In addition to wrongly omitted variables, the model may involve erroneously included variables. In theory, one such mistake should not be fatal if the model is otherwise correctly specified. The erroneously included variable should have a nearly zero coefficient. However, a mistake of this kind does impair the efficiency with which the model's coefficients are estimated. Hence, the investigator should not try to get by on the strategy of "including everything" on an initial run of his model. This strategy, pursued relentlessly, leads to underidentification, as we have seen in Chapter 6 (page 87).

Other issues properly subsumed under specification error include (*1*) tests of overidentifying restrictions (Chapter 3, pages 46–50; Chapter 7, pages 98–99); (*2*) the validity of the specification of linear and additive functional form, a matter that receives considerable attention in most statistical presentations of linear models; and (*3*) the acceptability of the homoskedasticity assumption in regard to disturbances, likewise treated in some statistics texts. The neglect of these last two topics in our sketch of this subject should not be mistaken for a judgement that they are unimportant. On the contrary, they are so important that they must be squarely faced throughout a project in constructing a model, but especially when considering the initial specification of the model. Statistical tests of linearity and homoskedasticity may be of use, but detailed inspection of the data aided by graphic plots is perhaps even more useful.

Exercise. *On page 17 we suggested that one could pose a countermodel to Model* II′ *as a means of discussing possible specification error in that model. On page 18, we made a similar suggestion regarding Model* III. *If you have not already done so, show how this might be done in each instance.*

FURTHER READING

The text by Rao and Miller (1971) is unusual in regard to the amount of attention given to matters relevant to this topic and also in that it is accessible to the reader not familiar with matrix algebra.

9

Measurement Error, Unobserved Variables

From a formal point of view, the topic of error in (measurement of) variables is much the same thing as that of unobserved variables. All observation is fallible, no matter how refined the measuring instrument and no matter how careful the procedure of applying it. In a strict sense, therefore, we never measure exactly the true variables discussed in our theories. In this same strict sense, all (true) variables are "unobserved."

It may happen that errors of measurement are negligible, relative to the magnitudes of the disturbances in our equations or the standard errors of sampling in our estimates of coefficients. Of course, an investigator will not blithely assume this is so, but will make every effort to assess his measurement errors and their impact on his results. If the verdict is reassuring (perhaps because the other threats to valid inference are so uncomfortably large!) he may proceed, for the moment, to treat his variables as error-free, as we have been doing implicitly throughout this book.

A second possibility is that measurement error is appreciable but "well-behaved" and, perhaps, estimable. That is, a relatively simple and manageable specification of the behavior of the errors is acceptable. Either the errors can be shown not to impair seriously the results obtained, or their parameters can be estimated and corrections for their distortions can be introduced.

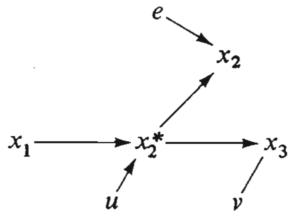
A third possibility—by all odds the most realistic one, in sociological investigations—is that our measuring instrument does not measure "cleanly" what we would like it to measure, but also reflects both random and systematic deviations from the "true" variable (the one discussed in the theory) which are both too large to neglect and too complicated ("messy") to manage on any elementary model of their behavior.

A counsel of perfection would be to work out a theory of errors and develop a model for the behavior of errors *pari passu* with the pursuit of the substantive objectives of every investigation. A more realistic kind of advice is predicated on the assumption that any single inquiry is just an incident in an historical stream of research. At a given moment, the investigator does all that he can to reduce errors of measurement, to estimate those that remain, and to accommodate his models and statistical procedures to the facts of measurement error, as best he can understand them on the basis of his own studies and the literature of his field. Over the long run, the cure for the more disruptive kinds of errors can only be improved techniques of measurement. But, as errors are tamed, it becomes possible to devise realistic and powerful theories of error, and to build directly into our models the requisite assumptions about errors. A mature science, with respect to the matter of errors in variables, is not one that measures its variables without error, for this is impossible. It is, rather, a science which properly manages its errors, controlling their magnitudes and correctly calculating their implications for substantive conclusions.

