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DETECTING DISCRIMINATION IN AUDIT AND CORRESPONDENCE STUDIES

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ABSTRACT

Audit studies testing for discrimination have been criticized because applicants from different groups may not appear identical to employers. Correspondence studies address this criticism by using fictitious paper applicants whose qualifications can be made identical across groups. However, Heckman and Siegelman (1993) show that group differences in the variance of unobservable determinants of productivity can still generate spurious evidence of discrimination in either direction. This paper shows how to recover an unbiased estimate of discrimination when the correspondence study includes variation in applicant characteristics that affect hiring. The method is applied to actual data and assessed using Monte Carlo methods.

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I. Introduction

In audit or correspondence studies, fictitious individuals who are identical except for race, sex, or ethnicity apply for jobs. Group differences in outcomes – for example, blacks getting fewer job offers than whites – are interpreted as reflecting discrimination. Across a wide array of countries and demographic groups, audit or correspondence studies find evidence consistent with discrimination, including discrimination against blacks, Hispanics, and women in the United States (Mincy 1993; Neumark 1996; Bertrand and Mullainathan [BM] 2004), Moroccans in Belgium and the Netherlands (Smeeters and Nayer 1998; Bovenkerk, Gras, and Ramsoedh 1995), and lower castes in India (Banerjee et al. 2008). These "field experiments" are widely viewed as providing the most convincing evidence on discrimination (Pager 2007; Riach and Rich 2002), and U.S. courts allow organizations that conduct audit or correspondence studies to file claims of discrimination based on the evidence they collect (U.S. Equal Employment Opportunity Commission 1996). Yet audit or correspondence studies have been sharply criticized by Heckman and Siegelman [HS] (HS 1993; Heckman 1998). Perhaps the most damaging criticism is that when the variance of unobserved productivity differs across groups – as in the standard statistical discrimination model (Aigner and Cain 1977) – audit or correspondence studies can generate spurious evidence of discrimination in either direction or of its absence; equivalently, discrimination is unidentified in these studies. Although this critique has been ignored in the literature, it clearly casts serious doubt on the validity of the evidence from these studies. This paper addresses the unobserved variance critique of audit and correspondence studies, proposing a method of collecting and analyzing data from these studies that can correctly identify discrimination.

The HS criticism that has received the most attention is that audit studies – which use "live" job applicants – fail to ensure that applicants from different groups appear identical to employers. Many of these criticisms can be countered by using correspondence studies, which use fictitious applicants on paper, or more recently the internet, whose qualifications can be made identical across groups. However, the Heckman and Siegelman (1993) critique applies even in the ideal case in which both observed and unobserved group averages are identical.

This paper develops and implements a method of using data from audit or correspondence studies that accomplishes two goals. First, it provides a statistical test of whether HS's unobservable variance critique applies to the data from a particular study. Second, and more important, it develops a statistical estimation procedure that identifies the effect of discrimination. The method requires the study to have variation in applicant characteristics that affect hiring. It is a simple matter to collect the requisite data in future correspondence studies, and as an illustration the method is implemented using data from a correspondence study (BM 2004) that has the requisite data.

The method rests on three types of assumptions. First, it is based on an assumed binary threshold model of hiring that asks whether the perceived productivity of a worker exceeds a standard. Second, it imposes a parametric assumption about the distribution of unobservables that is necessary for identification in this case. The model and parametric assumption parallel exactly the setting used by HS to interpret data from these types of studies; but there may of course be other contexts where this method is useful. Finally, to solve the identification problem highlighted by HS, it relies on an additional identifying assumption that some applicant characteristics affect the perceived productivity of workers, and hence hiring, and that the effects of these characteristics on perceived productivity do not vary with group membership (for example, race). This identifying assumption has testable implications in the form of overidentifying restrictions. The estimation procedure is assessed via Monte Carlo simulations.

II. Background on Audit and Correspondence Studies

Earlier research on labor market discrimination focused on individual-level employment or earnings regressions, with discrimination estimated from the race, sex, or ethnic differential that remains unexplained after including many proxies for productivity. These analyses suffer from the obvious criticism that the proxies do not adequately capture group differences in productivity, in which case the "unexplained" differences cannot be interpreted as discrimination.

Audit or correspondence studies are a response to this inherent weakness of the regression approach to discrimination. These studies are based on comparisons of outcomes (usually job interviews or job offers) for matched job applicants differing by race, sex, or ethnicity (see, for example, Turner, Fix, and Struyk 1991; Neumark 1996; BM 2004). Audit or correspondence studies directly address the problem of missing data on productivity. Rather than trying to control for variables that might be associated with productivity differences between groups, these studies instead create an artificial pool of job applicants, among whom there are intended to be no average differences by group. By using either applicants coached to act alike, with identical-quality resumes (an audit study), or simply applicants on paper who have equal qualifications (a correspondence study), the method is largely immune to criticisms of failure to control for important differences between, for example, black and white job applicants. As a consequence, this strategy has come to be widely used in testing for discrimination in labor markets (as well as housing markets). Thorough reviews are contained in Fix and Struyk (1993), Riach and Rich (2002), and Pager (2007).

Despite the widely-held view that audit or correspondence studies are the best way to test for labor market discrimination, critiques of these studies challenge their conclusions (HS 1993; Heckman 1998). Many of these criticisms have been acknowledged by researchers as potentially valid, and subsequent research has adapted. For example, HS noted that in the prominent audit studies carried out by the Urban Institute (for example, Mincy 1993), white and minority testers were told, during their training, about "the pervasive problem of discrimination in the United States," raising the possibility that testers subconsciously took actions in their job interviews that led to the "expected" result (HS 1993). A constructive response to this criticism has been the move to correspondence studies, which focus on applications on paper and whether they result in job interviews, thus cutting out the influence of the individual job applicants used in the test.

However, a fundamental critique of audit or correspondence studies has not been addressed by researchers. In particular, HS consider what most researchers view as the ideal conditions for an audit or correspondence study – when not only are the observable average differences between groups eliminated, but in addition the observable characteristics used in the applications are sufficiently rich that it is reasonable to assume that potential employers believe there are no average differences in *unobservable* characteristics across groups. HS show that, even in this case, audit or correspondence studies can generate evidence of discrimination (in either direction) when there is none, and can also mask evidence of discrimination when it in fact exists. Given the pervasive use of audit and correspondence studies, the failure of any research to address this critique is a significant gap in the social science and legal literature.

III. The Heckman-Siegelman Critique

I first set up the analytical framework that parallels the one used in HS's critique of evidence from audit and correspondence studies. This framework is used to illustrate the problem of identifying discrimination when there are group differences in the variance of unobservables. Then, in the following section, I show how discrimination can be identified in this setting.

Suppose that productivity depends on two individual characteristics, $X' = (X^I, X^{II})$. Let R be a dummy for race, with R = 1 for minorities and 0 for non-minorities (which I will refer to as "black" and "white" for short). Allow productivity also to depend on a firm-level characteristic F, so that productivity is P(X',F). Let the treatment of a worker depending on P and possibly R (if there is discrimination) be denoted T(P(X',F),R). For now, think of this treatment as continuous, even though that is not the usual outcome for an audit or correspondence study; suppose the treatment is, for example, the wage offered, set equal to a worker's productivity minus a possible discriminatory penalty for blacks as in Becker's (1971) employer taste discrimination model.

Define discrimination as

(1) $$T(P(X',F)|R = 1) \neq T(P(X',F)|R = 0).$$

Assume that P(.,.) and T(P(.,.)) are additive, so

(2) $$P(X',F) = \beta_I'X^I + X^{II} + F$$

(3) $$T(P(X',F),R) = P + \gamma'R.$$ [1]

Thus, discrimination against blacks implies that $\gamma' < 0$, so that blacks are paid less than equally-productive whites at the same firm.

In an audit or correspondence study, two testers (or applications) or multiple pairs of testers (one with R = 1 and one with R = 0 in each pair) are sent to firms to apply for jobs. The researcher attempts to standardize their productivity based on observable productivity-related characteristics. Denote expected productivity for blacks and whites, based on the productivity-related characteristics that the firm observes, as P_B^* and P_W^*; these are not necessarily based on both X^I and X^{II}, as we may want to treat X^{II} as unobserved by firms. The goal of the audit or correspondence study design is to set $P_B^* = P_W^*$. Given these observables, the outcome T is observed for each tester. So based on Equation 3, each test – thought of as the outcome of applications to a firm by one black and one white tester – yields an observation

(4) $T(P_B^*,1) - T(P_W^*,0) = P_B^* + \gamma' - P_W^*$.

If $P_B^* = P_W^*$, then averaging across tests yields an estimate of γ'. In this case, we can also estimate the mean difference between the outcome T for blacks and white by a regression of T on a constant and the race dummy, or

(5) $T(R) = \alpha' + \gamma' R_i + \varepsilon_i$

where the first argument of T(.,.) is suppressed because it is assumed the same for all applicants.[2]

Now consider explicitly the two components of productivity, X^I and X^{II}. Suppose the audit or correspondence study controls only X^I in the resumes or interviews.[3] Denote by X_B^j and X_W^j the values of X^I and X^{II} for blacks and whites, j = I, II. Suppose that the audit or correspondence study, as is usually done, sets $X_B^I = X_W^I$; the level at which they are set is later denoted as X^{I*}, the level at which X^I is "standardized" across applicants. Then for the test resulting from the application of a pair of black and white testers to a firm, P_B^* and P_W^* are

(6) $P_B^* = \beta_I' X_B^I + E(X_B^{II}) + F$

(7) $P_W^* = \beta_I' X_W^I + E(X_W^{II}) + F$.

(For simplicity, I suppress the conditioning on X_B^I or X_W^I. Both P_k^* and $E(X_k^{II})$, k = B, W, can be interpreted as conditional on X_k^I.)

In this case, each individual test provides an observation equal to

(8) $T(P_B^*,1) - T(P_W^*,0) = P_B^* + \gamma' - P_W^* = \beta_I' X_B^I + E(X_B^{II}) + \gamma' - (\beta_I' X_W^I + E(X_W^{II}))$

$$= \gamma' + E(X_B^{II}) - E(X_W^{II}).$$

Clearly observations from a sample of such tests identify γ' only if $E(X_B^{II}) = E(X_W^{II})$. Thus, a key assumption in an audit or correspondence study is that all productivity-related factors not controlled for in the test have the same mean for blacks and whites. Heckman (1998) and HS (1993), in critiques unrelated to the focus of this paper (group differences in the variance of unobservables), offer a detailed discussion of the reasons why this assumption might be violated in audit studies. Specifically, despite researchers' best efforts to standardize applicants, differences remain that may be observed by employers. And HS further show that even when these differences are weakly related to productivity, they can lead to large biases, precisely *because* the applicants are standardized on other productivity-related characteristics. Moreover, the design of an audit study allows for experimenter effects that can – through information conveyed to employers in the job interview – generate differences between $E(X_B^{II})$ and $E(X_W^{II})$.

