

CHAPTER 18

Measurement Error in Criminal Justice Data

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INTRODUCTION

While accurate data are critical in understanding crime and assessing criminal justice policy, data on crime and illicit activities are invariably measured with error. Measurement errors occur to some degree in nearly all datasets, but are arguably more severe in surveys of illicit activities. Some individuals may be reluctant to admit that they engage in or have been victims of criminal behaviors, whereas others may brag about and exaggerate accounts of illicit activities. Administrative data on arrests or reported crimes are likewise susceptible to systematic recording errors and misreporting. The utility of data on illicit behavior is reduced when variables are measured with error.

In this chapter, we discuss the implications of measurement error when drawing inferences on crime and justice policy. We take as given that even the best-designed surveys of illicit activities are apt to suffer extensive and systematic data errors.¹ In light of this problem, we review the consequences of measurement error for identification and inference, and document key issues that should be addressed when using potentially mismeasured data.

We begin, in section “Background and Evidence,” with a brief review of several important measurement problems in data on crime and illicit behavior. We consider three related, but conceptually different, forms of data error: response error, proxy error, and imputation error. Response errors arise when variables of interest are observed, but possibly reported with error. This type of error is thought to be pervasive in self-report surveys on illicit activities, but can also be problematic in administrative data, such as the Uniform Crime Reports. Proxy errors arise when unobserved variables are replaced by related variables. An example is the use of the fraction of suicides committed with a firearm as a proxy measure for the rate of firearm ownership (Azrael et al. 2001). Finally, imputation errors arise when missing data are replaced

¹ An extensive literature attempts to document the validity and reliability of criminal justice data (Lynch and Addington 2007; Mosher et al. 2002). In this chapter, we make no attempt to fully summarize this literature and offer no specific suggestions for modifying surveys to improve the quality of collected data.

by imputed values. A prominent example is the practice of imputing missing observations in the Uniform Crime Reports.

In sections “The Convolution Model” and “The Mixture Model,” we formalize a number of statistical models to illustrate the impact of measurement error on inference. Repeatedly, we observe that data errors lead to fundamental identification problems, and therefore have important consequences when drawing informative inferences.

Section “The Convolution Model” concentrates on the case where mismeasured variables follow a convolution generating process, such that errors are approximated by additive unobserved random terms. Using a bivariate mean regression model, we first review the implications of the classical errors-in-variable model, where measurement errors are assumed to be mean zero and exogenous. In this classical model, the ordinary least squares estimator is downwardly inconsistent, and without additional data or assumptions, the mean regression can only be partially identified. Though a useful starting point, we argue that classical assumptions are frequently inappropriate for the study of crime and justice data, where errors are likely to be systematic and related to activities and policies of interest. When classical model assumptions are relaxed, the asymptotic properties of the ordinary least squares estimator cannot be easily characterized and, without additional data or assumptions, the mean regression is not identified.

Section “The Mixture Model” argues that for crime data, where outcomes are often discrete and only some observations are in error, a mixing model may be an appropriate framework for thinking about the effects of measurement error. Combined with fairly weak assumptions about the degree and nature of data errors, we show that the mixing model can be used to partially identify parameters of interest. Under weak assumptions, however, we observe that even small amounts of measurement error can lead to high degrees of ambiguity. Stronger assumptions on the unobserved error process lead to sharper inferences, but may not be credible in many cases.

Section “Conclusion: The Law of Decreasing Credibility” concludes with a discussion on the law of decreasing credibility (Manski 2007). Agnostic models will often lead to indeterminate conclusions, while models imposing strong and potentially inaccurate assumptions lead to less credible inferences. Throughout this chapter, we suggest situations in which apparent middle grounds may be available. In a variety of cases, one can draw informative inferences without imposing untenable assumptions.

BACKGROUND AND EVIDENCE

The fundamental challenge of measuring crime is starkly illustrated by comparing different sources of crime data for the United States. The two most important sources are the uniform crime reports (UCR) and the National Crime Victimization Survey (NCVS). For almost eight decades, the Federal Bureau of Investigation (FBI) has compiled the UCR by collecting information on arrests and crimes known to the police in local and state jurisdictions throughout the country. The NCVS, which began in 1973, is a general population survey conducted by the Bureau of Justice Statistics; it is designed to discover the extent, nature, and consequences of criminal victimization in the United States.

Table 18.1 displays a time-series of the rates (per 1,000) of rape, robbery, aggravated assault, and property crime in the United States in 1990, 2000, and 2005, as reported in the official annual summaries of the UCR (US Department of Justice 2008) and NCVS (US

TABLE 18.1. UCR and NCVS annual crime rates (per 1,000) in the United States: 1990, 2000 and 2005^a

Crime survey	Rape		Robbery		Assault		Property crime	
	UCR	NCVS	UCR	NCVS	UCR	NCVS	UCR	NCVS
1990	0.4	1.7	2.6	5.7	4.2	9.8	50.7	348.9
2000	0.3	0.6	1.5	3.2	3.2	5.7	36.2	178.1
2005	0.3	0.5	1.4	2.6	2.9	4.3	34.3	154.0
% Change ^b	-22.6	-70.6	-45.1	-54.4	-31.2	-56.1	-32.4	-55.9

^aUCR estimates come from the US Department of Justice (2008), and NCVS estimates from the US Department of Justice (2006)

^bPercentage Change from 1990 to 2005

Department of Justice 2006). Comparison of these two surveys reveals major differences in estimated crime rates and crime trends (see, for example, Blumstein and Rosenfeld 2009; Lynch and Addington 2007; McDowall and Loftin 2007; NRC 2003, 2008). Crime rates estimated from the NCVS are always substantially greater than those from the UCR. Although trends move in the same direction over this period, the estimated percentage drop in crime is notably more pronounced in the NCVS. For example, data from the UCR imply that the annual rate of aggravated assaults fell from 4.2 in 1990 to 2.9 in 2005 – a 31% drop – while data from the NCVS indicate that the annual rate fell from 9.8 in 1990 to 4.3 in 2005 – a 56% drop.

Such discrepancies are largely attributable to basic definitional and procedural differences between the two surveys (US Department of Justice 2004; Lynch and Addington 2007). The two datasets measure different aspects of crime, with the UCR aiming to provide a measure of the number of crimes reported to law enforcement authorities, and the NCVS aiming to measure criminal victimization, including crimes not reported to authorities. These surveys also differ in the way they measure criminal behavior: the UCR is administrative data collected from individual criminal justice agencies (e.g. police departments), whereas the NCVS is a large-scale social survey that relies on self-reports of victimization.

Both methods of collecting data give rise to a number of response error concerns, including the potential for false reporting, nonstandard definitions of events, and general difficulties associated with collecting information on sensitive topics and illegal behavior. In the NCVS, for example, self-reports by respondents who are concerned with the consequences of truthful admissions may yield a number of inaccurate reports. Likewise, police discretion in whether and how to record incidents may lead to substantial errors in the measurement of reported crimes in the UCR (Mosher et al. 2002, 84–86). In fact, Black (1970) observes that about one-quarter of reported felonies and just under one-half of reported misdemeanors are never formally recorded by the police.

