Bruce Levin, Debra Millenson and Mary Ellen Wynn. I retain responsibility for error.

ADDITIONAL REFERENCES

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Comment

Arthur S. Goldberger

I am grateful to Arthur Dempster for pointing out an error in my article, but perturbed by his campaign against econometricians. On balance, my perturbation exceeds my gratitude.

A bit of background. The most popular approach to the assessment of gender discrimination has been to run the direct regression

$$\hat{y} = \mathbf{b}'\mathbf{x} + az,$$

where y = salary, $\mathbf{x}$ = vector of measured covariates and z = gender (coded 1 for men, 0 for women). In this approach, the coefficient "a" (typically positive) is taken to be the measure of discriminatory behavior on the part of the employer. An obvious objection is that relevant covariates have been omitted: $\mathbf{x}$ may not capture all the productivity-related characteristics available to the employer. When those covariates are correlated with gender, there is a presumption that their omission biases the direct regression estimate.

Some critics of direct regression had gone on to suggest that the bias would be eliminated by using reverse regression, in particular by running the composite covariate $q = \mathbf{b}'\mathbf{x}$ upon y and z,

$$\hat{q} = cy + dz,$$

and taking $-d/c$ as the measure of discriminatory behavior. The rationale for this was rather vague, some mention of errors in variables being made.

To an econometrician, it seemed inappropriate to discuss estimation bias until the parameter of interest had been defined and imbedded in a coherent model. I first sought a model that would support direct regression, and yet allow for omitted variables in the statisticians's regression. I found it in Model A:

$$\text{(A1)} \qquad y = p + \alpha z, \quad p = \mathbf{x}'\boldsymbol{\beta} + w, \quad \mathbf{x} = \boldsymbol{\mu} z + \mathbf{u},$$

$$\text{(A2)} \qquad E(w \mid \mathbf{x}, z) = 0, \quad E(\mathbf{u} \mid z) = \mathbf{0}.$$

The parameter of interest is α. I wrote that p is the "latent variable which is best interpreted as the employer's assessment of productivity" and that w is "a gender-free disturbance. That disturbance represents the additional information available to the employer but not to the statistician."

In this model, I deduced that

$$\text{(A3)} \qquad E(y \mid \mathbf{x}, z) = \mathbf{x}'\boldsymbol{\beta} + \alpha z,$$

so that "direct regression gives an unbiased assessment of discrimination ($a = \alpha$) despite the fact that the measured variables do not exhaust the information used by the employer in assessing productivity." The key to the conclusion is the assumption that $E(w \mid \mathbf{x}, z) = 0$—the omitted variables are uncorrelated with gender after controlling for the measured variables. That is precisely why I introduced it.

Next I sought a model that would support reverse regression. Drawing on suggestions made by proponents of reverse regression, I found it in Model B:

$$\text{(B1)} \qquad y = p + \alpha z, \quad \mathbf{x} = \boldsymbol{\gamma} p + \boldsymbol{\varepsilon}, \quad p = \mu z + u,$$

$$\text{(B2)} \qquad E(\boldsymbol{\varepsilon} \mid p, z) = \mathbf{0}, \quad E(u \mid z) = 0.$$

I wrote that here "each observed qualification [element of $\mathbf{x}$] is merely an indicator of the employer's assessment [p] subject to a gender-free disturbance." The parameter of interest is again α.

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In this model I deduced that

$$\text{(B3)} \qquad E(\mathbf{x} \mid y, z) = \boldsymbol{\gamma} y - \boldsymbol{\gamma}\alpha z,$$

whence, for $q = \mathbf{b}'\mathbf{x}$,

$$\text{(B4)} \qquad E(q \mid y, z) = c^* y + d^* z,$$

where $c^* = \mathbf{b}'\boldsymbol{\gamma}$, $d^* = -(\mathbf{b}'\boldsymbol{\gamma})\alpha$. Because $-d^*/c^* = \alpha$, I concluded that "the model clearly supports the composite reverse regression as a device for assessing discrimination."

I observed that Model B implies a testable restriction on the multivariate regression in (B3), namely proportionality of coefficients. Because this was the only model in hand that supported reverse regression, I announced that passing the test was a scientific prerequisite to use of reverse regression.

Now suppose that we modify Model B by allowing the disturbance in $\mathbf{x} = \boldsymbol{\gamma} p + \boldsymbol{\varepsilon}$ to vary with gender,

$$\text{(B*1)} \qquad E(\boldsymbol{\varepsilon} \mid p, z) = \boldsymbol{\theta} z.$$

Then

$$\text{(B*2)} \qquad E(\mathbf{x} \mid y, z) = \boldsymbol{\gamma} y - (\boldsymbol{\gamma}\alpha + \boldsymbol{\theta}) z,$$

and

$$\text{(B*3)} \qquad E(q \mid y, z) = c^* y + d^{**} z,$$

where $c^* = \mathbf{b}'\boldsymbol{\gamma}$ as before, while $d^{**} = -(b'\boldsymbol{\gamma}\alpha + \mathbf{b}'\boldsymbol{\theta})$. In (B*2) the proportionality restriction is gone, and yet in (B*3) *provided that* $\mathbf{b}'\boldsymbol{\theta} = 0$, one has $-d^{**}/c^* = \alpha$, so the composite reverse regression will still be appropriate for estimating α. Although the condition $\mathbf{b}'\boldsymbol{\theta} = 0$ seems implausible to me (unless $\boldsymbol{\theta} = \mathbf{0}$), Dempster's point is well taken: passing the proportionality test is not a scientific prerequisite to use of reverse regression.

Thus, Dempster has "undermined" my arguments for direct regression and against reverse regression. "Both forms of regression," he admonishes, "are subject to bias that purely statistical methods are unable to correct." I think that I was there first:

> "The models developed above hardly exhaust the possibilities. It is easy enough to write down a general omitted-variable system in which the structural discrimination parameter is not identified. For such a system neither direct nor reverse regression will be appropriate." Goldberger (1984, page 314)

But enough about me. Dempster is out after bigger game, the community of econometricians. They apply the term causal model indiscriminately, they may not be able to distinguish real causes from things that are merely called causes, they tend to blame the model rather than the real world for nonidentifiability, they use opaque assumptions. Their main tradition is to specify sampling distributions crudely. They have another tradition: fair reward is determined by a mathematical formula with added random disturbances. They rest a huge literature on naive randomness assumptions, contaminating other social scientists as well: "much stochastic modeling in these disciplines is undermined by a dependence on fictitious chance mechanisms."

I am perturbed, but not impressed with this enumeration of sins. I don't recognize my professional colleagues from his report. Despite a purportedly extensive reading in economics and sociology, Dempster doesn't provide a single citation. A fair reading of the econometrics literature would show that its main tradition is that prior information is essential to inference. As Dempster now writes, "any statistical estimate . . . must be adjusted from sources of knowledge outside the statistician's data."

In his peroration, Dempster offers his expertise to the American legal system and to American policy makers. Will they be grateful to learn that they can "address real problems of discrimination without violating the theory of probability"? Probably as grateful as economists will be to learn from Dempster that "the employer engages in decision making under uncertainty."

A final thought: when statisticians have important messages for econometricians, they might address them to econometricians rather than to other statisticians.