Correspondence studies are a response to these criticisms of audit studies (see, for example, BM, p. 994). In contrast to audit studies, they do not entail face-to-face interviews that might convey mean differences on uncontrolled variables between blacks and whites, and hence the researcher can eliminate differences observed by employers that are not controlled in the study. In addition, a correspondence study, by its very nature, avoids potential experimenter effects.

Even in a correspondence study, though, differences in employer estimates of mean unobserved characteristics for blacks and whites can affect the results, as in Equation 8. The difference, in this case, is one of interpretation, and even when this possibility arises, correspondence studies still have an important advantage. Regarding interpretation, because employers are not allowed to make assumptions about race or sex differences in characteristics not observed in the job application or interview process (U.S. Equal Employment Opportunity Commission, n.d.), any role of assumed mean differences in characteristics in affecting the outcomes from a correspondence study can be interpreted as statistical discrimination. Consequently, we can interpret the estimate of the expression in Equation 8 from a correspondence study as capturing the combined effects of taste discrimination (γ') and statistical discrimination ($E(X_B^{II}) - E(X_W^{II})$). But in a correspondence study, the estimate of the combined effects of the two types of discrimination is still more reliable than in an audit study, because of the absence of experimenter effects.

In other words, correspondence studies succeed, where audit studies may fail, in providing unbiased estimates of what the law recognizes as discrimination. However, they are not necessarily better at isolating taste discrimination. Nonetheless, 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.[4]

Turning to the main focus of this paper, HS show that a more troubling result emerges in audit or correspondence studies because the relevant treatment is not linear in productivity as it might be for a wage offer, but instead is non-linear. That is, we think that in the hiring process firms evaluate a job applicant's productivity relative to a standard, and offer the applicant a job (or an interview) if the standard is met. In this case, HS show that, even when there are equal group averages of both observed and unobserved variables, an audit or correspondence study can generate biased estimates, with spurious evidence of discrimination in either direction, or of its absence – or, in other words, discrimination is unidentified. Because this critique applies even to correspondence studies, which meet higher standards of validity, the remainder of the discussion refers exclusively to correspondence studies.

The intuitive basis of the HS critique is as follows. Consider the simplest case in which $E(X_B^j) = E(X_W^j)$, j = I, II, and the only difference between blacks and whites is that the *variance* of unobserved productivity is higher for whites than for blacks, for example. The correspondence study controls for one productivity-related characteristic, X^I, and standardizes on a quite low value of X^I (that is, the study makes the two groups equal on characteristic X^I, but at a low value X^{I*}). The correspondence study does not convey any information on a second, unobservable productivity-related characteristic, X^{II}. Because an employer will offer a job interview only if it perceives or expects the sum $\beta_I'X^I + X^{II}$ to be sufficiently high, when X^{I*} is set at a low level the employer has to believe that X^{II} is high (or likely to be high) in order to offer an interview. Even though the employer does not observe X^{II}, if the employer knows that the variance of X^{II} is higher for whites, the employer correctly concludes that whites are more likely than blacks to have a sufficiently high sum of $\beta_I'X^I + X^{II}$, by virtue of the simple fact that fewer blacks have very high values of X^{II}. Employers will therefore be less likely to offer jobs to blacks than to whites, *even though the observed average of X^I is the same for blacks and whites, as is the unobserved average of X^{II}*. The opposite holds if the standardization is at a high value of X^I; in the latter case the employer only needs to *avoid* very low values of X^{II}, which will be more common for the higher-variance whites.

The idea that the variances of unobservables differ across groups has a long tradition in research on discrimination, stemming from early models of statistical discrimination. For example, Aigner and Cain (1977) discuss these models and suggest that a higher variance of unobservables for blacks compared to whites is plausible, and Lundberg and Startz (1983) study how such an assumption can lead to an equilibrium with lower investment in human capital by blacks. On the other hand, Neumark (1999) finds no evidence that employers have better labor market information about whites than blacks, and if anything the estimates – although imprecise – indicate the opposite.

To see the bias formally, suppose that a job offer or interview is given if a worker's perceived productivity exceeds a certain threshold c'. As before, suppose that P is determined as a linear sum of X^I, X^{II}, and F (Equation 2), with X^{II} (and F) statistically independent of X^I.[5] The hiring rules for blacks and whites (with the possibility of discrimination) are

(9) $T(P(X^{I*},X_B^{II})|R = 1) = 1 \text{ if } \beta_I'X^{I*} + X_B^{II} + \gamma' + F > c'$

(9') $T(P(X^{I*},X_W^{II})|R = 0) = 1 \text{ if } \beta_I'X^{I*} + X_W^{II} + F > c'$.

Discrimination may lead employers to "discount" the productivity of a black worker, as captured in γ'.

To get to an econometric specification, assume that the unobservables X_B^{II} and X_W^{II} are normally distributed, with equal means (set to zero, without loss of generality), and standard deviations σ_B^{II} and σ_W^{II}. Finally, as long as the firm-specific productivity shifters F are normally distributed and independent of X^{II}, and because they have the same distribution for blacks and whites, we can ignore them and focus solely on the unobserved variation in X^{II} (effectively redefining the random variable in what follows as X^{II} + F). Under these assumptions, the probabilities that blacks and whites get hired are

(10) $\Pr[T(P(X^{I*},X_B^{II})|R = 1) = 1] = 1 - \Phi[(c' - \beta_I'X^{I*} - \gamma')/\sigma_B^{II}] = \Phi[(\beta_I'X^{I*} + \gamma' - c')/\sigma_B^{II}]$

(10') $\Pr[T(P(X^{I*},X_W^{II}) |R = 0) = 1] = 1 - \Phi[(c' - \beta_I'X^{I*})/\sigma_W^{II}] = \Phi[(\beta_I'X^{I*} - c')/\sigma_W^{II}]$,

where Φ denotes the standard normal distribution function.

There is a potential difference between audit and correspondence studies in terms of how we might think about what is observable to the econometrician and the firm. In an audit study, the simplest characterization might by that both the firm and the econometrician observe X_B^I and X_W^I, but the firm also observes X_B^{II} and X_W^{II}. In a correspondence study, however, both the firm and the econometrician observe only X_B^I and X_W^I, with no variation in X_B^{II} and X_W^{II} observed by employers. However, the distinction between audit and correspondence studies is not quite this sharp, because even though an employer interviews an applicant in an audit study, some (perhaps many) determinants of productivity remain unobserved when a job offer is made.

This issue is relevant to thinking about how we arrive at a statistical model of hiring, such as the probit specifications in Equations 10 and 10'. In an audit study, variables unobserved by the econometrician but observed by the firm can generate variation in hiring. In a correspondence study, however, if firms observe only X_B^I and X_W^I, then the decision about who to hire should be deterministic. Given X_B^I and X_W^I (assumed equal), the employer hires the higher variance group if the level of standardization is low, and vice versa, so all firms evaluating identical job applicants should make the same decisions about hiring whites and blacks. One way to introduce unobservables that generate random variation, with variances that differ by race as above, is to assume that there are random productivity differences across firms that are multiplicative in the unobserved productivity of a worker. In that case, the differences in the variances of X_B^{II} and X_W^{II} map directly into unobservables that vary across firms with relative variances proportional to the relative variances of X_B^{II} and X_W^{II}. Alternatively, employers may make expectational errors and rather than assigning a zero expectation to the unobservable assign a random draw based on the distribution of unobservables.

Returning to the main line of argument, the difference between Equations 10 and 10' – the success rates for black and white job applicants – is intended to be informative about discrimination. However, even if $\gamma' = 0$, so there is no discrimination, these two expressions need not be equal because σ_B^{II} and σ_W^{II}, the standard deviations of X_B^{II} and X_W^{II}, can be unequal. The earlier intuition about relative variance of the unobservables and the level of standardization of X^I can be made more precise. Consider the earlier case with $\gamma' = 0$, but $\sigma_W^{II} > \sigma_B^{II}$. Then if X^{I*} is set at a low level – that is, the standardization level is low – characterized by $\beta_I'X^{I*} < c'$, $\sigma_W^{II} > \sigma_B^{II}$ implies that $\Phi[(\beta_I'X^{I*} + \gamma' - c')/\sigma_B^{II}] < \Phi[(\beta_I'X^{I*} - c')/\sigma_W^{II}]$, generating spurious evidence of discrimination against blacks. When $\beta_I'X^{I*} > c'$, we instead get spurious evidence of discrimination in favor of blacks, and switching the relative magnitudes of σ_W^{II} and σ_B^{II} reverses these results.[6]

Thus, even if the means of the unobserved productivity-related variables are the same for each group, and firms use the same hiring standard (that is, $\gamma' = 0$), correspondence studies can generate evidence consistent with discrimination against blacks (or, alternatively, in their favor). Although demonstrated in the case of normally distributed unobservables, the argument holds for symmetric distributions (Heckman 1998). This is the basis for HS's claim that even under ideal conditions correspondence (or audit) studies are uninformative about discrimination.

IV. Detecting Discrimination

With the right data from a correspondence study, the framework from the preceding section can be used to recover an unbiased estimate of discrimination, conditional on an identifying assumption. The intuition is as follows. The HS critique rests on differences between blacks and whites in the variances of unobserved productivity. The fundamental problem, as Equations 10 and 10' show, is that we cannot separately identify the effect of race (γ') and a difference in the variance of the unobservables ($\sigma_B^{II}/\sigma_W^{II}$). But 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 is solved by invoking an identifying assumption – specifically, that there is variation in some applicant characteristics in the study that affect perceived productivity and have effects that are homogeneous across the races.

More formally, Equations 10 and 10' imply that the difference in outcomes between blacks and whites is

(11) $\Phi[(\beta_I'X^{I*} + \gamma' - c')/\sigma_B^{II}] - \Phi[(\beta_I'X^{I*} - c')/\sigma_W^{II}]$.

