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## **Comment** Dan A. Black

Decressin, Hill, McCue, and Stinson (DHMS henceforth) should be congratulated for a very good chapter. Moreover, the profession owes them a debt of gratitude for their efforts to create this intriguing data set. Economists are heavy users of data sets, but we seem to undervalue the effort and creativity associated with the creation of data. The authors of this chapter have done an immense amount of work creating an important data set that

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links employers and employees with administrative data on the fringe benefits offered by employers. In addition, their chapter provides some intriguing relationships between the performance of firms and the firms' decisions about whether to offer fringe benefits. In many ways, in the analysis of this chapter DHMS raise more questions than they answer. In this comment, I would like to focus on two important issues that jumped out at me when I read their chapter.

Using these data, the latter part of their chapter examines the association of fringe benefits and firm turnover, employment growth, productivity, and the likelihood the firm goes out of business. While the link between the association and causality is tenuous at best without a better model of the contracts between workers and their firms, these associations are suggestive of an important role of fringe benefits in the labor market. In their analysis, DHMS find that the provision of fringe benefits is associated with lower turnover, higher productivity growth, and a higher probability of surviving. Of course, these are good outcomes for the firms, and a natural concern is that there is another omitted factor that reduces turnover, increases productivity growth, and increases the probability of surviving, as well as allowing firms to offer fringe benefits. Obviously, an important next step would be to develop a model of the provision of fringe benefits and find some variables that would not directly affect the outcomes of interest, but would affect the likelihood that a firm does indeed offer fringe benefits. With such exclusion restrictions, we could then see if the intriguing associations documented in this chapter are in fact causal.

In the rest of this comment, I want to focus on the problem of measurement error when one matches employer and employee data. Due to the nature of the matching process, there are a lot of false negatives: many workers who in fact may have fringe benefits are not matched. In table 13C.1, I reproduce a comparison that DHMS make between Bureau of Labor Statistics (BLS) estimates of pension and health insurance with similar estimates from their data. One of the more striking features in DHMS data is the low incidence they find of health insurance. While their pension coverage appears to be measured very well, their measures of health insurance coverage are poor. Because of this measurement error, DHMS aggregate their measure into a single variable: whether the firm offers any fringe benefits.

Usually, measurement error in a binary variable is a problem without instruments: ordinary least squares (OLS) estimates are attenuated.[1] Because of the peculiar form of the measurement error, however, the situation is much more promising. Because we assured that there are virtually no

1. Instrumental variable (IV) estimates are biased away from zero; see Black, Berger, and Scott (2000). Black, Berger, and Scott, as well as Kane, Rouse, and Staiger (1999) discuss how identification may be achieved with two measures of the binary variable; Frazis and Loewenstein (2003) extend the analysis to any instruments. See Bound, Brown, and Mathiowetz (2001) for an excellent discussion.

**Table 13C.1** **Comparison of pension and health insurance coverage rates**

| | Pension coverage | Health insurance coverage |
|---|---|---|
| BLS | 56% | 86% |
| DHMS | 61% | 34% |

*Source:* Decressin, Hill, McCue, and Stinson (chapter 13, this volume).

false positive matches—workers incorrectly matched to firms—we may be assured that virtually all of the matched workers do indeed have the pension and health insurance ascribed to them. This allows the research to measure correctly one of two moment conditions, which allows for identification under much weaker assumptions. To see why, consider the impact of benefit coverage on an outcome, $y_i$. We wish to estimate:

$$\tau(X) = E(y_i \mid X, C = 1) - E(y_i \mid X, C = 0) \tag{1}$$

where $\tau(X)$ are the parameters of interest. We compare the expectation of our outcome when a worker is covered ($C = 1$) to the expectation of our outcome when a worker is not covered ($C = 0$), conditional on the realization of some covariates ($X$). When there are no false positives, we may estimate $E(y_i \mid X, C = 1)$ from the noisy measure of coverage $E(y_i \mid X, \tilde{C} = 1)$ if we are willing to assume that $E(y_i \mid X, \tilde{C} = 1) = E(y_i \mid X, C = 1)$ so that the mismeasurement is uncorrelated with $y_i$.

