymization of resumes is potentially promising as a research strategy, it will need to avoid experimenter effects. At this point, the far greater body of research on discrimination using AC studies provides more convincing evidence.

⁸³ Minorities are defined as those living in deprived neighborhoods or having a foreign background.

⁸⁴ For example, in the U.S. context an employer might understand that blacks are far more likely to be incarcerated, and downweight a criminal record for a black applicant. But if the employer just sees the criminal record absent race information, it does not attenuate the information about the criminal record as much for blacks. The authors use the example of an unemployment spell, which is viewed as a stronger negative signal for non-minority applicants.

⁸⁵ An alternative explanation is the John Henry effect mentioned above, whereby the controls change their behavior to avoid appearing discriminatory, which the treatments cannot do because their job applications are anonymous. The authors suggest they can rule this out based on no difference in behavior among the controls during the experiment and after, when they are no longer being observed. Of course, it would be better to have data from before and during the experiment, because the experiment could have a residual effect.

9. Conclusions and Directions for Future Research

What have we learned from this vast body of experimental research on labor market discrimination?

We have learned that for most groups for which non-experimental data on wages is consistent with labor market discrimination, the experimental research provides confirming evidence of hiring discrimination. This is most evident with respect to race and ethnicity, perhaps because there is so much more evidence. It is also true for gays, criminals, and the disabled. In addition, the experimental research points to hiring discrimination against groups for which the prima facie (i.e., wage) evidence does not point to discrimination – in particular, lesbians, and older workers (at least women).

The audit and correspondence study evidence on sex discrimination provides an interesting exception. Despite a great deal of non-experimental evidence consistent with sex discrimination, the experimental research does not give a clear indication that women are treated less favorably in hiring decisions. Indeed, the evidence appears to point more to discrimination against women in some jobs and men in other jobs, in a manner that helps replicate the existing sex segregation of jobs. If this is in fact what happens, it poses a bit of a puzzle for labor economists, since our models of discrimination do not naturally predict this pattern. That said, perspectives on discrimination from other fields, such as those emphasizing norms regarding who does which job, might fit these facts better. And the experimental evidence does not necessarily imply that the unexplained wage gaps between men and women do not reflect discrimination.

Overall, though, this review reinforces the conclusion that hiring discrimination is pervasive. This may be in part because, even with aggressive anti-discrimination laws such as in the United States, it is hard to root out discrimination in hiring, both because of the problem of damages/financial incentives in these cases, and the difficulty of inferring hiring discrimination from an unknown pool of potential hires – a problem that can extend from establishing discrimination in court all the way down to the difficulty of a worker who was not hired inferring that this might be attributable to discrimination. On the other hand, if discrimination along the hiring dimension is particularly hard to root out, the evidence of hiring discrimination may not carry over to other labor market decisions (e.g., pay, promotions, and layoffs).

What are the important questions for future research?

There are open questions about precisely what we learn from the experimental evidence on some groups, which probably revolve mainly around the question of “is the differential treatment discrimination?” This arises most sharply, I think, with respect to research on those with a criminal background, and the disabled, who – for very different reasons – might be viewed as having different productivity. With regard to the disabled, further progress on this question likely requires incorporating more information on the relationship between specific disabilities and productivity.

Many papers using experimental methods to study labor market discrimination have tried to extend their results to uncover the nature of discrimination. This is a critical question, as it can inform how both policymakers and employers might respond to reduce discrimination. More work is needed to pin down compelling tests, and to replicate these across studies. In particular, I have explained why I find many studies trying to distinguish between statistical and taste discrimination less than convincing, because we do not know – to paraphrase Senator Howard Baker’s famous question at the Watergate hearings – “what does the employer know, and when does he know it?”⁸⁶