In a standard probit, we can only identify the coefficients relative to the standard deviation of the unobservable, so we normalize by setting the variance of the unobservable to equal one. In this case, impose the normalization for whites only, or $\sigma_W^{II} = 1$. The parameter σ_B^{II} is then the variance of the unobservable for blacks relative to whites. To make this clear, replace σ_B^{II} with $\sigma_{BR}^{II} = \sigma_B^{II}/\sigma_W^{II}$. The normalization $\sigma_W^{II} = 1$ is equivalent to defining all of the coefficients in Equation 11 as their ratios relative to σ_W^{II}. Dropping the prime subscripts to indicate that the coefficients are now defined in relative terms, with this normalization Equation 11 becomes

(11') $\Phi[(\beta_I X^{I*} + \gamma - c)/\sigma_{BR}^{II}] - \Phi[\beta_I X^{I*} - c]$.

As Equation 11' shows, without knowing σ_{BR}^{II} we cannot tell whether the intercepts of the two probits – and hence the hiring probabilities – differ because $\gamma \neq 0$ or because $\sigma_{BR}^{II} \neq 1$. However, if there is variation in the level of qualifications used as controls (X^{I*}), and these qualifications affect hiring outcomes, then we can identify β_I/σ_{BR}^{II} and β_I in Equation 11', and the ratio of these two estimates provides an estimate of σ_{BR}^{II}. This lets us test the hypothesis of equal standard deviations (or variances) of the unobservables. Finally, identification of σ_{BR}^{II} implies identification of γ. Without meaningful variation in X^{I*} (that is, the variation that affects hiring) this is not possible, since in that case all we have in the model are different intercepts with different parameters in both the numerators and the denominators ($(\gamma - c)/\sigma_{BR}^{II}$ and c).

The critical assumption to identify σ_{BR}^{II} and hence γ is that β_I is the same for blacks and whites. Otherwise, the ratio of the two coefficients of X^{I*} for blacks and whites does not identify σ_{BR}^{II}. As HS point out, the constancy of β_I is assumed in the Urban Institute studies that they critique, with discrimination entering through an intercept shift in the evaluation of a worker's productivity, depending on their race. It is not hard to come up with reasons why the coefficients relating X^I to productivity might differ by race. For example, blacks and whites on average attend different schools, and if whites' schools are higher quality, a given number of years of schooling may do more to increase white productivity than black productivity. But in a correspondence or audit study, it should be possible to control for these kinds of differences; for example, in this case one can control for the area where applicants live, and hence hold school district constant (for example, BM 2004). In other words, in these field experiments the researcher has the capacity to generate data making it more likely that the identifying assumption holds.

HS raise other possibilities. One is that there is discrimination in evaluating particular attributes of a group. For example, employers may discriminate against high-education blacks but not low-education blacks. It is not possible to rule out differences in coefficients arising for these reasons. Finally, HS also suggest that differences in coefficients may reflect "statistical information processing," given incomplete information about productivity, as in statistical discrimination models. Of course this is the idea underlying the identification strategy suggested above, as the difference in β_I for blacks and whites is assumed to reflect precisely the accuracy with which X^I signals productivity for each race. However, as discussed below, when there is data on multiple productivity-related characteristics, there is more one can do to test whether there is homogeneity in the coefficients that allows identification of $\sigma_{BR}{}^{II}$ and hence γ.

The estimation of $\beta_I/\sigma_{BR}{}^{II}$ and β_I, and inference on their ratio ($\sigma_{BR}{}^{II} = \sigma_B{}^{II}/\sigma_W{}^{II}$), can be done via a heteroskedastic probit model (for example, Williams 2009), which allows the variance of the unobservable to vary with race. To do this, pool the data for blacks and whites. Similar to Equation 5, letting i denote applicants and j firms, there is a latent variable for perceived productivity relative to the threshold, assumed to be generated by

(12) $\quad T(P_{ij}{}^{*}) = -\, c + \beta_I X_{ij}{}^{I*} + \gamma R_i + \varepsilon_{ij}.$

As is standard, it is assumed that $E(\varepsilon_{ij}) = 0$. But the variance is assumed to follow

(13) $\mathrm{Var}(\varepsilon_{ij}) = [\exp(\mu + \omega R_i)]^2$.

This model can be estimated via maximum likelihood. The observations should be treated as clustered on firms to obtain a variance-covariance matrix that is robust to the dependence of observations across firms. The normalization $\mu = 0$ can be imposed, given that there is an arbitrary normalization of the scale of the variance of one group (in this case whites, with $R_i = 0$). Then the estimate of $\exp(\omega)$ is exactly the estimate of σ_{BR}^{II}.

In this heteroskedastic probit model, the assumption that β_I is the same for blacks and whites identifies γ.[7] Observations on whites identify $-c$ and β_I, and observations on blacks identify $(-c + \gamma)/\exp(\omega)$ and $\beta_I/\exp(\omega)$. Thus, the ratio of $\beta_I/\{\beta_I/\exp(\omega)\}$ identifies $\exp(\omega)$, which, from Equation 13, is the ratio of the standard deviation of the unobservable for blacks relative to whites and is, as before, identified from the ratio of the effect of X^{I*} on blacks relative to its effect on whites. With the estimate of $\exp(\omega)$ (or equivalently σ_{BR}^{II}), along with the estimate of c identified from whites, the expression $(-c + \gamma)/\exp(\omega)$ identified from blacks identifies γ as well.[8]

If $\sigma_{BR}^{II} = 1$, then there is no bias from differences in the distribution of unobservables. Alternatively, if $\sigma_{BR}^{II} \neq 1$, but we had some evidence on how the level of standardization X^{I*} compares to the relevant population of job applicants, we could determine the direction of bias. For example, if the study points to discrimination and there is a bias *against* this finding – based on the estimate of the ratio of variances and information about X^{I*}, then the evidence of discrimination is not spurious, because it would be even stronger absent this bias. But because we can identify γ directly under the assumptions above, we can recover an estimate of discrimination that is not biased by the difference in the variances of the unobservables. And we can do this *without* determining whether X^{I*} used in the study is a high or low level of standardization, which may be impossible to establish.

The identification of γ and σ_{BR}^{II} depends on the assumption that the unobservables are distributed normally. However, the approach need not be couched solely in terms of normally distributed unobservables and the probit specification. What is required is the separate identification of the effect of race in the latent variable model and the relative variance of the unobservables for blacks and whites. Although typically (for example, Maddala 1983) the logit model is not written with the standard deviation of the error term appearing, it is possible to rewrite it in this way, in which case the difference in coefficients would again be informative about the ratio of the variances of the unobservables (Johnson and Kotz 1970, p. 5).

On the other hand, in the specific setting of this paper – a discrete outcome (hiring) and a discrete treatment (race) – there is no clear way to separately identify how race affects the latent variable and the variance *without* distributional assumptions. This case is covered most explicitly in Manski (1988), who shows that when – in this context – the distribution of the unobservable differs by race, identification of the "structural" coefficient (γ, in this case) requires a parametric specification of the unobservable distribution unless various kinds of continuity assumptions are imposed on the distribution of the observables (with a tradeoff between the tightness of the restrictions on the distribution of the unobservables and on the observables); but the latter do not hold for a discrete treatment.[9] There is a budding literature on identification of variables with dichotomous outcomes with non-parametric or semi-parametric methods. Most prominently, perhaps, Matzkin (1992) shows how this kind of model can be identified non-parametrically in contexts where restrictions from economic theory (concavity, homogeneity of degree one) apply. But these types of restrictions do not apply here.

Moreover, a specific form of the hiring rule is assumed. Different hiring rules are possible, such as one that minimizes the distance between a worker's skill level and the required skill level on the job (for example, Rothschild and Stiglitz 1982). There is no claim being made that the identification result or the conclusions about discrimination that follow are invariant to different assumptions about the distribution of the unobservables or the hiring rule. Rather than deriving general results, the goal is to show that, in the specific context explored by HS in which they showed that it was impossible to identify discrimination, an additional assumption – which itself has testable implications that are discussed next – permits the identification of discrimination. It remains a question for future research whether it is possible to treat the data from a correspondence study in a less restrictive manner and still learn something about the effects of interest.

The assumption that β_I is the same for blacks and whites cannot be tested if there is only one productivity control. With more controls, however, there is a testable restriction, because if the effects on hiring of multiple productivity controls differ between blacks and whites *only* because of the difference in the variance of the unobservables, the ratios of the estimated probit coefficients for blacks and whites, for each variable, should be the same. Consider the case with two observables X^I and Z^I, modifying Equation 11' to be

(11'') $\Phi[(\beta_I^B X^{I*} + \delta_I^B Z^{I*} + \gamma - c)/\sigma_{BR}^{II}] - \Phi[(\beta_I^W X^{I*} + \delta_I^W Z^{I*} - c)]$,

where the coefficients on the observables have B and W superscripts to denote possible differences by race. Normalize the coefficients in the first expression so that $\tilde{\beta}_I^B = \beta_I^B/\sigma_{BR}^{II}$ and $\tilde{\delta}_I^B = \delta_I^B/\sigma_{BR}^{II}$. If the coefficients in Equation 11'' do not differ by race, but only the variances of the unobservables differ, then

(14) $\tilde{\beta}_I^B/\beta_I^W = \tilde{\delta}_I^B/\delta_I^W$.

In other words, the black/white ratios of the coefficients from separate probits for blacks and whites, or from a probit with a full set of race interactions, differ from 1 only because $\sigma_{BR}^{II} \neq$

1, and hence should be equal. Thus, the restrictions implied by homogeneity of effects but unequal variances of the unobservables can be tested. Of course failure to reject the restrictions does not decisively rule out the possibility that $\sigma_{BR}^{II} = 1$, with the coefficients differing by race for other reasons but Equation 14 still holding. With a larger number of control variables, however, it seems unlikely that this alternative scenario would explain failure to reject the restrictions in Equation 14. It is also possible to choose – as an identifying assumption – a subset of the observable characteristics for which Equation 14 holds, and to identify σ_{BR}^{II} only from the coefficients of this subset of variables, or to do this and then test the restriction on the other coefficients as overidentifying restrictions.

A final issue concerns the interpretation of the coefficients from the heteroskedastic probit model. Consider a model with generic notation, where the latent variable depends on a vector of variables S and coefficients ψ, and the variance depends on a vector of variables T, which includes S, with coefficients θ. The elements of S are indexed by k. For a standard probit, coefficient estimates are translated into estimates of the marginal effects of a variable using

(15) $\partial P(\text{hire})/\partial S_k = \psi_k \phi(S\psi)$,

where S_k is the variable of interest with coefficient ψ_k, $\phi(.)$ is the standard normal density, and the standard deviation of the unobservable is normalized to one. Typically this is evaluated at the means of S. When S_k is a dummy variable such as race, the difference in the cumulative normal distribution functions is often used instead, although the difference is usually trivial.