This is a very large topic. Indeed, almost the whole of psychometrics can be seen as a frontal assault on the problem of errors in variables. In sociology, substantial efforts to estimate error have been made in some survey research organizations. Moreover, sociologists have long been sensitized to the "indicator problem"—the often loose epistemic linkage between the variable specified in our theory and even the most plausible of the available empirical measures of it. Many chapters in the symposium edited by Goldberger and Duncan (1973) are concerned in one way or another with problems raised by models in which there are multiple indicators of unobserved variables and/or models in which measurement error complicates the estimation of structural

coefficients. The symposium opens up several new, but difficult, approaches to these problems. We shall be content here only to suggest a few of the sorts of problems that arise as soon as one proposes to take explicit account of errors in variables.

Let us begin with this example.



The primary causal model here is the simple causal chain, $x_1 \rightarrow x_2^* \rightarrow x_3$, already studied in Chapter 2 (with different notation). But now, one of the variables (x_2^*) is not directly observed. Instead, its observed counterpart (x_2) is contaminated with an error (e). Hence the complete model comprises three equations, one of which describes the fallible measurement of x_2^*, while the other two represent the causal model as such:

$$x_2 = x_2^* + e$$

$$x_2^* = b_{21}x_1 + u$$

$$x_3 = b_{32}x_2^* + v$$

To make the example easy, we require the error of measurement to be well-behaved. (Since we are talking about the properties of the model and not about what the world is really like, it will be recognized that this requirement is itself problematic in any realistic context.) Specifically, we assume that e is uncorrelated with x_2^* and also with both x_1 and x_3:

$$E(x_2^*e) = E(x_1 e) = E(x_3 e) = 0.$$

That is, roughly speaking, the error is "random" and not "systematic." Moreover, $E(e) = 0$, just as for all other variables in the model. We employ the usual specification in regard to disturbances:

$$E(x_1 u) = E(x_2^* v) = E(x_1 v) = 0.$$

In consequence of these specifications, we find that $E(eu) = E(ev) = E(uv) = 0$.

Exercise. *Verify that* $E(eu) = E(ev) = E(uv) = 0$.

We find, further, upon squaring x_2 and taking expectations, that

$$\sigma_{22} = \sigma_{22}^* + \sigma_{ee}$$

using the symbol σ_{22}^* for $E[(x_2^*)^2]$. That is, the total variance of the fallible measurement (x_2) is the sum of the variances of the true but unobserved variable we are trying to measure (x_2^*) and the variance of our measurement errors (e).

Another useful and perhaps surprising result is that the covariances are not affected by the error in measurement. That is, $E(x_1 x_2) = E(x_1 x_2^*)$ and $E(x_2 x_3) = E(x_2^* x_3)$.

Exercise. *Verify this.*

Thus, when we multiply through the x_2^*-equation by x_1 we obtain

$$\sigma_{12} = b_{21}\sigma_{11}$$

so that

$$b_{21} = \frac{\sigma_{12}}{\sigma_{11}}$$

We see that the OLS estimator m_{12}/m_{11} estimates b_{21} without bias. In this segment of the model, the well-behaved measurement error has a relatively benign effect. The effect is not wholly benign, however. We must reckon with its influence on the precision of our estimator. Substituting the x_2^*-equation into the equation relating the observed to the true value, we obtain

$$x_2 = b_{21}x_1 + u + e$$

Hence the total variance of x_2 is

$$\sigma_{22} = b_{21}^2\sigma_{11} + \sigma_{uu} + \sigma_{ee}$$

(obtained by squaring both sides and taking advantage of the vanishing covariances noted earlier). Regarding the "explained" variance,

$b_{21}^2\sigma_{11}$, as fixed, we note that the "unexplained" variance will increase as σ_{ee} increases. Thus, greater error variance means a larger standard error for our OLS estimate of b_{21}. This effect can, of course, be offset by drawing a larger sample.

So much for the good news. Now for the bad news. We substitute for x_2^* in the x_3-equation the expression $x_2^* = x_2 - e$:

$$x_3 = b_{32}x_2 + v - b_{32}e$$

or

$$x_3 = b_{32}x_2 + v'$$

where $v' = v - b_{32}e$.