In this section, we review several examples of measurement errors that are thought to confound inference on crime and criminal justice policy. Three related types of data errors are illustrated: response errors, proxy errors, and imputation errors.

Response Errors

Response errors arise when survey respondents misreport information. In the NCVS, for example, some respondents may misreport the incidence of crime; in the UCR, some police

may fail to report, or misclassify reported crimes. Although there is much indirect evidence of response errors in survey data on illicit behavior, there is almost no direct evidence – especially for the basic crime data collected in the UCR and NCVS. An exception is a series of validation studies that evaluate the incidence of illicit drug use by comparing self-reports to laboratory tests on hair, blood and urine. These validation studies suggest nontrivial and systematic self-reporting errors, but only apply to select populations (NRC 2001).

INDIRECT EVIDENCE ON RESPONSE ERRORS. Indirect evidence on response error is often framed in terms of disparate findings between apparently similar surveys. A striking example is found in the literature examining the incidence of rape. A number of surveys reveal that between 20 and 25% of American women have been victims of completed or attempted rape at some point over their lifetime (Koss 1993, Table 1); yet, NCVS data – which measure yearly, not lifetime, victimization – indicate that less than 0.1% of women experience a rape or attempted rape (Koss 1996). Similarly, divergent conclusions can be found in the most widely cited studies of the incidence of defensive gun use. Using data from the 1993 National Self-Defense Survey (NSDS), Kleck and Gertz (1995) estimate over 2 million defensive gun uses per year; yet, the NCVS data from 1992 and 1994 reveals just over one-hundred thousand defensive gun uses per year (McDowall et al. 1998).

These large differences have been attributed to the use of different survey questions, to sampling variability, and to response errors. As discussed in NRC (2005) and Tourangeau and McNeeley (2003), the surveys are structurally different, covering different populations, interviewing respondents by different methods, using different recall periods, and asking different questions. While the surveys attempt to study common topics, the particular measurements taken are different. Moreover, because rape and defensive gun use are sensitive, stigmatized and difficult to define, small differences in survey methods may lead to large differences in the quality of the self-reported data on these topics (NRC 2003). Although response errors almost certainly affect these data, the extent of such errors is unknown.

DIRECT EVIDENCE ON RESPONSE ERRORS. In contrast to the previous examples, a number of validation studies provide direct evidence on the direction and magnitude of response errors in self-report data on illicit drug use. As summarized by NRC (2001) and Harrison and Hughes (1997), report-validation studies have been conducted on arrestees, addicts in treatment programs, employees, and persons in high-risk neighborhoods. Some of the most detailed and important validation studies were conducted with data from the arrestee drug abuse monitoring (ADAM)/drug use forecasting (DUF) survey of arrestees, which elicits self-reports of drug use and also conducts urinalysis tests. Comparing self-reports of marijuana and cocaine use during the past three days to urinalysis tests for the same period, Harrison (1995) finds evidence of substantial and systematic response errors. In particular, she finds between 15 and 30% of respondents give inaccurate answers, with typically higher rates of false negatives than false positives. In the 1989 ADAM/DUF survey, for example, Harrison (1995) finds that 24.7% of respondents falsely deny using cocaine in the past three days, while 2.3% provide false positive reports. For marijuana use, 11.0% of responses are false negatives and 11.3% are false positives.

These validation studies provide some of the only direct evidence we have about the degree and nature of misreporting in surveys of illicit behaviors – we are not aware of similar direct evidence on response errors in other surveys of crime and victimization. Moreover, these studies provide only limited evidence about misreporting in national surveys on illicit drug use. The current validation studies examine particular subpopulations of individuals

who have much higher rates of drug use than the general population. Response rates in the validation studies are often quite low, and respondents are usually not sampled randomly from a known population.

Proxy Variable Errors

In studies of illicit activities, it is often costly or impossible to directly measure variables of interest. In these cases, researchers may decide to use measurable variables as stand-ins or “proxies” for the unobserved variables. A proxy variable may be closely related to the unobserved variable of interest, but it is not a measurement of that variable. As a result, using a proxy in place of an unobserved variable can introduce substantial measurement error.

An important example is the use of UCR data on reported crime to act as a stand-in for the actual incidence of crime. A central but often overlooked problem is that many crimes are not reported to the police. The propensity of individuals to report crimes may be influenced by a variety of factors including the actual crime rate, the way victims are treated, community policing efforts, police manpower, and so forth (Rand and Rennison 2002). It is therefore possible for crime reported to police, as measured by the UCR, to rise or fall independent of changes in actual crime. In this case, UCR data may result in misleading conclusions about crime trends and the impact of criminal justice policies on crime.

A number of other proxy variables are used in crime and justice research. For example, ecological studies evaluating the relationship between access to firearms and crime frequently rely on proxy variables for the rate of gun ownership (NRC 2005). Proxies employed in the literature include the fraction of homicides committed with a firearm, the fraction of suicides committed with a firearm, and subscription rates to *Guns & Ammo* magazine (Azrael et al. 2001). Similarly, without access to information on the quantity of illicit drugs consumed, attempts to evaluate the demand for illicit drugs often rely on proxy variables. A common proxy for the actual quantity of drugs consumed is an indicator of whether an individual has used an illegal drug in a specified period of time. The accuracy of this proxy is unknown (NRC 2001).

Imputation Errors

In nearly every survey, some members of the surveyed population choose not to respond to particular questions. This missing data problem is often addressed using imputation procedures that fill in missing data using values from complete records in the same dataset. Imputation errors arise when the values of imputed data differ from the true values of the underlying variable. Importantly, this measurement problem is conceptually different from the response and proxy variable problems. With imputation errors, the fraction of nonrespondents is known, and the survey often identifies records with imputed values. This contrasts with response and proxy variable problems, where the fraction of observations measured with error is generally unknown. Thus, in principle, we can learn much more about the implications of imputation error than response and proxy variable errors.

An important example of this type of error in criminal justice research involves the imputation of missing values in UCR data. These data are compiled through the voluntary reporting of local agencies. Some agencies neglect to provide information as called for in the reporting protocol, while others fail to report altogether (Maltz 1999; Maltz and Targonski 2002).

In 2003, for example, over one-third of agencies filed incomplete monthly reports (Lynch and Jarvis 2008). A higher frequency of nonresponding agencies come from small jurisdictions, so that the agencies with missing data serve just over 12% of the population.

The FBI uses a simple procedure to impute the values of crime data for nonresponding agencies. Missing values for agencies reporting three or more months of crime data are replaced by the agency-average crime rate over observed months. For agencies reporting two or fewer months, crime rates from agencies of similar size and location are used (for details, see Maltz 1999).

Surprisingly, little research has been aimed at examining the inferential implications of imputed response problems in the UCR. Maltz and Targonski (2002) argue that imputation methods are likely to bias estimates of crime rates and program evaluation at the county level, but may be benign for analyses at higher geographic aggregations. Lynch and Jarvis (2008), however, find that the imputations can have a large impact on estimates of national trends in crime: from 1992 to 1993, UCR data without imputed values indicate a 4.7% drop in the volume of offenses, whereas this drop is only 1.9% when imputed values are included in the data.