The marginal effect is more complicated in the case of the heteroskedastic probit model, because if the variances of the unobservable differ by race, then when race “changes” both the variance and the level of the latent variable that determines hiring can shift. As long as we use the continuous version of the partial derivative to compute marginal effects from the heteroskedastic probit model, there is a natural decomposition of the effect of a change in a variable S_k that also appears in T into these two components. In particular, generalize the notation of Equation 13 to

(13') $\mathrm{Var}(\varepsilon) = [\exp(T\theta)]^2$,[10]

with the variables in T arranged such that the k^{th} element of T is S_k. Then the overall partial derivative of P(hire) with respect to S_k is

(16) $\partial P(\text{hire})/\partial S_k = \phi(S\psi/\exp(T\theta))\cdot\{(\psi_k - S\psi\cdot\theta_k)/\exp(T\theta)\}$.[11]

This expression can be broken into two pieces. First, the partial derivative with respect to changes in S_k affecting only the level of the latent variable – corresponding to the counterfactual of S_k changing the valuation of the worker without changing the variance of the unobservable – is equal to

(16') $\phi(S\psi/\exp(T\theta))\cdot\{\psi_k/\exp(T\theta)\}$.

Second, the partial derivative with respect to changes via the variance of the unobservable is equal to

(16'') $\phi(S\psi/\exp(T\theta))\cdot\{(-S\psi\cdot\theta_k)/\exp(T\theta)\}$.

In the analysis below, these two separate effects are reported as well as the overall marginal effect, and standard errors are calculated using the delta method. The effect of race via how it shifts the latent variable – or how race shifts the employer's valuation of worker productivity – is of greatest interest. The point of the HS critique is that differential treatment of blacks and whites based only on differences in variances of the unobservable should not be interpreted as discrimination. And moreover, as argued in Section VI, the effect of race via the latent variable captures discrimination likely to be manifested in the real economy, whereas its effect through the variance is more of an artifact of the study.

V. Evidence, Implementation, and Assessment

A. Existing Evidence

As the preceding discussion shows, we need information on the effects of productivity-related characteristics on hiring or callbacks, estimated separately for blacks and whites (or other groups), to identify discrimination in an audit or correspondence study, or even to assess likely biases. Reporting of such evidence is rare in the literature, because these studies typically create one "type" of applicant for which there is only random variation in characteristics that does not (or is not intended to) affect outcomes. However, BM's well-known correspondence study of race discrimination is unusual in that – for reasons unrelated to the concerns of this paper – it uses two types of applicants.[12]

Part of their analysis studies callback differences by race for resumes that they constructed to be low versus high quality, to ask whether blacks and whites have different incentives to invest in skills, as in Lundberg and Startz (1983). White callback rates are higher for both types of resumes. But although white callback rates increase significantly with resume quality (from 8.5 to 10.8 percent), black callback rates increase only slightly (from 6.2 to 6.7 percent) and the change is not statistically significant. Similar qualitative conclusions are reached based on an analysis that measures resume quality for one part of the sample based on the predicted probability of callbacks estimated from another part of the sample. In this analysis, both groups experience an increase in callback rates from higher-quality resumes, but the effect is larger for whites.

Similarly, BM report probit models estimated for whites and blacks separately (their Table 5). These estimates reveal substantially stronger effects of measured qualifications for whites than for blacks. Among the estimated coefficients that are statistically significant for at least one group, effects are larger for whites for experience,[13] having an email address, working while in school, academic honors, and other special skills (such as language). The only exception is for computer skills, which inexplicably have a negative effect on callback rates for whites.[14]

As the present paper suggests, an alternative interpretation of smaller estimated probit coefficients or marginal effects for blacks than for whites is a difference in the variance of the unobservables. In particular, the lower coefficients for blacks are consistent with a larger variance for blacks, or $\sigma_{BR}^{II} > 1$. If it is also true that BM standardized applicants at low levels of the control variables, then the HS analysis would imply that there is a bias towards finding discrimination *in favor of* blacks; that is, the evidence of discrimination against blacks would be even stronger absent the bias from differences in the distribution of unobservables. BM explicitly state that they tried to avoid overqualification even of the *higher-quality* resumes (p. 995). But it is very difficult to assess whether the characteristics of applicants were low, since there is no way to identify the population of applicants. Hence, implementation of the estimation procedure proposed in this paper is likely the only way even to *sign* the bias, let alone to recover an unbiased estimate of discrimination.

B. Implementation Using Bertrand and Mullainathan Data

Because BM's data include applicants with different levels of qualifications, and the qualifications predict callbacks, their data can be used to implement the methods described above. Table 1 begins by simply presenting probit estimates for the probability of a callback. Marginal effects are reported for specifications with no controls except a dummy variable for females, adding controls for the individual characteristics included on the resumes, and finally adding also neighborhood characteristics for the applicant's zip code; the specific variables are listed in the footnote to the table. Estimates are shown for males and females combined, and for females only; as the sample sizes indicate, the male sample is considerably smaller.[15] Aside from the estimated effects of race, estimates are shown for a few of the resume characteristics capturing applicants' qualifications.

Echoing BM's conclusions, there is a sizable and statistically significant difference between the callback rates for blacks and whites, with the rate for blacks lower by 3-3.3 percentage points (or about 33 percent relative to the white callback rate of 9.65 percent). The estimated race differences are robust to the inclusion of the different sets of control variables, which is what we should expect since the resume characteristics are assigned randomly.

Interestingly, in light of the results of other audit and correspondence studies, there is no evidence of lower callback rates for females than for males. And although not reported in the tables, this was true if the same methods used below to recover unbiased estimates of race discrimination were applied to the estimation of sex discrimination. However, BM's study was to a large extent focused on jobs typically held by females, and was not designed to test for sex discrimination.[16] Table 1 also shows that a number of the resume characteristics have statistically significant effects on the callback probability; this, of course, is an essential input for using the methods described above to recover an unbiased estimate of discrimination.

The main analysis is reported beginning in Table 2, for the specifications with the full set of individual resume controls, and then adding as well the full set of neighborhood controls. Panel A simply repeats the estimated race effects from Table 1, for comparison. Panel B begins by reporting the estimated overall marginal effects of race from the heteroskedastic probit model (Equation 16). As the table shows, these estimates are slightly smaller (in absolute value) than the estimates from the simple probits. They remain statistically significant and indicate callback rates that are lower for blacks by about 2.4-2.5 percentage points (or about 25 percent).

Decomposing the marginal effect, the effect via the level of the latent variable is larger than the marginal effect from the probit estimation, ranging from −0.054 to −0.086. The effect of race via the variance of the unobservable, in contrast, is positive, ranging from 0.028 to 0.062. (This latter effect is not statistically significant.) The implication is that race discrimination is more severe than indicated by the analysis that ignores differences in the variances of the unobservables. The evidence that the probit estimates understate discrimination against blacks is consistent with a low level of standardization of X^{I*}, coupled with a higher estimated variance of the unobservable for blacks, as conjectured earlier based on BM's results. And as reported in the next row of the table, the estimated ratio of the standard deviation of the unobservable for blacks to the standard deviation for whites always exceeds one, although the difference is not statistically significant. The positive effect of being black via the variance is what we expect if X^{I*} is low, since then a larger relative variance for blacks increases the relative probability that they are hired (called back).

The next two rows of the table report diagnostic test statistics. First, the p-values from the test of the overidentifying restrictions (Equation 14) are shown, based on probit specifications interacting all of the controls with race. In all four cases the restrictions are not rejected, with p-values ranging from 0.17 to 0.62. Nonetheless, the lower end of this range of p-values suggests that the restrictions sometimes might be fairly inconsistent with the data. As a consequence, below some alternative estimates are discussed that use only a subset of variables, for which Equation 14 is more consistent with the data, to identify discrimination.

Finally, the subset of control variables for which the absolute value of the estimated coefficient for whites exceeded that for blacks – consistent with the larger standard deviation of unobservables for blacks – was identified. Then the heteroskedastic probit model was estimated leaving the race interactions of the other variables in the model – so that the restrictions from Equation 14 that were less consistent with the data were not imposed – and the joint significance of these latter variables was tested. Despite this latter subset of variables having estimated coefficients less consistent with the restrictions in Equation 14, the p-values indicate that these interactions can also be excluded from the model. This can be viewed as an overidentifying test of the restriction that there are no differences in the effects of any of the control variables by race, for the specifications for which the estimates are reported in the first row of Panel B. Although technically it is only necessary to assume that there is a single variable for which the coefficient is the same for blacks and whites, there is no obvious variable to choose for the purposes of identification; here, instead, I let the data select a set of variables more consistent with the identifying restriction.

Table 3 follows up on the last procedure, by instead simply dropping from the analysis the control variables for which the absolute value of the estimated coefficient for whites was less than for blacks. Given that the control variables are random with respect to race, dropping controls does not introduce bias. As we would expect, the p-values for the tests of this set of restrictions are now much closer to one, ranging from 0.68 to 0.92, compared with a range of 0.17 to 0.62 in Table 2. However, as the table shows, the estimated effects of race are similar to those in Table 2 and do not point to any different conclusions.

C. Monte Carlo Assessment

A Monte Carlo assessment of how well the estimation procedure proposed in this paper works in terms of removing the bias in estimates of discrimination from correspondence study evidence was carried out. The Monte Carlo assessment uses simulated data of the type needed to implement the estimator – namely, data with applicants at two different levels of productivity. The analysis is described in detail in Appendix 1; here the findings are briefly summarized.

First, using simulated data either with or without discrimination, the heteroskedastic estimation procedure eliminates the bias, and recovers an unbiased estimate of discrimination. (And as a benchmark, the simulated data are used to replicate the HS critique, showing that a simple probit analysis can generate bias in any direction.) Second, for a specific case we consider, when the identifying assumption is violated and the coefficient on the productivity-related characteristic in the simulated data is not equal for blacks and whites, but is treated as equal in the estimation, there are two findings. When there is no discrimination, the misspecification has no effect; the estimator still produces estimates of γ centered on zero. When there is discrimination and the model is misspecified, there is bias but it is multiplicative, so the estimator will not generate the wrong sign for the estimate of γ.

VI. The Meaning of Discrimination

The fact that a correspondence study can generate evidence of discrimination when $\gamma = 0$ raises the question of whether the evidence reflects a different kind of discrimination. In this case, the productivity of blacks and whites are regarded equally by employers (or equivalently there is no taste discrimination). Moreover, employers are not making any assumption about mean differences in unobservables between blacks and whites. However, they are making assumptions about distributional differences with regard to the variance of unobservables, and it is these assumptions that lead them, given the level of standardization of the study applicants, to prefer blacks or whites – which might be labeled "second-moment" statistical discrimination.