We are tempted to regress x_3 on x_2, that is, to use the OLS estimator of b_{32}, m_{23}/m_{22}. But, upon multiplying the x_3-equation through by x_2 we obtain

$$\begin{aligned}\sigma_{23} &= b_{32}\sigma_{22} + E(x_2 v') \\ &= b_{32}\sigma_{22} - b_{32}E(x_2 e) \\ &= b_{32}(\sigma_{22} - \sigma_{ee})\end{aligned}$$

whence

$$b_{32} = \frac{\sigma_{23}}{\sigma_{22} - \sigma_{ee}}$$

Therefore, our OLS procedure estimates *not* b_{32} but rather σ_{23}/σ_{22}, or

$$b_{32}\left(1 - \frac{\sigma_{ee}}{\sigma_{22}}\right) = b_{32}\frac{\sigma_{22}^*}{\sigma_{22}^* + \sigma_{ee}}$$

Hence, the greater the variance in the errors (that is, the lower the precision of our measurements) the greater the downward bias in our OLS estimate of b_{32}. (The bias actually is downward only if b_{32} is positive. The bias is toward zero, regardless of the sign of b_{32}.)

To generalize: Error in the dependent variable, if "well behaved," does not bias the OLS estimate. But error in the independent variable, even though "well behaved," imparts a downward bias to the OLS estimate. The kinship between "measurement error" and "specification error" is brought out by this last result. Our difficulty with OLS arose in writing the x_3-equation in terms of the observed variable x_2 and the disturbance v', because, in this formulation, the explanatory variable is not uncorrelated with the disturbance, that is, $E(x_2 v') \neq 0$.

But let us return to the good news. Adopting a different approach to the estimation of b_{32}, we multiply through the x_3-equation

$$x_3 = b_{32} x_2 + v'$$

by x_1 (rather than x_2) to obtain

$$\sigma_{13} = b_{32} \sigma_{12}$$

inasmuch as $E(x_1 v') = E(x_1 v) - b_{32} E(x_1 e) = 0$. Finding that $b_{32} = \sigma_{13}/\sigma_{12}$, we are led to propose instead of the OLS estimator of b_{32} (to wit, m_{23}/m_{22}) the IV estimator, m_{13}/m_{12}, using x_1 as an instrument for the contaminated x_2. But from the original formulation of the x_3-equation

$$x_3 = b_{32} x_2^* + v$$

we find (as previously noted) that

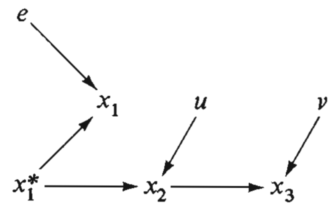
$$\sigma_{23} = b_{32} \sigma_{22}^* = b_{32}(\sigma_{22} - \sigma_{ee})$$

recalling that $E(x_2 x_3) = E(x_2^* x_3)$. Now, we have the IV estimate $\hat{b}_{32} = m_{13}/m_{12}$ and we can estimate σ_{23} and σ_{22} directly from our sample data. From these, we derive an estimate of the error variance,

$$\hat{\sigma}_{ee} = \frac{\hat{b}_{32} \hat{\sigma}_{22} - \hat{\sigma}_{23}}{\hat{b}_{32}}$$

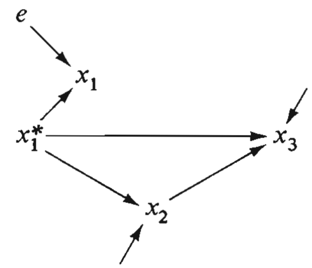
It is, therefore, a remarkable property of this model that one can not only secure unbiased estimates of its coefficients, despite the presence of measurement error, but actually estimate the crucial parameter of the measurement process itself. Clearly, this remarkable property is due to the fact that the three-variable, simple causal chain model is overidentified to begin with. Thus, we infer the principle that overidentification provides a possible weapon in an attack on measurement error. But the weapon is no better than the theory of error that provides its ammunition. (Our model, it should be clear, rests on a very strong theory of error.) Moreover, the overidentifying restriction(s) must be in the "right" place, relative to the location of the measurement error, in order for the weapon to work at all. To appreciate this last point, carry out this exercise.