THE CONVOLUTION MODEL

Because data used to monitor crime and evaluate crime policy are invariably measured with error, it is important to understand how such errors may impact inferences, and whether anything can be done to credibly mitigate negative effects. Exactly how data errors affect statistical inference depends heavily on the specifics of the problem: critical details include the type of model being estimated, the way measurement errors enter the model, and the joint distribution of the observed and unobserved random variables.

In this section, we use a convolution model, where errors are approximated by unobserved additive random terms, to focus on the problem of drawing inferences on a bivariate mean regression model. This model of data errors can be used to formalize the effects of response errors, and with minor modifications can also be generalized to accommodate proxy variable errors. Imputation errors, where only a known fraction of observations may be in error, are probably better addressed by the mixing model considered in section “The Mixture Model.” Consider the bivariate mean regression model

$$y^* = \alpha + x^*\beta + \varepsilon, \quad (18.1)$$

where y^* and x^* are scalars and ε is an unobserved random variable distributed according to some probability distribution F_ε , mean independent of x^* : formally, we assume

$$[A1] \ E[\varepsilon|x^*] = 0.$$

Assumption A1 is the standard mean independence requirement for linear regression models. Given a random sample on (y^*, x^*) , one can consistently estimate (α, β) using the ordinary least-squares estimator.

In the presence of data errors, however, x^* and y^* may not be revealed by the sampling process. Rather, the convolution error model assumes observable data are imperfect reflections of the true variables of interest: $x = x^* + \mu$ and $y = y^* + \nu$, with unobserved measurement errors, μ and ν , randomly distributed according to probability distributions F_μ and F_ν

respectively. The sampling process does not reveal the joint distribution of the variables of interest, (x^*, y^*) , but it does reveal that of (x, y) . Specifically, we assume an observable random sample of size N : $\{(x_i, y_i)\}_{i=1}^N$.²

What can this sampling process reveal about the parameters of interest, namely (α, β) ? Under various assumptions on the characteristics of the error distributions, F_μ and F_ν , we explore the effects of measurement error on the probability limit of the least squares slope estimator, and suggest potential solutions to the inferential problems caused by data errors. We first review the classical errors-in-variables model, and then motivate and consider several nonclassical variations on this model. Finally, we discuss complications introduced by measurement error in regressions on differenced panel data. These examples are by no means exhaustive, but help give a taste for important considerations and implications when using error-ridden measurements in statistical analysis.³ In particular, we observe the following:

- (a) The “classical” generating processes, where errors are assumed to be mean zero and exogenous, is unlikely to apply in many of the important data error problems in criminal justice research
- (b) Outcome-variable errors, ν , impact inferences on the mean regression in (18.1) and, in particular, can bias inferences on crime levels and trends
- (c) Regressor errors, μ , do not always result in attenuation bias
- (d) In panel data models, the impact of measurement error can be exaggerated by differencing or the inclusion of fixed effect terms.

In the following, let $\sigma_{x^*}^2$ denote the population variance of x^* , with similar notation for other variables, and let $\sigma_{x^*, \mu}$ and $\rho_{x^*, \mu}$ denote the population covariance and correlation of x^* and μ with similar notation for other pairwise combinations of variables. In regression models, we adopt the terminology that y^* is the “outcome variable” while x^* is the “regressor.” Finally, let $\hat{\beta}_{y,x}$ be the ordinary least squares (OLS) slope estimator from a sample regression of y on x .

Classical Assumptions

The classical measurement error model supposes that the additive error terms μ and ν are mean zero, uncorrelated with the true values of all variables in the model, and uncorrelated with each other. In the present model, classical measurement error assumptions may be stated as follows:

$$[A2] E[\mu] = E[\nu] = 0$$

$$[A3] \sigma_{x^*, \nu} = \sigma_{\mu, \nu} = \sigma_{\varepsilon, \nu} = 0$$

$$[A4] \sigma_{x^*, \mu} = 0$$

$$[A5] \sigma_{\varepsilon, \mu} = 0.$$

²To accommodate proxy errors, this model has often been generalized by including a factor of proportionality linking the observed and true variables. For example, $y = \delta y^* + \nu$, where δ is an unknown parameter. For brevity, we focus on the pure measurement model without an unknown factor of proportionality. Including a scaling factor of unknown magnitude or sign induces obvious complications beyond those discussed here. For additional details, see Wooldridge (2002, 63–67) and Bound et al. (2001, 3715–3716).

³The interested reader should consult more detailed presentations in Wooldridge (2002), Wansbeek and Meijer (2000) and Bound et al. (2001).

Assumption A2 implies that the measurement errors, μ and ν , are mean zero, while the remaining assumptions restrict errors to be uncorrelated with each other, with the outcome variable, and with the regressor. In particular, assumption A3 implies that the error in the outcome variable, ν , is uncorrelated with the other measurement error, μ , with the regressor x^* , and with the outcome variable y^* . Assumptions A4 and A5 restrict the error in the regressor, μ , to be uncorrelated with the true value of the regressor, x^* , and with the outcome, y^* .

Under A1–A5, it is well known that the probability limit of the OLS slope parameter is proportional to the true value β in the following way (Wooldridge 2002):

$$\text{plim}_{N \rightarrow \infty} \hat{\beta}_{y,x} = \beta \frac{\sigma_{x^*}^2}{\sigma_{x^*}^2 + \sigma_{\mu}^2}. \quad (18.2)$$

Two important conclusions may be drawn from (18.2). First, measurement error in the outcome variable does not affect the consistency of $\hat{\beta}_{y,x}$: the slope coefficient from a sample regression of y on x^* is a consistent estimator of β .⁴ Second, classic measurement error in the regressor causes the sample regression slope parameter to be an inconsistent estimator of β such that asymptotically $\hat{\beta}_{y,x}$ has the same sign as β but is closer to zero. This effect is generally termed “attenuation bias.” The presence of measurement error in the regressor dilutes the apparent strength of the relationship between x^* and y^* , causing the estimated slope parameter to understate the magnitude of the true effect. While we focus on the OLS estimator of the slope parameter, the estimator of the constant term, α , is also inconsistent when the regressor is measured with error.⁵

With access to auxiliary data or model structure, the parameters (α, β) may be point identified.⁶ For example, one common approach is to exploit an instrumental variable, z , that is known to be independent of all of the unobserved error terms, (μ, ν, ε) , but is also correlated with the regressor, x^* . In particular, assume that

$$[\text{A6}] \sigma_{z,x^*} \neq 0$$

$$[\text{A7}] \sigma_{z,\mu} = \sigma_{z,\nu} = 0$$

$$[\text{A8}] \sigma_{z,\varepsilon} = 0.$$

In this case, the instrumental variable (IV) estimator is consistent for β :

$$\text{plim}_{N \rightarrow \infty} \hat{\beta}_{y,x(z)}^{IV} = \beta. \quad (18.3)$$

⁴ When the available measurement of y^* is a proxy variable of the form $y = \delta y^* + \nu$, the probability limit of $\hat{\beta}_{y,x^*}$ is $\delta\beta$. If δ is known in sign but not magnitude, then the sign, but not scale, of β is identified.