Moreover, the evidence on the nature of discrimination that does exist is all over the map, with no clear indication from the existing studies. There is some evidence that supports each of the three central explanations of discrimination (taste, statistical, and implicit), some evidence against both statistical and taste discrimination, and a number of places where conclusions drawn in support of one of these explanations may not be well founded. This is likely a reflection of two things. First, all three types of discrimination may matter, and perhaps differently for different groups. There may, for example, be more taste discrimination against groups whose “otherness” is most pronounced (say, Middle Eastern immigrants), more implicit discrimination against groups for which discrimination is more of a social taboo (for example, gays and lesbians), and more statistical discrimination against groups with well known but hard to predict differences (e.g., women of childbearing age). Second, distinguishing among the competing

⁸⁶ There is some quite compelling evidence of statistical discrimination in other markets (e.g., Castillo and Petrie, 2010; Laouenan, 2015; List, 2004), made easier in some cases because of the availability of information on learning about market participants (as in Laouenan’s study).

explanations is difficult. That said, the general approach of trying to “tack on” additional data collection to field experiments is likely to be the source of additional progress in learning about the nature of discrimination from these studies.

Researchers have frequently noted that audit and correspondence studies have somewhat limited scope – applying only to certain segments of the labor market, excluding some common channels of job search, etc. But there has been virtually no work trying to understand what these limitations might mean, or how we might expand the boundaries of experimental studies to reduce these limitations. Can we do more to figure out the role of the kinds of on-line job applications now used in field experiments in the overall job search of workers, perhaps in part by exploiting cross-country differences in reliance on government employment offices versus more private search? With changes in the technology of how people search for jobs, can we expand the types of jobs covered? Can social media be harnessed to improve these studies? (Alternatively, does the ability to use social media and other information technology to check credentials and identities undermine future field experiments?⁸⁷)

And what about the other side of the market? How important is the kind of hiring we study relative to the hiring employers do, and does the behavior these experiments study accurately capture a large extent of the hiring that at least these same employers – not to mention other employers – do? In general, researchers doing field experiments on labor market discrimination might consider how to assess the representativeness of the results from these experiments, and try to extend such experiments into a more diverse set of jobs and job search methods.

The focus on the application and hiring stage in field experiments on discrimination typically precludes gathering evidence on other types of discriminatory behavior – perhaps most importantly pay. There have been a few recent efforts to try to incorporate information on pay-related outcomes into field experiments, but nothing like proceeding all the way to final wage offers in an audit study. A recent

⁸⁷ If, for example, a correspondence study uses minority applicants with high credentials relative to the population, employers may be more likely to try to verify these credentials for minorities, and failing to do so may be less likely to call them back, leading to an overestimate of discrimination. In their study of race, ethnicity, and criminal background, Pager et al. (2009) set up voicemail boxes to see if employers checked references. Almost none did (four employers out of 350 that were tested). While this finding may not generalize, it suggests that concerns that AC studies might be invalidated because of employers checking on information on the resumes may be largely unfounded.

experimental study of race discrimination in consumer markets goes all the way through to transaction prices (Doleac and Stein, 2013). However, that study could consummate the actual transactions, so there presumably was not a problem of getting human subjects approval. It is an open question whether an institutional review board would currently approve even a standard audit study, let alone one that incorporated taking the interview process further to try to settle on pay.

Based on these considerations, if I were advising a highly ambitious graduate student who wanted to do a significant audit or correspondence study, I would discourage them from simply doing another study of differential treatment in hiring between two groups – even if they were groups that have not been studied much. Such a study would, to a large extent, provide another data point in a field where I think we know where most data points are going to lie – although such work can play a role in advocacy or even litigation on behalf of such groups. Rather, I think the important gains are going to come from embedding an AC study in a research project that is going shed additional light that helps us understand the findings, whether concerning the nature of discrimination, the implications of the experiment for the broader labor market, or perhaps how policy variation or other differences impact the discrimination these experiments are intended to measure.