The HS critique can be recast as showing that the analysis of data from a standard audit or correspondence study cannot distinguish between discrimination as it usually interpreted, and discrimination based on different variances of unobservables. Indeed Manski's (1988) paper discussed earlier, in the context of identification, makes this same point, noting that we can estimate the "reduced-form" effect of a binary treatment variable on a binary outcome without strong assumptions, but not the "structural" effect. The present paper imposes an assumption to identify the structural parameter γ, distinguishing between what is typically viewed as discrimination (stemming from tastes or, as noted earlier, from standard statistical discrimination) and different treatment stemming from differences in variances of the unobservable.

A natural question, then, is whether the structural effect of race, captured in γ, is of interest, or whether instead all we want to know is the reduced-form effect of race – that is, how race affects the probability of hiring *whether* because employers discount black workers' productivity (for example) or because employers treat blacks and whites differentially because of different distributions of the unobservable. There are two reasons why the structural coefficient γ is important. 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.[17] Thus, identification of γ speaks to the discrimination that is most clearly illegal.

The second and more compelling argument is that taste discrimination or "first-moment" statistical discrimination, captured 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 to these firms.

Suppose that the actual population of applicants to the employers included in a correspondence study comes from a large range of values of X^I. Then the different distributions of the unobservables by race can have strong effects on hiring outcomes in the correspondence study because in the study the applicants are standardized to a narrow range of X^I. In that sense, the differential treatment by race is strongly influenced by the design of the correspondence study, rather than by behavior of real firms evaluating real applicants, and can be generated *solely* from the standardization of applicants in a narrow range of X^I; in that sense, "discrimination" attributable to differences in the variance of the unobservables is an artifact of the study.[18] In contrast, the structural effect of race via the latent variable would generalize to how the firms in the correspondence study, and presumably similar firms, evaluate actual job applicants and make hiring decisions.

VII. Conclusions and Discussion

Many researchers view audit and correspondence studies as the most compelling way to test for labor market discrimination. And research applying these methods to many different types of groups nearly always finds evidence of discrimination. The use of audit studies to test for labor market discrimination has been criticized on numerous grounds having to do with whether applicants from different groups appear identical to employers. Many of these criticisms can be countered by using correspondence studies of fictitious applicants on paper rather than fictitious in-person applicants.

However, Heckman and Siegelman (1993) show that even in correspondence studies in which group averages are identical conditional on the controls, group differences in the variances of unobservable dimensions of productivity can invalidate the empirical tests, leading to spurious evidence of discrimination in either direction, or spurious evidence of an absence of discrimination. This is a fundamental criticism of correspondence studies, as it implies that evidence regarding discrimination from even the best-designed correspondence study can be misleading. Nonetheless, this criticism has been ignored in the literature.

This paper shows that if a correspondence study includes observable measures of variation in applicants' quality that affect hiring outcomes, an unbiased estimate of discrimination can be recovered even when there are group differences in the variances of the unobservable. The method is applied to Bertrand and Mullainathan's (2004) correspondence study, and leads to stronger evidence of race discrimination that adversely affects blacks than is obtained when differences in the variances of the unobservable are ignored. Moreover, this conclusion is bolstered by Monte Carlo simulations suggesting that the estimation procedure performs well, eliminating the problems highlighted by Heckman and Siegelman that could otherwise lead to badly misleading conclusions from the analysis of data from correspondence (or audit) studies.

Finally, it should be recognized that the method proposed here can be easily implemented in any future correspondence (or audit) study. All that is needed is for the resumes or applicants to include some variation in characteristics that affect the probability of being hired.[19] This is different from what is often done in designing these studies, where researchers try to create a pool of equally-qualified applicants. All that needs to be done is to intentionally create resumes of different quality. Once a researcher confirms that a set of productivity-related characteristics on the resumes affected hiring outcomes, it should then be possible – conditional on an identifying assumption that has testable implications – to detect discrimination.

Appendix 1. Monte Carlo Assessment

This appendix provides Monte Carlo evidence on how well the estimation procedure proposed in this paper works in terms of removing the bias in estimates of discrimination from correspondence study evidence, and explores the consequences of violation of the identifying assumption. Figure A1 replicates the basic result from Heckman (1998), showing that probit analysis of the data from a correspondence study can generate substantial bias in either direction. Paralleling Heckman, this is done for the case in which c = 0, $\beta_I = 1$, $Var(X_W^{II})/Var(X_B^{II}) = (\sigma_W^{II})^2/(\sigma_B^{II})^2 = 2.25$,[20] and there is no discrimination ($\gamma = 0$). For the Monte Carlo simulations, the assumed data generating process is $X^{I*} \sim N(0,1)$, $X_B^{II} \sim N(0,1)$, $X_W^{II} \sim N(0,2.25)$. Paralleling the standardization of correspondence study applicants, the data are generated by sampling X^{I*} from a truncated normal distribution, in steps of $0.1 \pm 0.1 \cdot SD(X^{I*})$. The simulation is done 100 times at each value of X^{I*} shown in the graph, with samples of 2,000 blacks and 2,000 whites in each simulation (roughly BM's sample sizes), and a probit model is estimated for each simulated data set. The panels in the figure show – for both the estimates of γ and the marginal effects – the true values based on the assumed parameters, and the means based on the estimates.[21]

The upper figures clearly illustrate that, despite the absence of discrimination in the data generating process (the true effect is constant at 0), the evidence can either point to discrimination against blacks or discrimination in favor of blacks, depending on the level of standardization of X^{I*}. The marginal effects show that even though there is no discrimination in the data generating process, quite strong evidence of discrimination in either direction can emerge, with a marginal effect of −0.1 (0.1) for low (high) values of X^{I*}. Finally, as we would expect, only at $X^{I*} = 0$ is the estimate of γ (and the marginal effect) unbiased. The lower panels of Figure A1 report the same kind of evidence when $\gamma = -0.5$, consistent with discrimination. A similar result is apparent, with substantial bias relative to the true γ or the true marginal effect.

The heteroskedastic probit estimation requires data with multiple levels of the value of X^{I*} at which applicants are standardized. As an intermediate step to separate the consequences of generating the data this way, and the consequences of implementing the heteroskedastic probit estimator, Figure A2 shows results with such generated data, but continuing to use the probit specification. X^{I*} is now sampled from two truncated normal distributions, one using X^{I*} in steps of $0.1 \pm 0.1 \cdot SD(X^{I*})$, as before, and the second using instead $X^{I*} + 0.5$, again in steps of $0.1 \pm 0.1 \cdot SD(X^{I*})$. Figure A2 shows qualitatively similar results to Figure A1, so simply using data with variation in productivity-related characteristics does not, in itself, eliminate the bias. Nonetheless, the biases in both the no discrimination and discrimination cases are a bit smaller than in Figure A1 because of the larger range covered by X^{I*}.[22]

Figure A3 reports results for the heteroskedastic probit estimation, using the same data generating process for simulating data as in Figure A2, although in this case 5,000 simulations are run for each pair of values of X^{I*} because the heteroskedastic probit estimation is less precise than the simple probit estimation. The top panel covers the no discrimination case ($\gamma = 0$). The left-hand graph shows the means of the true and estimated values of the marginal effects for each value of X^{I*}. These are largely indistinguishable in the figure, indicating no bias. The right-hand panel provides evidence on the distribution of the estimates, showing the distance between the 25th and 75th percentiles of the estimates and between the 2.5th and 97.5th percentiles at each value of X^{I*}. The distribution of estimates is quite tight at levels of standardization near the center of the distribution of X^{I*}, but becomes wider at more extreme values, when hiring rates in the generated data move towards zero or one. The discrimination case ($\gamma = -0.5$) similarly demonstrates that the heteroskedastic probit estimation eliminates the bias.

The last analysis, reported in Figure A4, considers the implications of the data generating process violating the identifying assumption that the coefficient(s) on the productivity-related characteristics are equal for blacks and whites. Results are presented for two cases: mild violation in which the coefficient on X^{I*} (β_I) is slightly larger for whites than for blacks (1.1 versus 1); and strong violation in which it is much larger (2 versus 1). As Figure A4 shows, in the case of no discrimination – the left-hand panels – the results are indistinguishable from when the identifying assumption is not violated. In contrast, in the discrimination case the estimated marginal effects become more negative than the true effects over much of the range, only slightly with mild violation of the identifying assumption, but more so when the violation is more pronounced.

The implications of what happens when the identifying assumption is violated in this specific setting make sense, thinking about how γ is identified. Using estimates of the separate probits in Equations 10 and 10', the ratio of the standardized white probit coefficient to the black probit coefficient identifies σ_{BR}^{II} (which equals $\sigma_B^{II}/\sigma_W^{II}$). When the true value of β_I is larger for whites than for blacks, but it is assumed that they are equal, σ_{BR}^{II} is overestimated. For example, in the case in the top panel of Figure A4, the ratio of coefficients is $(\beta_I \cdot 1.1)/(\beta_I/\sigma_{BR}^{II}) = 1.1 \cdot \sigma_{BR}^{II}$. Recall from the earlier discussion that the probit for blacks identifies $(-c + \gamma)/\exp(\omega) = (-c + \gamma)/\sigma_{BR}^{II}$. Because $c = 0$ in the simulations, we identify γ by multiplying the estimate of this expression by the estimate of σ_{BR}^{II}; the upward bias in the estimate of σ_{BR}^{II} therefore implies that the estimate of γ is biased away from zero. In the no discrimination case, when $\gamma = 0$, this is irrelevant; multiplying an estimate that averages zero by the upward-biased estimate of σ_{BR}^{II} has no effect. But when the true γ is non-zero (and negative), this bias leads to an estimate of γ that is more negative. When γ is more negative, we get exactly the "bending" of the estimated marginal effects that the right-hand panels of Figure A4 illustrate.[23] A violation of the assumption in the opposite direction (β_I larger for blacks) would lead to biases in the opposite direction. Nonetheless, it follows from this reasoning that the bias is multiplicative, and hence does not generate the wrong sign for the estimate of γ, or generate spurious evidence of discrimination when there is no discrimination. However, further analysis shows that when $c \neq 0$ (or, more generally, when c is not equal to the expected value of unobserved productivity), the implications of violation of the identifying assumption are less sharp.