Exercise. *Suppose the fallible variable is x_1 in a simple causal chain model, and that the error is "well-behaved" as before:*

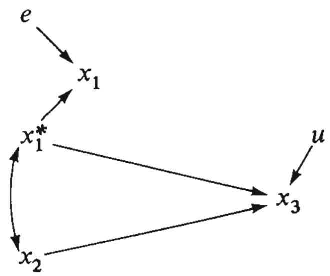


Show that an unbiased estimate of b_{21} cannot be obtained from sample moments, whether by OLS *or* IV, *despite the availability of an overidentifying restriction on the model.*

The exercise prepares the ground for another example, a model in which we dispense with the overidentifying restriction:



We already know (from the exercise) the nature of the difficulty with the x_2-equation. Let us, therefore, focus on the x_3-equation or, equivalently, the model



Continuing with the now-familiar specifications on the error and disturbance terms, we have $E(x_1^* e) = E(x_2 e) = E(x_3 e) = E(x_1^* u) = E(x_2 u) = 0$, from which it follows that $E(x_1 u) = E(eu) = 0$. The equations of the model are

$$x_1 = x_1^* + e$$

$$x_3 = b_{31} x_1^* + b_{32} x_2 + u$$

We note that $\sigma_{11} = \sigma_{11}^* + \sigma_{ee}$, and that $E(x_1^* x_2) = \sigma_{12}$ and $E(x_1^* x_3) = \sigma_{13}$. We find (verify this) that

$$\sigma_{13} = b_{31}(\sigma_{11} - \sigma_{ee}) + b_{32}\sigma_{12}$$

$$\sigma_{23} = b_{31}\sigma_{12} + b_{32}\sigma_{22}$$

Hence,

$$b_{31} = \frac{\sigma_{13}\sigma_{22} - \sigma_{12}\sigma_{23}}{\sigma_{11}\sigma_{22} - \sigma_{12}^2 - \sigma_{ee}\sigma_{22}}$$

$$b_{32} = \frac{\sigma_{11}\sigma_{23} - \sigma_{12}\sigma_{13} - \sigma_{ee}\sigma_{23}}{\sigma_{11}\sigma_{22} - \sigma_{12}^2 - \sigma_{ee}\sigma_{22}}$$

From the presence of terms involving σ_{ee} in the denominator of b_{31} and both the numerator and denominator of b_{32}, we infer that the OLS estimates obtained in regressing x_3 on x_2 and x_1 will not be satisfactory in regard to either structural coefficient. The formula (above) for b_{31} takes the form $K_1/(K_2 - K_3)$, where K_1 is the numerator, $K_2 = \sigma_{11}\sigma_{22} - \sigma_{12}^2$, and $K_3 = \sigma_{ee}\sigma_{22}$. Both K_2 and K_3 are intrinsically positive, although K_1 may be either positive or negative. The OLS procedure estimates not b_{31} but K_1/K_2. Therefore, we conclude that there is a downward bias in the absolute value of the OLS estimate. To see the bias in the OLS estimate of b_{32}, we derive another formula for that coefficient:

$$b_{32} = \frac{\sigma_{11}\sigma_{23} - \sigma_{12}\sigma_{13}}{\sigma_{11}\sigma_{22} - \sigma_{12}^2} - \frac{b_{31}\sigma_{12}\sigma_{ee}}{\sigma_{11}\sigma_{22} - \sigma_{12}^2}$$

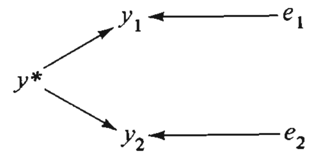
Exercise. *Verify this.*

By OLS, we estimate only the first term, or

$$b_{32} + \frac{b_{31}\sigma_{12}\sigma_{ee}}{\sigma_{11}\sigma_{22} - \sigma_{12}^2}$$

The sign of the bias depends only on the signs of b_{31} and σ_{12}. If both of these are positive, we overestimate b_{32} (while underestimating b_{31}).