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Table 1: Selected Results from Laboratory Experiments on Labor Market Discrimination

Study	Method	Summary/Results
<i>Sex discrimination</i>		
Rosen and Jerdee (1974)	Vignettes administered to bank managers in training program, regarding personnel decisions.	<i>Discrimination against women.</i> Higher selection of men than of women for promotion and for professional development. Higher likelihood of approving termination in response to request from male supervisor, when performance is the issue.
Olian et al. (1988)	Meta-analysis of vignette studies.	<i>Discrimination against women in selection.</i> Similar negative effect sizes for student and professional subjects.
Correll et al. (2007)	Vignettes administered to undergraduates for rating and selection, same-sex married pairs that differ by parenthood status.	<i>Discrimination against mothers among female, married women.</i> Large motherhood penalty, and smaller fatherhood “premium,” in rating, recommended salary, and recommendation for hiring. Hiring outcome mediated but not eliminated by controls for ratings. No sex discrimination among non-parents.
<i>Ethnicity discrimination</i>		
Blommaert et al. (2014)	Vignettes administered to students in university and higher vocational education institutions.	<i>Discrimination against minorities.</i> Weakly lower evaluations for Turkish or Moroccan ethnicity versus Dutch, and stronger evidence of less selection for interviews. Discrimination in selection for interviews weaker for respondents reporting positive interethnic contacts.
<i>Age discrimination</i>		
Rosen and Jerdee (1977)	Vignettes administered to subscribers of <i>Harvard Business Review</i> , ages 32/61.	<i>Discrimination against older workers.</i> Respondents more likely to recommend reassignment from older worker in response to complaints, and less likely to recommend selection for development or promotion. Stereotypes indicate older workers perceived as less likely to change behavior, less likely to want to keep up with technology, and less likely to succeed in position requiring creativity and innovation.
Rosen and Jerdee (1976)	Vignettes administered to undergraduate business students, ages 32/61.	<i>Discrimination against older workers.</i> Respondents more likely to recommend reassignment from older worker in response to complaints, and less likely to recommend selection for development or promotion. Stereotypes indicate older workers perceived as less likely to change behavior, less likely to want to keep up with technology, less likely to succeed in position requiring creativity and innovation, and less desirable for a financial position involving quick judgments and high risk.
Weiss and Maurer (2004)	Vignettes administered to undergraduate engineering students, replication of Rosen and Jerdee (1976), ages 32/61.	<i>Little evidence of age discrimination.</i> Few differences found between younger and older workers, with the only exception that older workers were deemed less likely to change behavior in response to complaints, prompting a higher likelihood of recommending the work be reassigned.
<i>Looks and obesity discrimination</i>		
Hosoda et al. (2003)	Meta-analysis of vignette studies.	<i>Discrimination against less attractive.</i> Large positive advantage for the attractive. Does not vary much with the sex of the applicant, the sex type of the job, the provision of job-relevant information, or whether subjects were students or professionals. Effect declines over time.
Rudolph et al. (2009)	Meta-analysis of vignette studies.	<i>Discrimination against obese.</i> Adverse effects of obesity for performance evaluation, selection for promotion, and hiring (for which the effects are largest).
<i>Statistical vs. taste discrimination</i>		
Heilman (1984)	Vignettes administered to MBA students for selection for managerial position. Sex manipulated, as is provision of information on college coursework.	Lower selection of women (and ratings of potential and likelihood of success) when less job-relevant information is provided. Difference largely eliminated when job-relevant information provided (coursework in economics and business) that may undermine sex stereotypes.
Tosi and Einbender (1985)	Meta-analysis of vignette studies of selection by sex.	Across studies, sex differences in outcomes are lower when subjects are given more information about applicants.
Davison and Burke (2000)	Meta-analysis of vignette studies of selection by of men and women into “opposite-sex” jobs.	For both men and women, lower ratings for opposite-sex jobs are diminished when more information is provided. Effect may have more to do with reduced salience of sex, rather than statistical discrimination.