⁵ Although a full treatment of the effects of measurement error in multivariate regression is beyond the scope of this chapter, several general results are worth mentioning. First, measurement error in any one regressor will usually affect the consistency of all other parameter estimators. Second, when only a single regressor is measured with classical error, the OLS estimator of the coefficient associated with the error-ridden variable suffers attenuation bias in the standard sense (see, for example, Wooldridge 2002, 75). In general, all other OLS parameters are also inconsistent, and the direction of inconsistency can be asymptotically signed by the available data. Finally, with measurement error in multiple regressors, classical assumptions imply that the probability limit of the OLS parameter vector is usually attenuated in an average sense, but there are important exceptions (Wansbeek and Meijer 2000, 17–20).

⁶ A number of possible strategies are available, and the interested reader should consult the discussions in Wansbeek and Meijer (2000) and Bound et al. (2001).

It is often observed that alternative measurements of x^* may serve as instrumental variables satisfying these conditions. For example, suppose a researcher has access to an alternative measurement $x' = x^* + \eta$ with η randomly distributed according to some probability distribution F_η . If error in the alternative measurement, η , is classical and uncorrelated with μ , then $z = x'$ satisfies A6–A8 and a consistent point estimator of β is available even in the presence of classical measurement error.⁷

In the absence of auxiliary data or structure, point identification of β is impossible. Under classical assumptions, however, the sampling process places bounds on the true value of β (Frisch 1934). Part of the work is already done: with classical measurement error in the outcome variable, (18.2) shows that probability limit of $\hat{\beta}_{y,x}$ is closer to zero than is the true value of β , so that $|\hat{\beta}_{y,x}|$ is an estimable lower bound on the magnitude of β .

Now, consider a new estimator of β constructed from the reverse regression of x on y . Specifically, define the new estimator of β as the inverse of the ordinary least squares estimator of the slope from a regression of x on y : $\hat{\beta}_{x,y}^{-1}$. Under A1–A5, the probability limit of this new estimator is

$$\text{plim}_{N \rightarrow \infty} \hat{\beta}_{x,y}^{-1} = \beta + \frac{\sigma_\varepsilon^2 + \sigma_v^2}{\beta \sigma_{x^*}^2}. \quad (18.4)$$

Like the usual slope estimator from a regression of y on x , the probability limit of $\hat{\beta}_{x,y}^{-1}$ has the same sign as β . Unlike the usual slope estimator, however, the probability limit of $\hat{\beta}_{y,x}$ is farther from zero than is the true value of β .

This means that in the presence of classical measurement error, data on x and y alone can be used to provide informative bounds on the set of possible values of β . When $\beta > 0$, $\hat{\beta}_{x,y}^{-1}$ and $\hat{\beta}_{y,x}$ are asymptotic upper and lower bounds on the true value of the slope parameter:

$$\text{plim}_{N \rightarrow \infty} \hat{\beta}_{y,x} < \beta < \text{plim}_{N \rightarrow \infty} \hat{\beta}_{x,y}^{-1} \quad (18.5)$$

and inequalities reverse when $\beta < 0$.⁸

Problems with Classical Assumptions

In any analysis with imperfectly measured data, the researcher should carefully consider whether classical assumptions are appropriate. Trivial examples which give rise to classical conditions are the cases where errors are generated by random clerical mistakes in data entry or by sampling variation when x^* and y^* represent population averages (Bound et al. 2001). In general, however, classical measurement error assumptions are inappropriate. Failure of any one of the assumptions has implications for drawing inferences on the regression parameters (α, β).

⁷ Of the required conditions for using a second measurement as an instrumental variable, the assumption that the two errors are uncorrelated, $\sigma_{\eta,\mu} = 0$, may be the most difficult to satisfy in practice. Even if both errors are classical in other regards, errors in different measurements of the same variable may be expected to correlate so that $\sigma_{\eta,\mu} > 0$. When covariance between μ and η is nonzero, the IV slope estimator is no longer a consistent point estimator of β , though it may still provide an informative bound on β under certain circumstances (see, for example, Bound et al. 2001, 3730; Black et al. 2000).

⁸ Klepper and Leamer (1984) suggest a similar strategy for the case where multiple regressors are measured with error. The general approach is described by Bound et al. (2001, 3722–3723).

To evaluate these problems, we present two illustrations where nonclassical measurement errors are likely to confound inference: these are the common monitoring problem of inferring levels and trends in crime rates, and the problem of inferring how expected crime rates vary with illicit drug use. In each situation, we argue that various classical assumptions are unlikely to hold. We then derive formal results to illustrate the implications of alternative assumptions on the measurement error process.

ILLUSTRATION 1: CRIME LEVELS AND TRENDS. Perhaps, the most central function of the data on crime is to monitor levels and trends in crime rates. Level estimates from the UCR and NCVS, however, are thought to be downwardly biased. This violates assumption A2, because measurement errors are not mean zero.

As estimated crime rates are perceived to be systematically biased, trend estimates are often argued to be more reliable than levels. An example is the study of differences in crime statistics between the UCR and NCVS; these two crime series differ systematically in levels, with the NCVS always estimating a higher rate of crime than the UCR (see Table 18.1). Despite obvious level differences, some researchers suggest that the two series are comparable in terms of long-term time trends (US Department of Justice 2004). On this topic, McDowall and Loftin (2007, 96) argue that studies attempting to reconcile differences between the UCR and NCVS crime rates might want to focus on differences in trends as a less demanding standard of comparability than differences in levels.

Similar sentiments have been expressed in the context of survey data on illicit drug use. To the extent that survey respondents may be reluctant to admit engaging in illegal and socially unacceptable behavior, it seems likely that drug use data may suffer from a systematic, negative bias. On the basis of the premise that measurement errors are constant over time, Johnston et al. (1998, 47–48) argue that measurements of drug use trends should be robust to the presence of measurement error. Anglin et al. (1993, 350) take a similar stance, claiming “[I]t is easier to generate trend information. . . than to determine the absolute level.”

Under what conditions might one consistently estimate trends, even if the level estimates are systematically biased? Let x^* be an indicator function measuring two distinct time periods, say 2009 and 2010, and suppose the noisy reflection of the crime rate satisfies $y = y^* + v$. Assume that the measurement error, v , is uncorrelated with the time period so that $\sigma_{x^*,v} = 0$, but allow for the possibility of nonzero expected errors (e.g., $E[v] < 0$). That is, maintain assumption A3 but relax A2 to allow for nonzero mean errors. In this case, with only the outcome variable measured with error, the OLS estimator of the slope parameter from a regression of y on x^* is a consistent estimator of β (see (18.2)) but the estimator of α is generally inconsistent:

$$\text{plim}_{N \rightarrow \infty} \hat{\alpha}_{y,x^*} = \alpha + E[v]. \quad (18.6)$$

Thus, even when the available measurement of y^* is systematically biased, we can consistently estimate the trend, β , but not the level of $E[y^*|x^*]$.