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Table 1
Probit Estimates for Callbacks: Basic Results

	Males and females			Females		
	(1)	(2)	(3)	(4)	(5)	(6)
Black	-.033	-.030	-.030	-.033	-.030	-.030
	(.006)	(.006)	(.006)	(.008)	(.007)	(.007)
Female	.009	-.001	.001	…	…	…
	(.012)	(.011)	(.011)			
Selected individual resume controls						
Bachelor's degree		.009	.009		.019	.019
		(.009)	(.009)		(.010)	(.010)
Experience $\cdot 10^{-1}$		.080	.076		.080	.076
		(.029)	(.028)		(.034)	(.033)
Experience$^2 \cdot 10^{-2}$		-.022	-.021		-.019	-.018
		(.011)	(.010)		(.013)	(.012)
Academic honors		.039	.040		.026	028
		(.015)	(.015)		(.017)	(.017)
Special skills		.056	.055		.060	.059
		(.009)	(.009)		(.010)	(.010)
Other controls:						
Individual resume characteristics		X	X		X	X
Neighborhood characteristics			X			X
Mean callback rate	.080	.080	.080	.082	.082	.082
N	4,784	4,784	4,784	3,670	3,670	3,670

Note: Marginal effects using Equation 15 are reported. Standard errors are computed clustering on the ad to which the applicants responded, and are reported in parentheses; the delta method is used to compute standard errors for the marginal effects. Individual resume characteristics include bachelor's degree, experience and its square, volunteer activities, military service, having an email address, gaps in employment history, work during school, academic honors, computer skills, and other special skills. Neighborhood characteristics include the fraction high school dropout, college graduate, black, and white, as well as log median household income, in the applicant's zip code.

Table 2
Heteroskedastic Probit Estimates for Callbacks: Full Specifications

	Males and females		Females	
	(1)	(2)	(3)	(4)
A. Estimates from basic probit (Table 1)				
Black	-.030	-.030	-.030	-.030
	(.006)	(.006)	(.007)	(.007)
B. Heteroskedastic probit model				
Black (unbiased estimates)	-.024	-.026	-.026	-.027
	(.007)	(.007)	(.008)	(.008)
Marginal effect of race through level	-.086	-.070	-.072	-.054
	(.038)	(.040)	(.040)	(.040)
Marginal effect of race through variance	.062	.045	.046	.028
	(.042)	(.043)	(.045)	(.044)
Standard deviation of unobservables, black/white	1.37	1.26	1.26	1.15
Wald test statistic, null hypothesis that ratio of standard deviations = 1 (p-value)	.22	.37	.37	.56
Wald test statistic, null hypothesis that ratios of coefficients for whites relative to blacks are equal, fully interactive probit model (p-value)	.62	.42	.17	.35
Test overidentifying restrictions: include in heteroskedastic probit model interactions for variables with |white coefficient| < |black coefficient|, Wald test for joint significance of interactions (p-value)	.83	.33	.34	.56
Number of overidentifying restrictions	3	6	2	6
Other controls:				
Individual resume characteristics	X	X	X	X
Neighborhood characteristics		X		X
N	4,784	4,784	3,670	3,670

Note: See notes to Table 1. In the first row of Panel B the marginal effects in Equation 16 are reported, with the decomposition in Equations 16’ and 16’’ immediately below; the marginal effects are evaluated at sample means. The standard errors for the two components of the marginal effects are computed using the delta method. Test statistics are based on the variance-covariance matrix clustering on the ad to which the applicants responded. Individual resume characteristics also include the variables listed separately in Table 1.

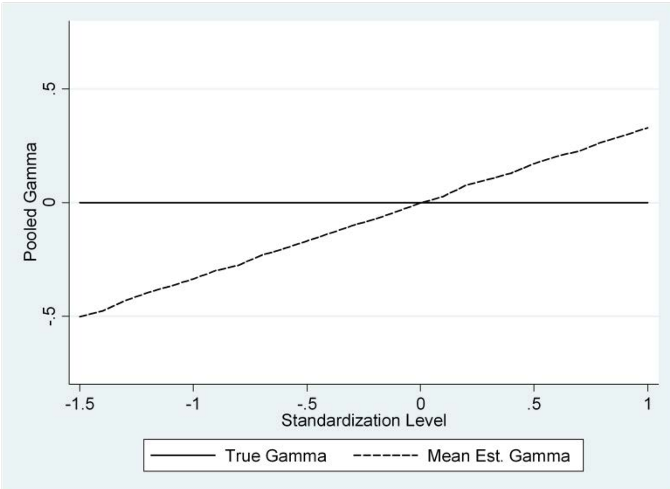
Table 3
Heteroskedastic Probit Estimates for Callbacks: Restricted Specifications Using only Controls with Absolute Value of Estimated Effect in Fully Interactive Probit Model Larger for Whites than Blacks

	Males and females		Females	
	(1)	(2)	(3)	(4)
A. Estimates from basic probit				
Black	-.030	-.030	-.030	-.030
	(.006)	(.006)	(.007)	(.006)
B. Heteroskedastic probit model				
Black (unbiased estimates)	-.024	-.025	-.024	-.025
	(.007)	(.007)	(.009)	(.008)
Marginal effect of race through level	-.090	-.080	-.086	-.077
	(.037)	(.036)	(.040)	(.038)
Marginal effect of race through variance	.066	.056	.062	.052
	(.041)	(.039)	(.044)	(.042)
Standard deviation of unobservables, black/white	1.41	1.33	1.37	1.30
Wald test statistic, null hypothesis that ratio of standard deviations = 1 (p-value)	.19	.23	.25	.29
Wald test statistic, null hypothesis that ratios of coefficients for whites relative to blacks are equal, fully interactive probit model (p-value)	.84	.92	.68	.74
Other controls:				
Individual resume characteristics	X	X	X	X
Neighborhood characteristics		X		X
N	4,784	4,784	3,670	3,670

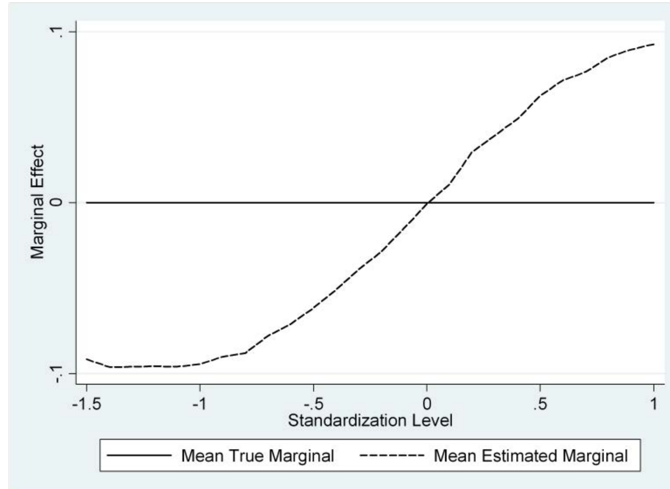
Notes: See notes to Tables 1 and 2.

No discrimination (γ = 0)

Probit estimates of γ

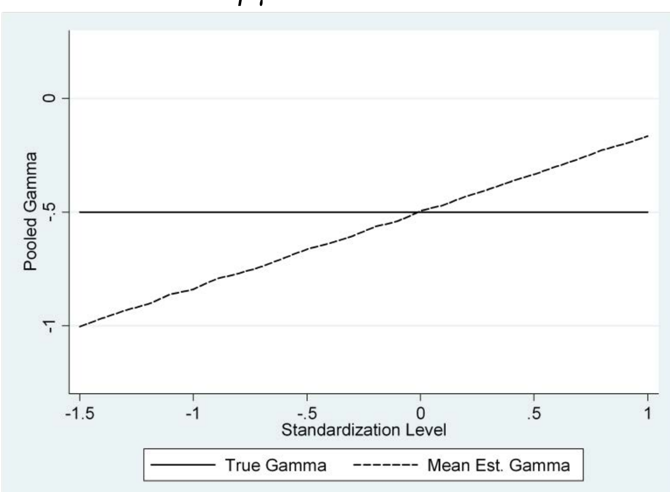


Probit estimates of marginal effect of black

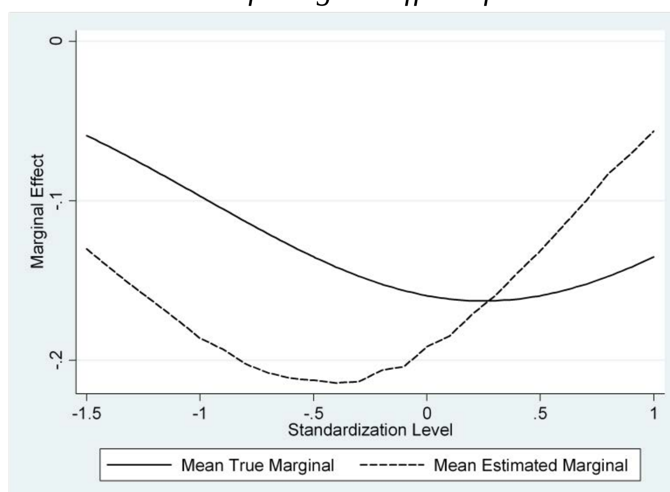


Discrimination (γ = -.5)

Probit estimates of γ



Probit estimates of marginal effect of black



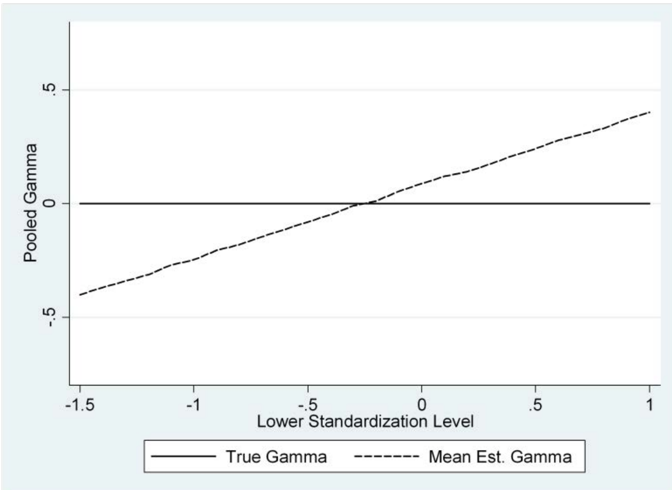
Notes: Left-hand graphs shows true γ and mean estimated γ. Right-hand graphs show marginal effects, evaluated at sample means for simulated data. In the data generating process, $X^{I*} \sim N(0,1)$, $X_B^{II} \sim N(0,1)$, $X_W^{II} \sim N(0,2.25)$, so $Var(X_W^{II})/Var(X_B^{II}) = 2.25$ (X_W^{II} and X_B^{II} are unobservable); $\beta_I = 1$ and $c = 0$ for both blacks and whites. Estimates are generated by Monte Carlo simulation, drawing 4,000 observations (2,000 white and 2,000 black) from truncated normal distribution at each value of X^{I*} (in steps of $0.1 \pm 0.1 \cdot SD(X^{I*})$) and estimating probit model. Simulation is done 100 times at each value of X^{I*}.