This situation is hopeless, unless we can secure auxiliary information about σ_{ee}. Various experimental designs for estimating σ_{ee} are available. The choice of an appropriate one depends (among other things) on the nature of the variables, and the literature of a well-developed substantive field will usually include studies specifically designed to estimate measurement error. Suppose that the true variable is considered to be a relatively permanent attribute for each member of the population and suppose that we can make a (fallible) measurement of it more than once without any carry over of error from one occasion to another. (This assumption clearly is implausible if memory or learning is involved or if the very act of measurement releases causal forces that otherwise would not come into the picture.) If the true variable is y^*, a model for the experiment of carrying out two measurements on a sample of members of the population is



or, more explicitly, $y_1 = y^* + e_1$ and $y_2 = y^* + e_2$.

On each occasion, according to the model, the error is uncorrelated with the true value, so that $E(y^* e_1) = E(y^* e_2) = 0$. By ruling out "carry over" we mean, more precisely, to specify $E(e_1 e_2) = 0$. The supposition of no systematic error is conveyed by $E(e_1) = E(e_2) = 0$. If the experiment is well executed, so that each measurement simulates faithfully the conditions encountered in a substantive investigation, then the variance of measurement errors should be the same: $E(e_1^2) = E(e_2^2)$. This is one assumption of the experiment that can actually be tested, provided, of course, that one accepts the prior assumption that y^* itself is unchanged between occasions. Our model implies that

$$E(y_1^2) = \sigma_{yy}^* + E(e_1^2)$$

and

$$E(y_2^2) = \sigma_{yy}^* + E(e_2^2)$$

Hence, if $E(e_1^2) = E(e_2^2)$, $E(y_1^2) = E(y_2^2)$. Statistical procedures for testing for homogeneity of variances are available. If the data are consistent with the hypothesis of homogeneity, that is, the null hypothesis $E(y_1^2) = E(y_2^2)$, we may derive an estimate of the error variance from the model by noting, first, that the model implies

$$y_1 - y_2 = e_1 - e_2$$

whence

$$E(y_1^2) + E(y_2^2) - 2E(y_1 y_2) = E(e_1^2) + E(e_2^2)$$

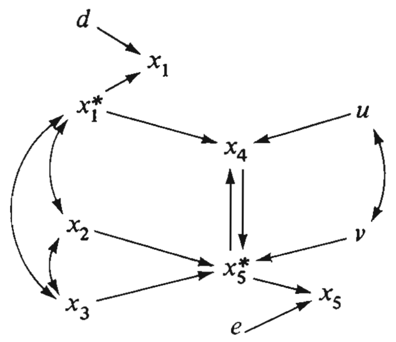
since $E(e_1 e_2) = 0$ but $E(y_1 y_2) \neq 0$. But if $E(y_1^2) = E(y_2^2) = \sigma_{yy}$ and $E(e_1^2) = E(e_2^2) = \sigma_{ee}$ (one will have decided already to accept this assumption before proceeding), we have

$$\sigma_{ee} = \sigma_{yy} - \sigma_{12}$$

The variance σ_{yy} is estimated upon pooling m_{11} and m_{22}, and the covariance σ_{12} is estimated from m_{12}.

Given the estimate of σ_{ee}, we may return to the formulas already given for b_{31} and b_{32} and see at once that this is the one item of information needed to "correct" the OLS estimates for their biases.

In our final example of a model taking account of "well-behaved" errors of measurement, we raise the question of whether principles observed to operate in recursive models carry over to the nonrecursive case. The model, a slightly modified version of one discussed by Goldberger (1972), is represented by the following path diagram:



The structural equations are

$$x_4 = b_{41} x_1^* + b_{45} x_5^* + u$$

$$x_5^* = b_{52} x_2 + b_{53} x_3 + b_{54} x_4 + v$$

The usual specification on the disturbance terms is adopted:

$$E(x_1^* u) = E(x_1^* v) = E(x_2 u) = E(x_2 v) = E(x_3 u) = E(x_3 v) = 0.$$

The equations describing the behavior of measurement errors are

$$x_1 = x_1^* + d$$

$$x_5 = x_5^* + e$$

To make the errors "well-behaved," we specify

$$E(x_1^* d) = E(x_2 d) = E(x_3 d) = E(x_4 d) = E(x_5^* d)$$
$$= E(x_1^* e) = E(x_2 e) = E(x_3 e) = E(x_4 e) = E(x_5^* e) = 0.$$

We also have to make explicit the specification, implied by the diagram, that $E(de) = 0$. (The assumption that errors in two variables are uncorrelated will often be a problematic one in a realistic application.) The specifications already stated suffice to guarantee that measurement errors are uncorrelated with structural disturbances.