Study	Method	Summary/Results
Baert and DePauw (2014)	Vignettes administered to students regarding ethnic discrimination, with additional survey questions.	Subjects were more likely to indicate that customers or co-workers would enjoy interacting with natives than minorities, although they indicated that as employers, there is no difference in selection for hiring. Subjects were likely to rate the minority applicants as having the required productivity for the job, but to rate the minority group overall as less qualified. Interpreted as evidence of customer and co-worker discrimination, but not employer statistical discrimination; validity of interpretation can be questioned.
Mobius and Rosenblat (2006)	Lab experiment in which workers solve mazes (ability unrelated to beauty), and employers interact with workers in different ways (some visual and some not) to estimate productivity and pay workers.	Employers overestimate ability of better-looking workers by employers, forming incorrect stereotypes. No role for taste discrimination.
<i>Bargaining</i>		
Dittrich et al. (2014)	Male and female subjects alternate roles as employer or employee, with same- and mixed-gender pairings.	<i>Evidence of wage discrimination in bargaining framed as employer-employee negotiations.</i> Males acting as employees negotiate higher wages than females, mainly because of higher initial wage offers of males as employees, and higher initial wage offers of male employers to male employees. The differences emerge because of differences in initial offers rather than subsequent bargaining, suggesting that sex differences in bargaining skills are not critical.

Table 2: Results from Natural Experiments on Labor Market Discrimination

Question/issue	Study	Method	Summary/Results
<i>Sports</i>			
Do umpires favor pitchers of the same race/ethnicity in making ball and strike calls in Major League Baseball?	Parsons et al. (2011)	Estimates whether in same-race/ethnicity pairings of umpires and pitchers, strikes are more likely to be called, and measures difference when there is more explicit or implicit monitoring.	<i>Discrimination when players evaluated by umpires of different race/ethnicity.</i> Strikes are more likely to be called when the umpire and pitcher are of the same race/ethnicity. Bias affects pitchers' behavior so that performance of other-race pitchers is worse. The bias in umpiring diminishes considerably under more intensive monitoring.
Are more fouls called on players of the opposite race from referees in the National Basketball Association?	Price and Wolfers (2010)	Estimate relationship between racial composition of referee crew and race of player in calling fouls.	<i>Discrimination when players evaluated by referees of opposite race.</i> Fouls are more likely to be called on players whose race deviates more from that of the umpiring crew. The bias mainly affects white players, either via worse treatment by black referees or favorable treatment by white referees. Bias appears to affect players' behavior adversely.
Do referees award yellow cards after fouls in English Premier League soccer games, based on differences between referees and players ("oppositional identity," based on race, and foreign origin from poorer countries)?	Gallo et al. (2013)	Estimate whether a yellow card is awarded on a foul, and how this differs with player's oppositional identity to referee.	<i>Evidence interpreted as pointing to implicit discrimination against players with oppositional identities to referees.</i> Yellow cards awarded more commonly to players of oppositional identity, and this is more common on parts of the field when the decision is more rushed, or the call is more ambiguous.
<i>Orchestras</i>			
Did the switch to blind auditions for major orchestras increase the selection of females?	Goldin and Rouse (2000)	Regression models for selection of females, using variation in the adoption by orchestras of blind auditions where the musician plays behind a screen and other steps are taken to ensure that the musician's identity is not known when selection decisions.	<i>Discrimination against women in selection.</i> In general, the selection of females increased as a result of blind auditions, and this change in audition procedures accounted for about one quarter of the increase in the percentage of females in the period studied.