The result that trends are robust to biased measurements of the outcome variable is critically dependent on the assumption that measurement error is uncorrelated with the observed regressor: assumption A3. The reason results depend so heavily on this condition is easily seen in terms of general conditional expectations of y at arbitrary values $x^* = x_a^*$ and $x^* = x_b^*$. For any given value of x^* – say, $x^* = x_a^*$ – the expected value of y is

$$E[y|x^* = x_a^*] = E[y^*|x^* = x_a^*] + E[v|x^* = x_a^*] \quad (18.7)$$

and the difference in conditional expectations is

$$E[y|x^* = x_a^*] - E[y|x^* = x_b^*] = (E[y^*|x^* = x_a^*] - E[y^*|x^* = x_b^*]) + (E[v|x^* = x_a^*] - E[v|x^* = x_b^*]). \quad (18.8)$$

Thus, the observed level is equal to the true level only when $E[v|x^*] = 0$, which is violated in this example. The observed difference, on the other hand, is equal to the true difference under the weaker condition that $E[v|x^*] = E[v]$ for all x^* . Intuitively, if the expected measurement error is mean independent of the conditioning variable, then the bias terms cancel out so that changes in conditional expectations of y are the same as changes in the conditional expectation of y^* .

As previously asserted, the critical assumption is that measurement errors in y are unrelated to the conditioning variable x^* . This assumption is particularly questionable when x^* represents time. Consider measurement errors in self reports of illicit drug use: changes over time in the social and legal stigma of drug use seem likely to correlate with response errors (see [Pepper 2001](#)). Similarly, for crime rate data in the UCR, the frequency and quality of reporting seems likely to vary over time in response to changes in laws, social attention to crime, and the crime rate itself ([Mosher et al. 2002](#); [Rand and Rennison 2002](#)).

Nonclassical measurement errors in the outcome variable are also likely to impact numerous program evaluation studies of criminal justice policy. Consider, for example, the problem of assessing the impact of the police-force size or policing practices on crime, or the impact of restrictive firearms policies on crime. In these cases and many others, errors in measuring crime are likely to be associated with effects of the policies of interest.

When measurement errors in crime data are associated with the conditioning variable, analysis in trends or marginal changes is not clearly preferable to that in levels. Comparing bias terms in (18.7) and (18.8), the absolute magnitude of $E[v|x^*]$ may be greater than that of $E[v|x_a^*] - E[v|x_b^*]$, but relative to $E[y^*|x^*]$ and $E[y^*|x_a^*] - E[y^*|x_b^*]$, the impact of the bias term in changes may well exceed that in levels. Certainly, the claim that crime data are biased in levels but not trends cannot generally be supported.

ILLUSTRATION 2: DRUGS AND CRIME. In many cases, concerns over errors in the regressor may also play an important role in inference. As an example, suppose a researcher sought to measure the effect of drug use on the propensity to engage in criminal behavior ([Bennett et al. 2008](#); [Chaiken and Chaiken 1990](#)). Assume the bivariate mean regression model in (18.1), where x and y are indicator functions for self reports of drug use and criminal activity, respectively. To properly interpret the slope parameter from a sample regression of y on x , a detailed understanding of the sources of measurement error is required. Far from random clerical errors or sampling variability, measurement errors in this model potentially violate classical assumptions A3–A5.

Take assumption A3, which requires measurement error in the outcome variable to have zero covariance with both the true value of the regressor and the measurement error in this variable. If individuals who falsely deny engaging in one of the activities are likely to falsely deny engaging in both (with similar logic for false positives), then measurement errors in self reports of x^* and y^* will positively correlate in the population: $\sigma_{v,\mu} > 0$. Likewise, errors in reporting criminal activity, v , are arguably related to whether the respondent actually used illicit drugs, x^* .

Because x^* is a binary variable in this model, assumption A4 is violated by definition. To see why, note that when $x^* = 0$ any error must be a false positive, and when $x^* = 1$ any

error must be a false negative. As such, errors in the regressor exhibit negative correlation with true values of the variable, violating assumption A4.⁹ Of course, assumption A4 can also be violated in cases where x^* is not binary. Suppose binary reports were replaced by state-level aggregates of self-reported drug use and criminal behavior. If the negative stigma of drug use decreases as drug use becomes more mainstream, then errors in the state-level statistics should again negatively correlate with actual aggregate drug use: $\sigma_{x^*,\mu} < 0$.

Finally, assumption A5 fails when measurement error in the regressor is correlated with unobserved random variable, ϵ , in the population regression. An active police presence in the community may tend to reduce the likelihood that an individual would actually commit a crime and at the same time make individuals less likely to truthfully admit illicit drug use, leading to negative covariance between measurement error in the regressor, μ , and the regression error term, ϵ . A similar story can be told at the state level. Violence and other criminal behavior in areas with higher-than-average crime rates may desensitize these populations to the negative stigma associated with admitting drug use, again leading to negative covariance between drug use statistics and the error term in the population model: $\sigma_{\mu,\epsilon} < 0$.

NonClassical Assumptions

As the previous illustrations demonstrate, classical assumptions may be invalid in many important applications. Although the effects of measurement error on the consistency of the OLS slope estimator were simple to characterize under classical errors-in-variables assumptions (A2–A5), relaxing these assumptions leads to a less parsimonious probability limit:

$$\text{plim}_{N \rightarrow \infty} \hat{\beta}_{y,x} = \frac{\sigma_{x^*,v} + \sigma_{\mu,v}}{\sigma_{x^*}^2 + \sigma_{\mu}^2 + 2\sigma_{x^*,\mu}} + \beta \frac{\sigma_{x^*}^2 + \sigma_{x^*,\mu}}{\sigma_{x^*}^2 + \sigma_{\mu}^2 + 2\sigma_{x^*,\mu}} + \frac{\sigma_{\mu,\epsilon}}{\sigma_{x^*}^2 + \sigma_{\mu}^2 + 2\sigma_{x^*,\mu}}. \quad (18.9)$$

Each term on the right-hand side of (18.9) corresponds to the failure of one of the classical assumptions: A3–A5.¹⁰ The first term is nonzero when errors in the outcome are related to the true value of the regressor or its measurement error, so that assumption A3 is not satisfied. The second term differs from the classical result when errors in the regressor are related to the true value of the regressor, $\sigma_{x^*,\mu} \neq 0$, so that assumption A4 is not satisfied. The third term is nonzero when the errors in the regressor are related to the regression residual, so that assumption A5 does not hold.

In this more general setting, the two central lessons of the classical errors-in-variables model no longer apply: data errors in the outcome variable have consequences for inference, and measurement errors in the regressor need not bias the OLS estimator toward zero. In particular, because the first term in (18.9) is unrestricted in both size and sign, the failure of A3 alone is a sufficient condition for $\hat{\beta}_{y,x}$ to be potentially inconsistent for both the sign and magnitude of the true slope parameter. Likewise, the failure of assumption A5 – where the error in the regressor, μ , is related to the regression error, ϵ – leads to an additive term which is unrestricted in size and sign. Failure of A5 is also sufficient to cause general inconsistency of the OLS estimator of β .

⁹ Similar logic suggests violation for any discrete variable, and any continuous but bounded variable.

¹⁰ Failure of assumption A2 affects inference regarding α , but not β (see, for example, Illustration 1).