The problem is that $E(y_i \mid X, \tilde{C} = 0)$ is contaminated with false negatives. Thus, we have

$$E(y_i \mid X, \tilde{C} = 0) = \theta(X)E(y_i \mid X, C = 1, \tilde{C} = 0) + [1 - \theta(X)]E(y_i \mid X, C = 0). \tag{2}$$

Fortunately, we have an estimate of $E(y_i \mid X, C = 1, \tilde{C} = 0)$, which should just be $E(y_i \mid X, \tilde{C} = 1)$ under the assumption that $E(y_i \mid X, \tilde{C} = 1) = E(y_i \mid X, C = 1)$. Thus, equation (2) has only two unknown parameters: $\theta(X)$ and $E(y_i \mid X, C = 0)$. If we may use alternative data sets that allow us to estimate, conditional on $X$, the probability of benefit coverage, then we may obtain estimates of $\theta(X)$. This would then allow the researcher to recover estimates of $E(y_i \mid X, C = 0)$ directly from equation (2). Thus, with auxiliary data on the probability of coverage, it would be possible to identify the parameters of interest nonparametrically if we are, of course, willing to make the assumption that $E(y_i \mid X, \tilde{C} = 1) = E(y_i \mid X, C = 1)$.

This is potentially an important result because, as we see the growth of more and more data of the type that DHMS use (matched administrative records), this is likely to become a more common form of measurement error. If the matching problem is severe (as it is in the DHMS measure of the coverage of health insurance), we may at least take some comfort from the fact that attenuation bias may be corrected through the use of auxiliary data.

This result also has some important implications for how data matching should be performed. Often researchers are confronted with tough decisions regarding whether a particular pair is correctly matched. This analysis suggests that researchers should be quite demanding on the data before agreeing that there is a true match. This will insure that we have the necessary one-sided error that allows us to recover the parameters of interest. Of course, if the researcher wants to perform a probabilistic match for observations that may be correctly matched, an indicator variable that documents perfect matches will also allow researchers to recover the parameters of interest, as well as assess the accuracy of their probabilistic matches.

Of course, the identification is achieved only if we are able to maintain the assumption that $E(y_i \mid X, \tilde{C} = 1) = E(y_i \mid X, C = 1)$. Assessing the validity of this assumption is, of course, quite difficult. As DHMS document, the match rate varies systematically with firm size, and the variation is larger than what we would expect if the variation were solely due to differences in the rates of provision of fringe benefits. We know, of course, that there are systematic differences in outcomes by firm size so it would be prudent to include firm size in the vector of covariates. One fears, however, that conditional on coverage, unmatched firms are simply inferior at filling out paperwork (hence the lack of a match), but these firms are also inferior at running their businesses and hence have worse outcomes on the average.

Unfortunately, there is little empirical content to the assumption that $E(y_i \mid X, \tilde{C} = 1) = E(y_i \mid X, C = 1)$. If means are sufficiently disparate, it is possible that the observed mean, $E(y_i \mid X, \tilde{C} = 0)$, will not allow the imposition of $E(y_i \mid X, \tilde{C} = 1) = E(y_i \mid X, C = 1)$, but this is quite unlikely unless the means $E(y_i \mid X, C = 1, \tilde{C} = 0)$ and $E(y_i \mid X, C = 0)$ are greatly different than the $E(y_i \mid X, \tilde{C} = 1)$. In most applications, however, one suspects that the means are not that greatly different and so the assumption will pass this weak test.

At some level, the lack of empirical content of the identification assumption is troublesome. Clearly, one would prefer a strong test of the identification assumption before we make heavy use of it. Yet most empirical papers make another, even stronger assumption: there is no measurement error in the data used. More importantly, I think this identification strategy shows that there may be immense value to matching employer and employee data even when the match rates may be quite low. As long as researchers can use the resulting data to estimate accurately one of the two moment conditions, the use of auxiliary data may allow researchers to recover the parameters of interest despite very high levels of measurement error.

Again, DHMS should be congratulated for a very good chapter and their immense efforts in the creation of this extremely interesting data set.

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