Table 3: Selected Results from Field Experiments on Labor Market Discrimination

Study	Method	Summary/Results
<i>Race and ethnicity</i>		
Riach and Rich (2002)	Survey of existing studies through 2000	<i>Consistent evidence of hiring discrimination against racial and ethnic minorities in many countries.</i> Every comparison points to discrimination against the minority group, most statistically significant.
Rich (2014)	Survey of existing studies from 2000 through around 2012	<i>Consistent evidence of hiring discrimination against racial and ethnic minorities in many countries.</i> Across many studies, near-uniform evidence of hiring discrimination against racial and ethnic minorities, with few exceptions. Many studies for European countries document evidence of discrimination against applicants of Middle Eastern origin (often immigrants).
Carlsson and Rooth (2012)	Combine data from correspondence study in Sweden with survey information on attitudes towards immigrants, which vary at the municipal level.	Call-back rate for Middle Eastern minorities was lower in municipalities where respondents had more discriminatory attitudes, interpreted as consistent with a role for taste discrimination. However, the attitudinal question could detect population differences and hence the results could reflect statistical discrimination.
Zschirnt and Ruedin (2015)	Meta-analysis of studies of race and ethnicity from OECD countries, for 1990-2015	<i>Consistent evidence of hiring discrimination against racial and ethnic minorities in many countries. Mixed evidence regarding taste versus statistical discrimination, and interpretation not always clear.</i> Near-uniformity of findings of hiring discrimination against ethnic minorities. Discrimination somewhat lower for second-generation than for first-generation immigrants, but not significantly. Discrimination highest for Arabs/Middle Eastern origin, followed by Indians, Pakistani, Bangladeshi, and Chinese, and then Turks, interpreted as taste discrimination associated with the most distant groups. Discrimination lower in German-speaking countries, where job applications are much more detailed, which the authors interpret as consistent in part with statistical discrimination.
Oreopoulos (2011)	Correspondence study of immigrants from many countries in Canada, across multiple jobs.	<i>Evidence of hiring discrimination against immigrants. Evidence often not consistent with statistical discrimination.</i>
Rooth (2010)	Incorporation of responses to IAT and explicit discrimination measures into data from correspondence studies of discrimination against Arab-Muslim men in Sweden.	<i>Implicit discrimination may account for ethnic discrimination, but better evidence needed.</i> Recruiters with higher IAT (more discriminatory) less likely to call back Arabs. Responses to explicit measures do not explain relationship, nor do explicit measures significantly predict call-backs. Evidence interpreted as showing implicit discrimination, but because explicit measures do not condition on applicant characteristics, they may not reflect discriminatory attitudes correctly.
Bartoš et al. (2014)	Test for differences in “attention” in employer evaluation of job applicants by measuring parts of resumes examined by employers or efforts towards getting resume.	<i>“Attention discrimination” may disadvantage minority applicants, causing employers to spend less time evaluating them, and reinforcing emphasis on group averages of disadvantaged groups.</i> In the Czech study, employers call-back minorities at lower rates, and pay less attention to the minority resumes, in terms of both opening resumes and looking at more information – although these differences are generally not significant. In the German study employers exert less effort to obtain minority resumes.
Neumark (2012); Carlsson et al. (2013); Baert et al. (2015)	Application of heteroskedastic probit method from Neumark (2012) to existing data or new data.	<i>Allowing for different variances of unobservables tends to increase estimated discrimination against racial/ethnic minorities. No indication that existing studies badly overstate discrimination.</i> In two of the three studies, evidence of larger variance of unobservable for minority group, and larger estimate of discrimination, consistent with a low level of standardization of resumes. Estimates of the relative variances are imprecise, but allowing the difference does not reduce precision of estimate of discrimination.
<i>Sex</i>		
Riach and Rich (2002)	Survey of existing studies through 2000.	<i>Less clear evidence of general discrimination against women than of hiring discrimination consistent with sex stereotyping of jobs.</i> Across three U.S. studies, and one for Austria and Australia, evidence points to discrimination against women in male and/or high-status/high-pay jobs, and against men in female jobs. No clear evidence of sex-integrated jobs.
Rich (2014)	Survey of existing studies from 2000 through around 2012.	<i>Absence of evidence of general discrimination against women in hiring, and more evidence consistent with sex stereotyping of jobs.</i> In studies for China, England, and France, absence of consistent evidence of discrimination against women, and more consistent evidence of discrimination against women in male-dominated