Even if A3 and A5 hold, so that the only deviation from classical assumptions is nonzero covariance between the true value of the regressor and its measurement error, attenuation bias is not guaranteed. If $\sigma_{x^*,\mu} > 0$, then $\hat{\beta}_{y^*,x}$ is consistent for the sign of β and indeed suffers from attenuation bias. However, when $\sigma_{x^*,\mu} < 0$, as might be the case in a study of the impact of illicit drug use on crime, nonclassical error in the regressor may lead to arbitrary inconsistencies. Depending on the relative variance and correlation of x^* and μ , the probability limit of the OLS slope estimator may have the incorrect sign, or may artificially amplify (rather than attenuate) the strength of the relationship between the outcome variable and regressor.

As should be clear at this point, nonclassical measurement error is a more insidious concern than classical error. Without auxiliary data or structure, the sampling process places no restrictions on the true value of β , and the prospects for identification and estimation of β are grim.

Under certain restrictions, however, it may still be possible to construct informative bounds on β . For example, consider the situation where the only deviation from classical assumptions is a negative covariance between the regressor and its measurement error; $\sigma_{x^*,\mu} < 0$. Thus, assumptions A2, A3 and A5 are assumed to hold, but assumption A4 does not. Assume for simplicity that $\beta > 0$. As long as x^* and μ are not too highly correlated, the OLS estimator still acts as an asymptotic lower bound on β , and under fairly weak conditions the IV estimator using an alternative measurement of x^* is an asymptotic upper bound on the true slope parameter (Black et al. 2000).¹¹ Thus, the value of β can be bounded in a manner similar to the case for the classical errors-in-variables model:

$$\text{plim}_{N \rightarrow \infty} \hat{\beta}_{y,x} < \beta < \text{plim}_{N \rightarrow \infty} \hat{\beta}_{y,x}^{IV} \quad (18.10)$$

and inequalities reverse when $\beta < 0$.

Panel Data Models

Up to this point, we have maintained assumption A1 so that OLS estimators of (α, β) would be consistent if x^* and y^* were observable in the data. In many cases, however, this assumption is likely to be violated. A common example is the situation where multiple observations are collected on each sampling unit: for example, let i index a county and let t index the year of the observation. Let the unobserved regression error contain unit-specific effects, α_i , so that $\varepsilon_{i,t} = \alpha_i + \omega_{i,t}$ where α_i is potentially correlated with $x_{i,t}^*$ and $\omega_{i,t}$ is an unobserved random variable distributed according to some probability distribution F_ω , mean independent of x^* . To simplify discussion, suppose $y_{i,t}^*$ is measured accurately, $x_{i,t} = x_{i,t}^* + \mu_{i,t}$ is an imperfect measurement of $x_{i,t}^*$, classical measurement error assumptions hold, and all variables have stationary variance and covariance across counties and time.¹²

We assume access to a panel-data sample of two years worth of data: $\{(x_{i,t}, y_{i,t})\}_{i=1}^N$. Denote by $\hat{\beta}_{y,x}$ the OLS slope estimator from regression of $y_{i,t}$ on

¹¹ Bollinger (1996) and Frazis and Loewenstein (2003) derive bounds when a binary regressor is measured with error.

¹² With panel data, assumptions A1–A5 must account for correlations in both the cross-section and the time series dimension. For detailed examples, see Wooldridge (2002) and Griliches and Hausman (1985).

$x_{i,t}$ which does not account for variation in α across counties. As should be expected, $\hat{\beta}_{y,x}$ is an inconsistent estimator of β :

$$\text{plim}_{N \rightarrow \infty} \hat{\beta}_{y,x} = \beta \frac{\sigma_{x^*}^2}{\sigma_{x^*}^2 + \sigma_{\mu}^2} + \frac{\sigma_{x^*,\alpha} + \sigma_{\mu,\alpha}}{\sigma_{x^*}^2 + \sigma_{\mu}^2}. \tag{18.11}$$

The first term in (18.11) is just the classical attenuation bias observed in the linear regression models without unobserved effects. The second term comes from failure to account for unobserved county-specific effects: these terms end up in a composite regression-error term which correlates with x_i^* , effectively violating the assumption of conditional mean zero regression errors (assumption A1) and resulting in an additional source of inconsistency.

To avoid problems caused by ignoring time-constant unobserved effects, the researcher may exploit the panel structure of the collected data. For example, a first difference (FD) estimator eliminates any time-constant terms while leaving β estimable:

$$\Delta y_i^* = \Delta x_i^* \beta + \Delta \varepsilon_i, \tag{18.12}$$

where $\Delta y_i^* = y_{i,2}^* - y_{i,1}^*$ with similar notation for other variables, and where $\Delta \alpha_i = 0$ by definition. With an accurate measurement of x_i^* available to the researcher, OLS performed on Δy_i^* and Δx_i^* would provide a consistent estimator of β .

Since x_i^* is not observed, let $\hat{\beta}_{\Delta y^*, \Delta x}$ denote the slope parameter from a sample regression of Δy_i^* on Δx_i . Under classical assumptions, $\hat{\beta}_{\Delta y^*, \Delta x}$ suffers from standard attenuation bias:

$$\text{plim}_{N \rightarrow \infty} \hat{\beta}_{\Delta y^*, \Delta x} = \beta \frac{\sigma_{\Delta x^*}^2}{\sigma_{\Delta x^*}^2 + \sigma_{\Delta \mu}^2}. \tag{18.13}$$

At first glance, (18.13) would seem an improvement upon (18.11), with consistency of $\hat{\beta}_{\Delta y^*, \Delta x}$ only limited by attenuation bias due to the presence of measurement error in x_i . While the FD estimator does eliminate inconsistency due to the presence of time-constant unobserved variables, it may also exacerbate attenuation bias due to measurement error. More extreme attenuation bias results if the true regressor exhibits relatively strong serial correlation while measurement error does not.¹³

Switching from levels to differences is not uniformly preferable when the regressor is measured with error. On one hand, working with differences allows the researcher to eliminate inconsistency due to the presence of time-constant unobserved effects. On the other hand, differencing and related approaches may tend to increase the magnitude of measurement error bias when true values of the regressor exhibit strong serial correlation and measurement errors do not. For a detailed discussion of related panel data models and solutions when more than two periods of data are available, see [Griliches and Hausman \(1985\)](#).

THE MIXTURE MODEL

The previous section presented textbook results for several chronic errors-in-variables models, where the observable variable, y , is the noisy reflection of the true variable of interest, y^* ,

¹³ Note that the variance of Δx_i^* is smaller when x_i^* has positive autocorrelation: $\sigma_{\Delta x^*}^2 = 2\sigma_{x^*}^2(1 - \rho_{x_2^*, x_1^*})$. To see why the relative strength of serial correlation is a concern, suppose that $\rho_{x_2^*, x_1^*} > 1/2$ while random measurement errors exhibit no autocorrelation. This implies $\sigma_{\Delta x^*}^2 < \sigma_{x^*}^2$ while $\sigma_{\Delta \mu}^2 = 2\sigma_{\mu}^2$, so attenuation bias will be greater after first differencing the data.

such that $y = y^* + \nu$. In many settings, this model of chronic errors may be inappropriate. When considering imputation errors in the UCR, for example, we know that some observations are imputed while others are not. Likewise, when focusing on invalid response problems regarding victimization in the NCVS or illicit drug use in the National Survey of Drug Use and Health (NSDUH), it seems likely that some respondents report accurately while others may not.