Figure A1

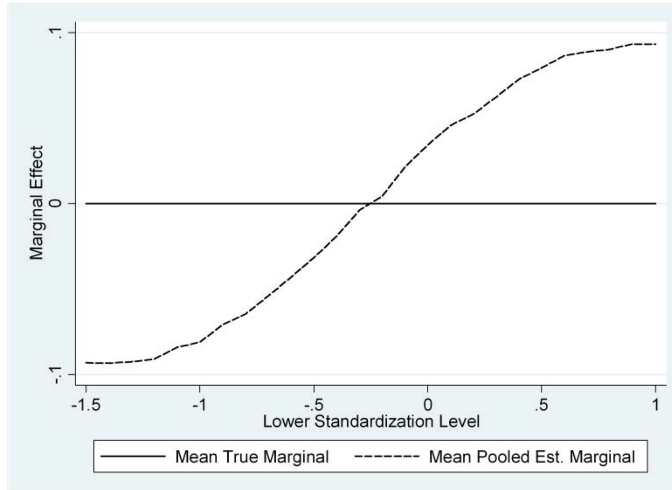
Replication of Heckman (Figure 1, 1998), and Monte Carlo Simulations of Estimates of Marginal Effects from Simple Probit Estimation

No discrimination (γ = 0)

Probit estimates of γ

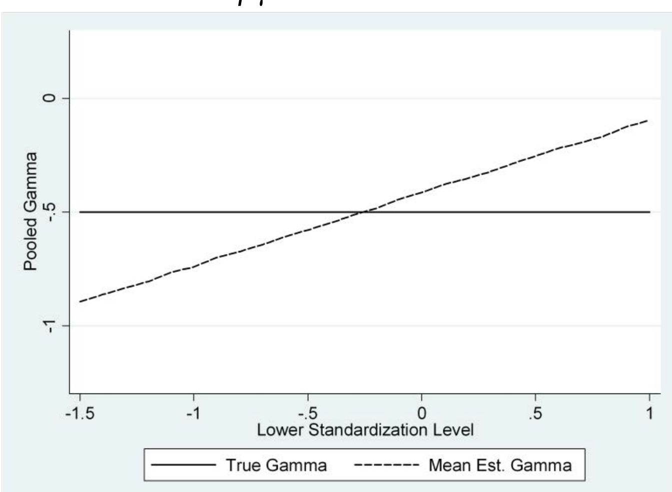


Probit estimates of marginal effect of black

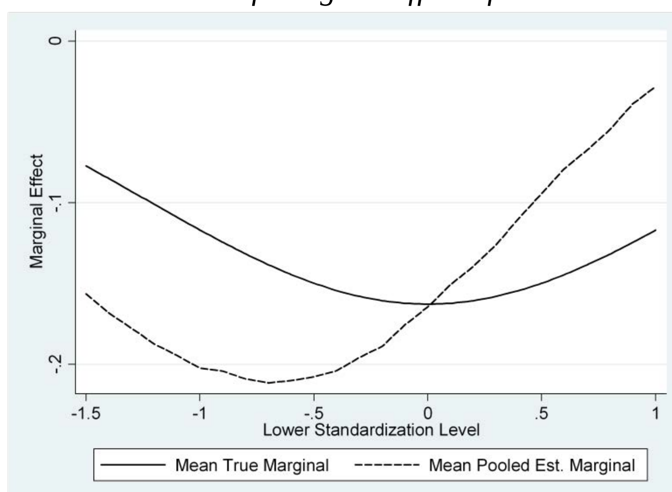


Discrimination (γ = -.5)

Probit estimates of γ



Probit estimates of marginal effect of black



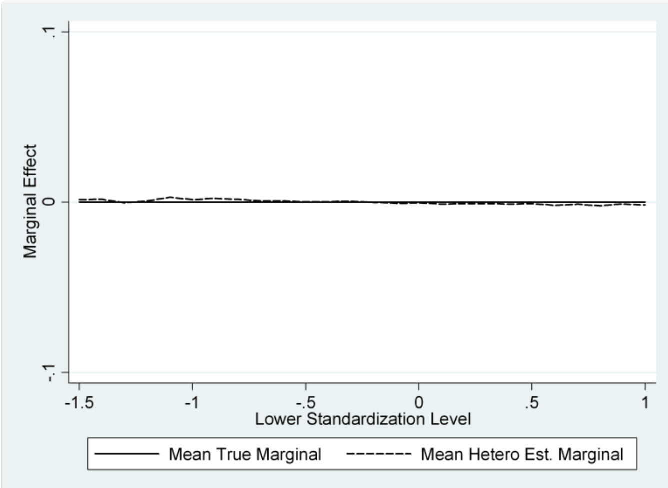
Notes: See notes to Figure A1. The only difference is as follows: Estimates are generated by Monte Carlo simulation, drawing 4,000 observations (2,000 white and 2,000 black) observations from two truncated normal distributions (one at each value of X^{I*} (in steps of 0.1 $\pm$ 0.1·SD(X^{I*})), *and one at each value of* X^{I*} + *.5* (again in steps of 0.1) $\pm$ 0.1·SD(X^{I*})), and estimating probit model.

Figure A2

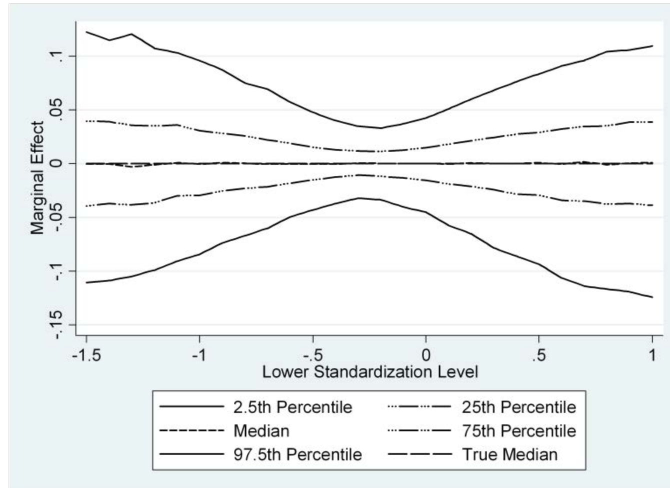
Monte Carlo Simulations of Probit Estimates and Marginal Effects from Simple Probit Estimation with Two Types of Applicants

No discrimination (γ = 0)

Estimates of marginal effects of black

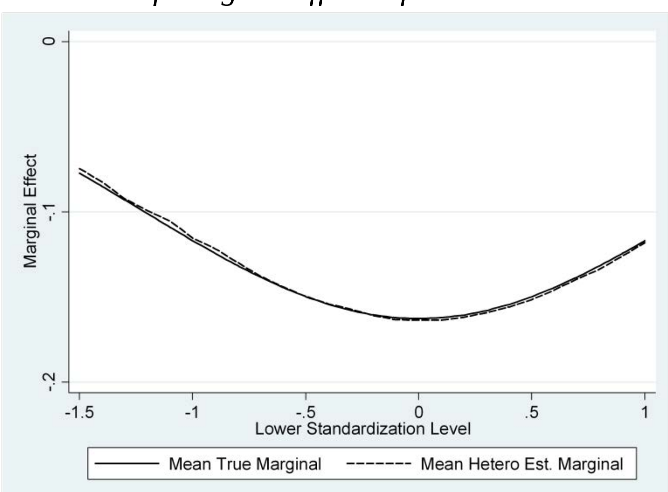


Distribution of estimates

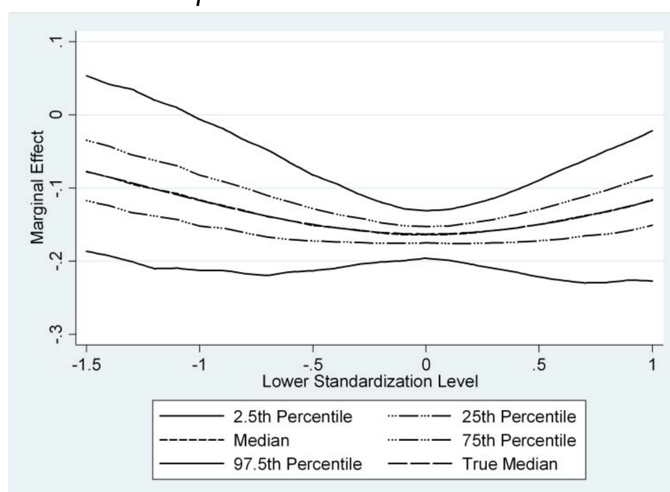


Discrimination (γ = -.5)

Estimates of marginal effects of black



Distribution of estimates



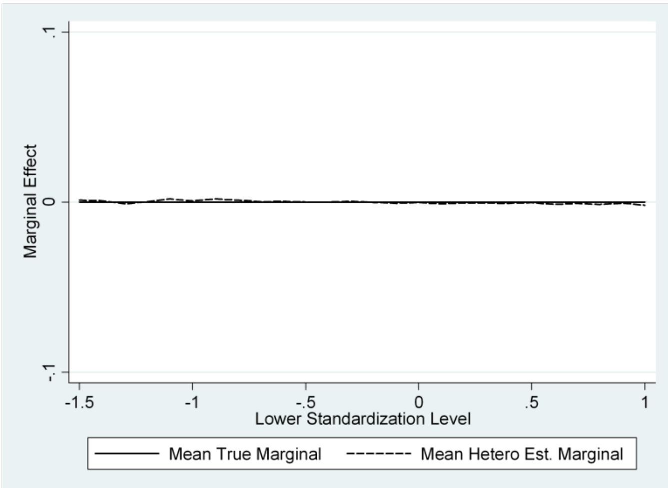
Notes: Left-hand graphs show mean true and estimated marginal effects of being black, evaluated at sample means for simulated data. Right-hand graphs show distributions of estimates. The marginal effects shown correspond to the effect of race on the latent variable, as in Equation 16'. Estimates are generated by Monte Carlo simulation, drawing 4,000 observations (2,000 white and 2,000 black) observations from two truncated normal distributions (one at each value of X^{I*} (in steps of 0.1 $\pm$ 0.1·SD(X^{I*})), and one at each value of X^{I*} + .5 (again in steps of 0.1 $\pm$ 0.1·SD(X^{I*})), and estimating heteroskedastic probit model. Simulation is done 5,000 times at each value of X^{I*} shown in graph. As in Figure A1, the data generating process has $X^{I*} \sim N(0,1)$, $X_B^{II} \sim N(0,1)$, $X_W^{II} \sim N(0,2.25)$, so $Var(X_W^{II})/Var(X_B^{II}) = 2.25$ (X_W^{II} and X_B^{II} are unobservable); and $\beta_I = 1$ and c = 0 for both blacks and whites.