Study	Method	Summary/Results
		jobs and against men in female-dominated jobs (possibly stronger).
<i>Age</i>		
Bendick et al. (1997)	Correspondence study for management information systems (men), executive secretaries (women), and writer/editor jobs, 57 vs. 32.	<i>Strong evidence of discrimination against older workers, and some evidence of statistical discrimination.</i> Evidence of age discrimination for men and women. Weaker evidence for applicants de-emphasizing age and emphasizing youthful qualities.
Bendick et al. (1999)	Audit study for entry-level sales or management jobs, almost all applicants male, 57 vs. 42.	<i>Strong evidence of discrimination against older male workers.</i> Evidence of age discrimination, mainly at pre-interview stage, and for job characteristics as well when offers made to both testers.
Lahey (2008)	Correspondence study, general entry-level jobs, 35/45/50/55/62.	<i>Strong evidence of discrimination against older female workers.</i> Evidence of age discrimination, increasing in age. No clear evidence from tests intended to detect statistical discrimination, or employer, employee, or customer discrimination.
Riach and Rich (2006, 2010); Riach (2015)	Correspondence studies for four European countries, focused mainly on jobs as waiters, but also female applicants in retail (in England), 47 vs. 27.	<i>Strong evidence of discrimination against older males, but not older females.</i> Clear evidence of age discrimination against older men for waiter jobs in all countries. Evidence of favoritism toward older female applicants in retail (one country only).
Neumark et al. (2015)	Large-scale U.S. correspondence study, focused on sales (male and female), administrative assistants (female), and janitors and security (male), 29-31/49-51/64-66.	<i>Robust evidence of age discrimination against older women. Less clear evidence for men.</i> Evidence for men not robust to using low-experience vs. commensurate-experience resumes for older men, or correcting for difference in the variance of unobservables.
Farber et al. (2015)	U.S. correspondence study of administrative support job applications for women, 35-37/40-42/55-58.	<i>Evidence of age discrimination against older women.</i> Significantly lower call-back rates for oldest group of applicants.
<i>Obesity and looks</i>		
Rooth (2009)	Correspondence study for Sweden, multiple occupations, photos digitally manipulated to make non-obese applicant look obese.	<i>Evidence of discrimination against obese men and women.</i> For both men and women, and most occupations, obesity lowers the call-back rate. An attractiveness rating explains the obesity differential for men but not for women, although it is hard to distinguish the two effects. Some indication that obesity discrimination is larger in jobs requiring customer contact (such as sales).
Agerström and Rooth (2011)	Incorporation of responses to IAT and explicit discrimination measures into data from correspondence study of discrimination against obese applicants in Sweden.	<i>Implicit discrimination may account for obesity discrimination, but better evidence needed.</i> Lower call-backs to obese applicants from recruiters with higher IAT (more discriminatory). Responses to explicit measures do not explain this relationship, nor do explicit measures significantly predict call-backs. Evidence interpreted as showing implicit discrimination, but explicit measures do not condition on applicant characteristics.
Bóo et al. (2013)	Correspondence study in Argentina, with digitally manipulated photos, across multiple occupations.	<i>Ambiguous evidence of discrimination based on looks.</i> Call-back rates for attractive candidates one third higher, similar for men versus women. But no difference when photos are subjectively ranked.
Ruffle and Shtudiner (2015)	Correspondence study in Israel, based on subjectively rated photos, across multiple jobs.	<i>Discrimination based on looks for men, more so in jobs requiring experience. No evidence that customer contact matters.</i>
Galarza and Yamada (2014)	Correspondence study in Peru, based on subjectively rated photos, across multiple occupations.	<i>Evidence of discrimination based on looks and race, but lookism appears to explain racial difference.</i> Among unskilled jobs, looks matter only in jobs with customer contact.
<i>Women with family responsibilities</i>		
Duguet and Petit (2005); Petit (2007)	Correspondence study for financial sector in France, men and women of different ages and family status associated with likely future children.	<i>Evidence of discrimination against young women based on expected future childbearing, in jobs where turnover may matter more.</i> No significant differences in call-back rates by sex. For higher-skill jobs with higher turnover costs there is evidence of lower call-backs for women aged 25 and single.
Correll et al. (2007)	U.S. correspondence study designed to match lab	<i>Evidence of discrimination against women with childrearing responsibilities.</i> No significant difference in call-back rates for men dependent on parental status, but a