In light of these concerns, a growing body of literature conceptualizes the data error problem using a mixture model in which the observed outcome distribution is a mixture of the unobserved distribution of interest, and another unobserved distribution. See, for example, Horowitz and Manski (1995), Lambert and Tierney (1997), Dominitz and Sherman (2004), Mullin (2005) and Kreider and Pepper (2007, 2008, forthcoming). In this setting, the observed random variable, y , is viewed as a contaminated version of the variable of interest, y^* . In particular, the observed variable is generated by the mixture:

$$y = y^*z + \tilde{y}^*(1 - z), \quad (18.14)$$

where z indicates whether the observed outcome, y , comes from the distribution F_{y^*} or some alternative distribution, $F_{\tilde{y}^*}$.

In this environment, the “contaminated sampling” model pertains to the case in which the mixing process, z , is known to be statistically independent of sample realizations from the distribution of interest, y^* . The more general “corrupted sampling” model pertains to the case where nothing is known about the pattern of data errors.

Using nonparametric methods, Horowitz and Manski (1995) derive sharp bounds on the distribution of y^* under both corrupt and contaminated sampling models. Hotz et al. (1997), Kreider and Pepper (2007), Kreider and Hill (2009), and Kreider et al. (2009) use this framework to derive bounds on the mean regression model when a regressor, x^* , is measured with error.

To illustrate how these bounds work, we focus on the important and relatively simple example of a binary outcome variable which may in some cases be misclassified.¹⁴ To simplify the exposition, regressors are assumed to be accurately measured and are left implicit; in any of the following, one can condition the results on the observed regressors, x^* . Although our focus is on identification, the actual estimation strategy is very simple. Given a random sample from the joint distribution of (y, x^*) , the derived identification bounds can be consistently estimated by replacing population probabilities with their sample analogs.

In this context, we first present the corrupt and contaminated sampling bounds, and then apply these methods to a couple of important questions in criminal justice research. These examples are by no means exhaustive, but help give a taste for important considerations and implications when using error-ridden measurements in statistical analysis. Most notably, we observe that small amounts of measurement error can lead to substantial ambiguity about the true mean regression. Stronger assumptions on the unobserved error process can lead to sharper inferences, but may not be credible in many applications.

¹⁴ As discussed in the previous section, when a variable with bounded support is imperfectly classified, it is widely recognized that the classical errors-in-variables model assumption of independence between measurement error and true variable cannot hold. Molinari (2008) presents an alternative and useful conceptualization of the data error problem for discrete outcome variables. This “direct misclassification” approach allows one to focus on assumptions related to classification error rates instead of restrictions on the mixing process.

Mixture Model Bounds

Let y^* be an indicator function for self-reported drug use, and suppose one is interested in making inferences on the rate of illicit drug use: $P(y^* = 1) = E(y^*)$. Some unknown fraction of respondents, $P(y = 1, z = 0)$, inaccurately admit to using drugs (false positives) while another fraction, $P(y = 0, z = 0)$, inaccurately deny using drugs (false negatives). The relationship between the true and reported rates of drug use is as follows:

$$P(y^* = 1) = P(y = 1) + P(y = 0, z = 0) - P(y = 1, z = 0). \quad (18.15)$$

If the fraction of false negatives exactly offsets the fraction of false positives, then the reported rate of use equals the true rate of use: $P(y^* = 1) = P(y = 1)$. Unfortunately, the data alone only identify the fraction of the population that self-reports use: $P(y = 1)$. The sampling process cannot identify the fraction of false negatives or false positives.

A common starting point in this literature is to assume a known lower bound ν on the fraction of cases that are drawn from the distribution of interest:¹⁵

$$P(z = 1) \geq \nu. \quad (18.16)$$

A particular lower bound restriction may be informed by a validation study of a related population (e.g., [Harrison 1995](#)) or the known fraction of responses that are imputed. Moreover, by varying the value of ν , we can consider the wide range of views characterizing the debate on inaccurate reporting. Those willing to assume fully accurate reporting can set $\nu = 1$, in which case the sampling process identifies the outcome probability. Those uncomfortable with placing any lower bound on the fraction of accurate responses can set $\nu = 0$, in which case the sampling process is uninformative. Middle ground positions are evaluated by setting ν somewhere between 0 and 1.

Given the restriction that no more than some fraction, $1 - \nu$, of the population misreport, we know from (18.15) that in the case of corrupt sampling, bounds are as follows:

$$\max\{P(y = 1) - (1 - \nu), 0\} \leq P(y^* = 1) \leq \min\{P(y = 1) + (1 - \nu), 1\}. \quad (18.17)$$

Under contaminated sampling, where we are willing to assume that y^* and z are statistically independent, the bounds are different:

$$\max\{[P(y = 1) - (1 - \nu)]/\nu, 0\} \leq P(y^* = 1) \leq \min([P(y = 1)/\nu], 1). \quad (18.18)$$

These bounds are derived by [Horowitz and Manski \(1995, Corollary 1.2\)](#), and are referred to as the corrupt and contaminated sampling bounds, respectively.

Several features of these bounds are important. First, the bounds are sensitive to the upper bound misreporting rate, $1 - \nu$. Identification of $P(y^* = 1)$ deteriorates rapidly with the allowed fraction of misclassifications; so small amounts of error can have large effects on inferences. Second, the contaminated sampling bounds are narrower than the corrupt sampling bounds. Whether the independence assumption is valid, however, depends on the application:

¹⁵ This type of restriction is used in the literatures on robust statistics ([Huber 1981](#)) and data errors with binary regressors (see, e.g., [Bollinger 1996](#) and [Frazis and Loewenstein 2003](#)).

it seems unlikely, for example, that the misreporting of illicit drug use is independent of actual drug use status or that the true crime rate is independent of whether an observation must be imputed. Finally, without additional assumptions, we cannot point identify the mean regression. Rather, all we can conclude is that the true outcome probability lies within some upper and lower bound.

Mixture Model Applications

To illustrate how the mixture model might be applied in practice, we consider two important questions in criminal justice research where nonclassical measurement errors are likely to confound inference: these are the problem of using data from the NSDUH to infer the rate of illicit drug use, and using data from the UCR to infer the aggravated assault rate. In the former case, the data are contaminated from response errors; some respondents do not provide accurate reports of drug use. In the latter case, the data are contaminated from imputation errors; about one-third of all reporting agencies, representing over 10% of the population, have imputed crime data.

ILLUSTRATION 3: ILLICIT DRUG USE. To illustrate the implications of data errors using the mixing model approach, consider using data from the NSDUH to draw inferences on the true rate of illicit drug use. While there is very little information on the degree of data errors in this survey, there are good reasons to believe that the errors are extensive and systematic. Respondents concerned about the legality of their behavior may falsely deny consuming illicit drugs, while the desire to fit into a deviant culture or otherwise be defiant may lead some respondents to falsely claim to consume illicit drugs (Pepper 2001). Thus, ignoring these errors may be problematic and the classical errors-in-variables model is inappropriate.