Figure A3

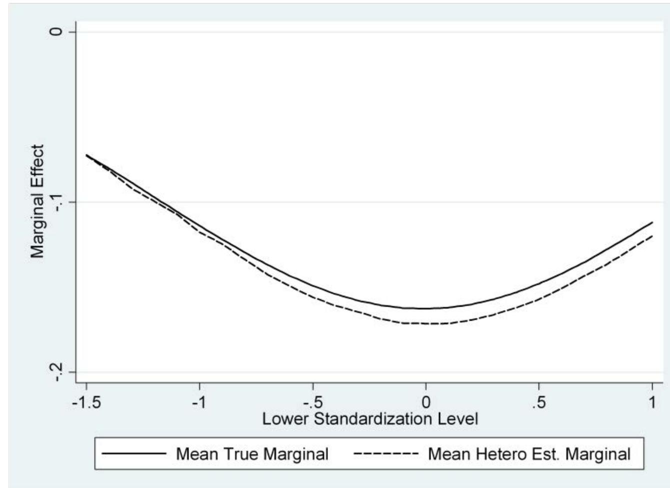
Monte Carlo Simulations of Heteroskedastic Probit Estimation, Estimates of Marginal Effects and Distributions

Mild violation of identifying assumption in data generating process (β_I for whites = 1.1)

No discrimination (γ = 0)

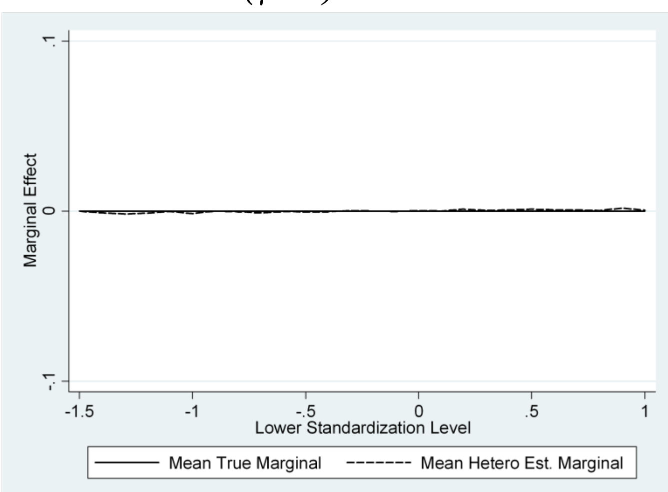


Discrimination (γ = -.5)

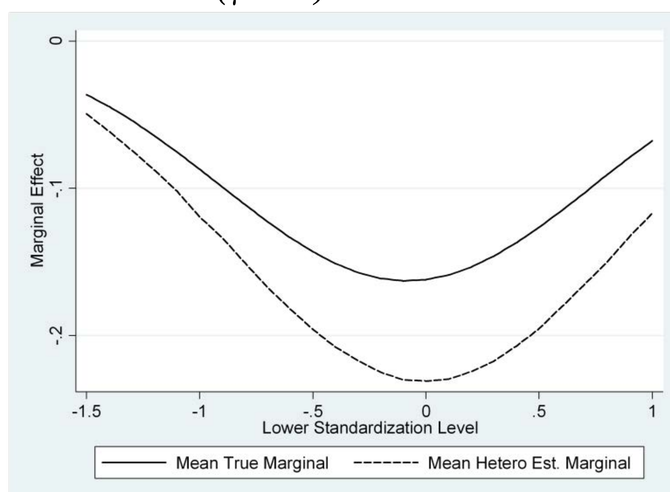


Strong violation of identifying assumption in data generating process (β_I for whites = 2)

No discrimination (γ = 0)



Discrimination (γ = -.5)



Notes: See notes to Figure A3. The only difference is that β_I is unequal for blacks and whites; it is always equal to 1 for blacks, and as indicated in the graph headings for whites.

Figure A4

Monte Carlo Simulations of Heteroskedastic Probit Estimation, with Model Misspecification Masking Higher Unobserved Variance for Whites, Estimates of Marginal Effects

Endnotes

[1] In the ensuing discussion X^{I} is observable and X^{II} unobservable. I put a coefficient on X^{I} in Equation 2 because X^{I} has a definable scale, whereas I treat X^{II} as scaled such that its coefficient equals one. Following HS, I assume that the coefficient on X^{I} is the same for blacks and whites, so discrimination is reflected only in an intercept difference. I return to this issue later.

[2] We could include resume characteristics, which should not matter for the estimate of γ' since they are randomly assigned by race. The same is true of firm fixed effects, which are orthogonal to race.

[3] As discussed below, some characteristics of workers not controlled by the researcher are observed by the employer in an audit study; but many are not.

[4] This is more problematic in correspondence studies of age discrimination, because even with many other qualifications on the resumes, if researchers give older applicants the same amount of experience as younger applicants, employers are likely to make adverse assumptions about older applicants whose resumes reflect limited work experience. See Lahey (2008) and Riach and Rich (2007) for suggestions for addressing this problem in age discrimination studies.

[5] We can treat F as statistically independent because resume characteristics are, or should be, assigned randomly (for example, Lahey and Beasley 2009). And we can always think about X^{II} as the variation in unobserved productivity that is orthogonal to X^{I}.

[6] Heckman (1998, footnote 7) suggests that the case with a low level of standardization and higher dispersion for whites "seems to rationalize" audit study evidence of discrimination against blacks. It is not clear, however, that we know either the level of standardization or the relative dispersion of unobserved productivity. Even though the first issue relates to observables, there is no obvious way to compare the distributions of qualifications of testers in an audit study to the population of job applicants. In fact, there are two conflicting tendencies in setting standards for

audit studies. Setting a low standard implies that call-back rates will be low, reducing the statistical power of the evidence. But setting a standard too high raises concerns about "overqualification" of candidates (for example, BM 2004, p. 995), which in an economic context presumably means that the job applicant will be expected to get better job offers, deterring employers from making offers.

[7] Conversely, the following argument can be easily modified to show that if β_I is *not* the same for blacks and whites, then γ is unidentified. The assumption that only race shifts the variance of the unobservable (Equation 13) is less consequential, since in a correspondence (or audit) study the observed characteristics of applicants other than race are essentially the same.

[8] Consistent with the earlier discussion of statistical discrimination, we might want to allow for the possibility that $E(X_B^{II}) - E(X_W^{II}) \neq 0$. In this case, we can normalize by assuming $E(X_W^{II}) = 0$ and defining $E(X_B^{II}) - E(X_W^{II}) = \mu_{BW}^{II}$. We can then replace γ in the preceding identification argument with $\gamma + \mu_{BW}^{II}$, and it is this sum of parameters, reflecting the combination of taste discrimination and the expected mean difference in the unobservable (statistical discrimination), which is identified.

[9] Some of the other controls on the resumes may be continuous, but they do not contribute to identification of the effect of race on the latent variable given that they are, by construction, orthogonal to race. By way of contrast, in audit-type studies of discrimination on a continuous outcome – such as price quotes for cars or car repairs (Goldberg 1996; Gneezy and List 2004) – the identification problem is simpler.

[10] Recall that μ in Equation 13 is normalized to zero.

[11] See Cornelißen (2005).

[12] BM actually study differences in treatment between applicants with black-sounding names and names that do not sound black. For simplicity, I discuss the results as if they capture differences between blacks and whites, which is certainly a plausible interpretation of their findings.

[13] This variable enters as a quadratic, and the effect of experience is stronger for whites up to about 16 years of experience, more than twice the mean in their sample.

[14] Also, the effect of gaps in employment is inexplicably positive, but not significant for estimates disaggregated by race.

[15] Probits estimated for males only yielded similar results for the effects of race, although the estimated coefficients of some of the productivity-related characteristics were quite imprecise or had unexpected signs. In estimating the heteroskedastic probit model for males, in some cases there were computational problems, likely reflecting these other issues regarding the estimates for males, and perhaps also the much smaller sample for males.

[16] They study sales, administrative support, clerical, and customer service jobs. The male applicants were used almost exclusively for the sales jobs, so the sex difference is identified mainly from the sales jobs.

[17] There is a small economics literature that studies discrimination based on second moments – in particular the variance of productivity – where a group with higher variance may be penalized because of risk aversion on the part of employers. The idea goes back to Aigner and Cain (1977). Dickinson and Oaxaca (2009) provide an experimental study of this type of discrimination in labor markets.

[18] One could imagine more complicated scenarios in which different firms engage in strategies that draw applicants from different parts of the distribution, in which case it is harder to

characterize how differences in the distribution of unobservables by race will impact the treatment of whites versus blacks.

[19] In principle this can be done in an audit study as well a correspondence study, although it is much harder to generate large samples in audit studies.

[20] This is the ratio of the variances of the unobservables. Note that a larger value for whites is the opposite of the common assumption in models of statistical discrimination.

[21] The true marginals are based on the heteroskedastic probit specification (Equation 16'), because the simulated data are heteroskedastic. The true marginal effect is reported as a mean because it is computed once using each simulated data set, holding the parameters fixed, and then averaged.

[22] In this case the unbiased estimate occurs at the value of −0.25 (for X^{I*}) on the horizontal axis, where the average of the upper and lower standardization levels equals zero. The reduction in bias is a little less clear in the discrimination case. To clarify, the bias in Figures A1 and A2 should be contrasted at comparable values of X^{I*}, given that Figure A2 shows the mean estimates at the lower level of standardization of X^{I*}. For example, for the discrimination case, the mean estimate of γ at $X^{I*} = 1$ in Figure A1 should be compared to the mean estimate at $X^{I*} = 0.75$ in Figure A2 (in which case this is the lower standardization level and the average is 1); the latter estimate is in fact closer to zero.

[23] In the standard marginal effect – $\psi_k\phi(S\psi)$, from Equation 15 – nearer the center of the distribution the larger estimate of ψ_k dominates the marginal effect, whereas nearer the tails the larger estimate of ψ_k lowers $\phi(S\psi)$ enough that the product $\psi_k\phi(S\psi)$ is closer to zero.