Study	Method	Summary/Results
	experiment in same paper.	call-back rate for childless women 1.8 times that for mothers.
<i>Criminal background</i>		
Pager (2003)	Audit study in Milwaukee, black and white tester pairs, broad array of low-skill jobs.	<i>Clear evidence of differential treatment, which may not reflect discrimination.</i> For whites, call-back rates were lower by 50 percent for those with criminal backgrounds; among blacks call-back rates were lower by 71 percent.
Baert and Verhofstadt (2015)	Correspondence study in Belgium, across multiple occupations.	<i>Clear evidence of differential treatment for young men with juvenile delinquent past, which may not reflect discrimination.</i> The call-back rate for male applicants with a criminal background was 42 percent lower.

Sexual orientation

Weichselbaumer (2003)	Correspondence study in Austria, secretary and accountant jobs.	<i>Evidence of hiring discrimination against lesbians.</i> Lesbians receive substantially fewer call-backs, with no difference depending on masculinity or femininity. Unlikely to reflect statistical discrimination.
Drydakis (2009)	Correspondence study in Greece, office, industrial, restaurant, and retail jobs.	<i>Evidence of hiring discrimination against gays.</i> Substantially lower call-back rates for gays, although no difference in wages of jobs for which both gays and straights invited to interview.
Drydakis (2011)	Correspondence study in Greece, office, industrial, restaurant, and retail jobs.	<i>Evidence of hiring discrimination and consistent with wage discrimination against lesbians.</i>
Tilcsik (2011)	Correspondence study in United States, across many jobs sought by new college graduates.	<i>Evidence of hiring discrimination against gays, perhaps mediated by anti-discrimination laws.</i> Significantly lower call-backs for gay applicants, but only in some states. Multivariate analysis suggests anti-discrimination laws reduce call-back differentials, and that gays were less likely to receive call-backs from employers with ads referencing stereotypically male traits.
Drydakis (2014)	Correspondence study in Cyprus, with variation in information provided, office, industrial, restaurant, and retail jobs.	<i>Evidence of hiring discrimination and consistent with wage discrimination against gays and lesbians.</i> Lower call-back rates for gays and lesbians, and lower potential wage offers. No evidence that more information affects measured discrimination.

Disability

Ravaud et al. (1992)	Correspondence study in France, broad array of employers, paraplegics.	<i>Evidence consistent with hiring discrimination against disabled.</i> Lower call-back rates for the disabled, more so among less-qualified applicants.
Baert (2014b)	Correspondence study in Belgium, for blind, deaf, or autistic applicants, also varying based on eligibility of disabled for wage subsidy.	<i>Evidence consistent with hiring discrimination against disabled.</i> Disabled candidates about half as likely to receive a positive call-back. Results reported as not sensitive to correction for differences in the variances of unobservables.
Ameri et al. (2015)	Correspondence study in the United States. Applicants with spinal cord injuries or Asperger's syndrome to accounting jobs.	<i>Evidence consistent with hiring discrimination against disabled, and with an effect of anti-discrimination provisions in reducing this discrimination.</i> Significantly lower call-backs for disabled applicants. Gap larger for more-skilled applicants, and small and not statistically significant for the less-experienced applicants. A variety of analyses suggest that discrimination is concentrated among employers below the ADA cutoff of 15 employees.

Evidence from anonymized application experiments

Krause et al. (2012)	Anonymize applications for selection for interview at European research institute.	<i>Possible evidence of discrimination in favor of women, or anonymization does not increase interviewing of women.</i> Female applicants were more likely to be invited for interviews in the non-anonymous sample, whereas this advantage was erased in the anonymous sample, although this could be due to hiring committee's knowledge of experiment.
Behaghel et al. (2014)	Anonymize applications for selection for interview in France.	<i>Possible evidence of discrimination in favor of minorities, or anonymization does not increase interviewing of minorities.</i> Minority applicants were less likely to be interviewed and hired when applications were anonymized. Possibly attributable to inability to downweight negative signals for minorities, or to engage in preferential hiring of minorities.