Exercise. *Show that* $E(du) = E(dv) = E(eu) = E(ev) = 0$.

But note that nothing that has been said would imply that the disturbances are uncorrelated; we must suppose, in general, that $E(uv) \neq 0$.

We recall from an earlier example that measurement error in an exogenous variable causes trouble. But in that example the method of estimation under consideration was OLS. Here, we already know that OLS is not suitable, so we have to reopen the question of whether estimates will be biased by the error in measuring x_1^*. We also recall earlier examples in which measurement error in the dependent variable did not bias OLS estimates. But now, x_5^* is not only a dependent variable, but is simultaneously a causal variable (with respect to x_4). Again, we cannot infer without special study of the issue that the principle developed earlier will carry over to the nonrecursive case.

Let us first examine the x_5^*-equation. We can immediately restate it as an x_5-equation by substituting its right-hand side for x_5^* in $x_5 = x_5^* + e$. This yields

$$x_5 = b_{52}x_2 + b_{53}x_3 + b_{54}x_4 + v'$$

where $v' = e + v$. We see that this equation is just identified. There are three coefficients to estimate, and three (observed) instrumental variables are available, to wit, x_1, x_2, and x_3. Their eligibility as instrumental variables follows from the fact that each is uncorrelated with the new disturbance, v', since each is uncorrelated with both the original disturbance (v) and the measurement error (e).

Query: How do we know, in particular, that $E(x_1 v) = 0$ and $E(x_1 e) = 0$, since these are not explicit specifications of the model?

We may therefore proceed to IV estimation of the rewritten x_5-equation. Writing out the covariances in the usual fashion and substituting corresponding sample moments for them, we find

$$m_{15} = m_{12}\hat{b}_{52} + m_{13}\hat{b}_{53} + m_{14}\hat{b}_{54}$$
$$m_{25} = m_{22}\hat{b}_{52} + m_{23}\hat{b}_{53} + m_{24}\hat{b}_{54}$$
$$m_{35} = m_{23}\hat{b}_{52} + m_{33}\hat{b}_{53} + m_{34}\hat{b}_{54}$$

so that we may solve for the $\hat{b}$'s by any convenient algorithm.

We turn our attention to the x_4-equation and note that we can eliminate the unobserved variables from it by the substitutions, $x_1^* = x_1 - d$ and $x_5^* = x_5 - e$, to obtain

$$x_4 = b_{41}x_1 + b_{45}x_5 + u'$$

where $u' = u - b_{41}d - b_{45}e$. We see that neither x_1 nor x_5 can serve as an instrumental variable, since $E(x_1 u') \neq 0$ and $E(x_5 u') \neq 0$.

Exercise. *Verify these results.*

However, since $E(x_2 u') = E(x_3 u') = 0$, we have two instrumental variables available. Since there are only two structural coefficients to estimate, the rewritten x_4-equation is just identified.

Exercise. *Verify that* $E(x_2 u') = E(x_3 u') = 0$.

IV estimates are the solutions of the following pair of equations:

$$m_{24} = m_{12}\hat{b}_{41} + m_{25}\hat{b}_{45}$$
$$m_{34} = m_{13}\hat{b}_{41} + m_{35}\hat{b}_{45}$$

To consolidate our results: Well-behaved measurement error in an endogenous variable does not render the IV method (or the two-stage regression procedure mentioned in the next chapter, where there are "too many" instrumental variables) unsuitable for estimating an equation in a nonrecursive model. (We note, without further discussion, that it does inflate standard errors of estimated coefficients, however.) Moreover, well-behaved measurement error in an exogenous variable does not rule out estimation by use of instrumental variables provided that the original structural equation is overidentified. If there are enough predetermined variables in the model to provide a sufficient number of instrumental variables (at least as many as the number of coefficients to be estimated), we may set up estimating equations, on the IV principle, whose solution will yield the desired estimates.

Two noteworthy features of this example may be mentioned. First, it is not sufficient for the model to have an overidentified equation "somewhere." The overidentifying restriction(s) must be in the "right place." In a complicated model, it may require extensive analysis to be sure whether this holds true. Second, it is of interest that, although x_1 was eligible as an instrument in estimating the x_5-equation, it was no longer so for the x_4-equation. Again, careful analysis is required to take advantage of such subtle properties of the model.

Finally, we note that observations on the five variables in this model disclose nothing about the magnitude of the errors in x_5. Without additional evidence, we cannot estimate σ_{ee}. For x_1, on the contrary, the model itself provides a method of estimating the error variance. Recall the "solved-out" version of the x_4-equation:

$$x_4 = b_{41}x_1 + b_{45}x_5 + u - b_{41}d - b_{45}e$$

We multiply through by x_1 and take expectations:

$$\sigma_{14} = b_{41}\sigma_{11} + b_{45}\sigma_{15} - b_{41}\sigma_{dd}$$

Exercise. *You should verify that* $E(x_1 u) = E(x_1 e) = 0$ *and* $E(x_1 d) = \sigma_{dd}$, *since these facts have not hitherto been made explicit.*

We already have estimates of the b's, and the σ's can be estimated directly from the sample. After substituting the estimates for the parameters in this equation, we may solve for $\hat{\sigma}_{dd}$.

Exercise. *Show that the same approach will not enable us to estimate* σ_{ee}.

Our illustrations in this chapter have exemplified two broad strategies for coping with measurement error, if its location gives rise to difficulties in estimation:

(*1*) Imbed a model of measurement error in a substantive model that is otherwise overidentified and use the overidentifying restriction(s) as a means of estimating error variance (or covariance, if that, too, is present) and estimating structural coefficients free of bias due to measurement error.

(*2*) Use a model of measurement error to conduct an auxiliary investigation designed to estimate error variance(s) with which to "correct" the estimates of structural coefficients.

The two strategies are not mutually exclusive, but may be used in tandem. Variants of each are conceivable. If, in an auxiliary experiment, one can actually measure $y^* = y_1$ directly (that is, with negligible error) at great pains and expense and at the same time obtain y_2 by the standard procedure, then σ_{ee} is given directly by $E(y_2^2) - E(y_1^2)$ and both these quantities are estimated from the experimental data. (Note that a minimal test of the model is that $\hat{\sigma}_{22} > \hat{\sigma}_{11}$.)

A variant of the first strategy arises in connection with multiple indicators of an unobserved variable, each of which is not only fallible (perhaps highly so) but also potentially subject to systematic distortions. Such a situation demands a very subtle but nonetheless powerful theory concerning sources of error in measurement. Standard multivariate procedures for coping with a plethora of indicators (factor analysis, principal component analysis, canonical correlation, for example) are available, but they may not always prove satisfactory for complicated sociological problems. In the next chapter we try to suggest the character of the complications.

Another especially ugly complication is that many sociological variables can take on only a small number of discrete values and are

subject to hard and fast floor and/or ceiling effects. Suppose the true variable is the number of rooms in an apartment or house. The minimum is one and the maximum, though not well defined, may be, effectively, seven or eight. There can be no negative error if $y^* = 1$ and (almost) no positive error if $y^* = 8$. Under such circumstances to assume that $E(y^*e) < 0$ may be more plausible than to specify $E(y^*e) = 0$. Situations like this—no doubt there are numerous significantly different variants thereof—require more systematic investigation than they have received. We are only beginning to realize that measurement problems require theories quite as powerful and procedures just as well controlled as those needed to execute a study not fatally flawed by sampling error and specification error.

FURTHER READING

Although the psychometric literature contains much material on measurement error, it is less useful than it might be for present purposes, since the approach is usually via correlation and standardized variables. See Walker and Lev (1953, Chap. 12) for an introduction to this literature. In the econometric literature, Wonnacott and Wonnacott (1970, Chap. 7) is especially instructive in that error in the independent variable is considered in the same context as other sources of correlation between regressor and disturbance. Two highly recommended papers presenting models involving measurement error are D. E. Wiley and J. A. Wiley (Chap. 21 in Blalock, 1971) and D. E. Wiley (Chap. 4 in Goldberger and Duncan, 1973).