Instead, Kreider and Pepper (2009) consider using the mixing model to address the problem of drawing inferences on the rate of marijuana use in the presence of nonrandom reporting errors. The 2002 NSDUH reveals that 54% of 18–24 year-olds claimed to have consumed marijuana within their lifetime, with 30% reporting use during the last year (Office of Applied Studies 2003). To draw inferences about true rates of illicit drug use in the United States, one must combine these self-reports with assumptions about the nature and extent of reporting errors. As noted above, Harrison (1995), who compares self-reported marijuana use to urinalysis test results among a sample of arrestees, finds a 22% misreporting rate for marijuana consumption. Arguably, the accurate reporting rate, z , in the general noninstitutionalized population exceeds that obtained in the sample of arrestees studied by Harrison (1995). Arrestees have a relatively high incentive to misreport (Harrison 1995; Pepper 2001). Under this restriction alone, the corrupt sampling bounds reveal much uncertainty about the true rates of drug use. For example, we only learn that between 32% ($=54 - 22$) and 76% ($=54 + 22$) of the young adult population has ever used marijuana. Importantly, this uncertainty reflects the identification problem caused by data errors; these bounds do not reflect the presence of additional uncertainty due to sampling variability.

Under the contaminated sampling assumption, the bounds narrow considerably. The bounds on lifetime marijuana use, for example, narrow from [32%, 76%] to [41%, 69%], a 36% reduction in bound width. When Kreider and Pepper impose the additional assumption that all draws from the alternative distribution, \tilde{y}^* , are in error (i.e., the response error model), the lifetime marijuana use rate is nearly point-identified, lying in the narrow range

[54%, 57%]. These latter findings, however, rest on the implausible contaminated sampling assumption that drug use rates are identical among accurate and inaccurate reporters. More realistically, the rate of illicit drug use is higher among inaccurate reporters. Under this restriction, the lifetime rate of marijuana use is bounded to lie within [54%, 76%].

A useful practical feature of mixing model results is that we can assess the sensitivity of the bounds to variation in v . After all, Harrison’s estimates might not accurately reflect misreporting rates in the general population. Figures 18.1 and 18.2 display bounds on the lifetime and past year marijuana use rates, respectively, under the corrupt sampling, contaminated sampling, and response error models considered by Kreider and Pepper (forthcoming). The vertical axis measures the outcome probability, $P(y = 1)$, and the horizontal axis represents the lower bound fraction of responses known to come from the distribution of interest, v .

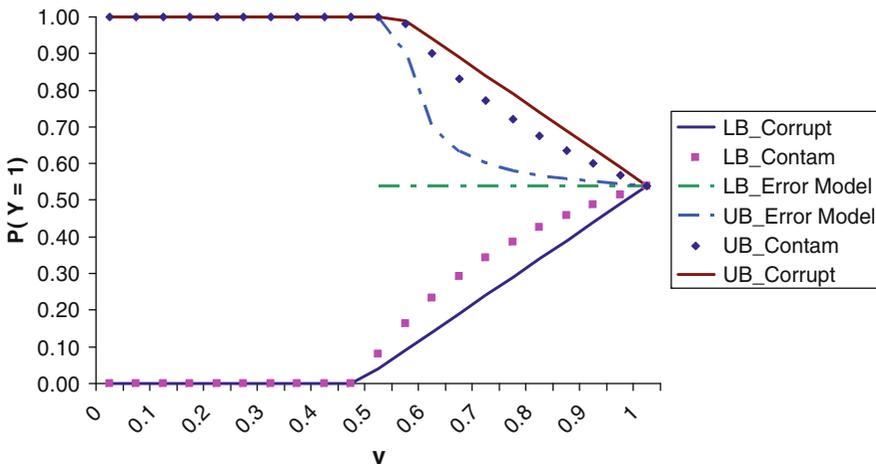


FIGURE 18.1. Bounds on lifetime marijuana use given response error [$P(y^* = 1) = 0.54$].

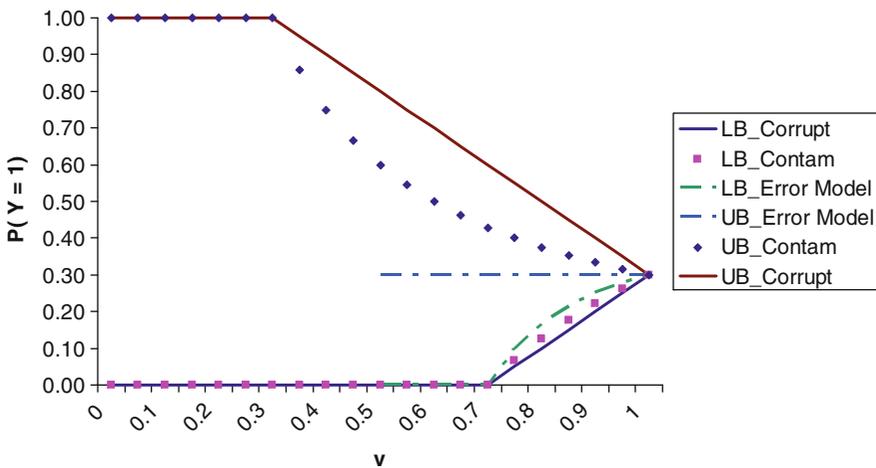


FIGURE 18.2. Bounds on past year marijuana use given response errors [$P(y^* = 1) = 0.30$].

The diagonal lines converging at $P(y = 1) = P(y^* = 1)$ when $v = 1$ depict the Horowitz and Manski corrupt sampling bounds. For any v , $P(y = 1)$ must lie within the vertical interval between these diagonal lines. The lower bound is uninformative for $v \leq 1 - P(y^* = 1)$, while the upper bound is uninformative for $v \leq P(y^* = 1)$. Thus, for lifetime use, the bounds are uninformative unless we know that over 50% of responses are valid; for past year use, the lower bound is uninformative unless we know that over 30% of responses are valid. What is most striking about these corrupt sampling bounds is that even with fairly small degrees of reporting error, there is much ambiguity in the prevalence rate of marijuana use. If, for example, we know that only 10% of all reports may be misclassified, then the lifetime use rate can only be restricted to lie within a 20 point range: [0.44, 0.64]. Without richer information on the nature and degree of reporting errors in the NSDUH, the only way to draw tighter inferences is to impose additional assumptions.

It is tempting to address the data error problem with strong, but possibly flawed, modeling assumptions. The contaminated sampling models, for example, substantially narrow the corrupt sampling bounds (see Figs. 18.1 and 18.2). As discussed earlier, however, these models are untenable in this setting. In this case, stronger assumptions do not resolve the ambiguity reflected in the corrupt sampling bounds – they simply replaced uncertainty over data errors with uncertainty over the model.

There may be other assumptions that can be credibly applied. [Pepper \(2001\)](#), for example, assumes the fraction of false negatives exceeds the fraction of false positives, in which case the reported lifetime rate of 0.54 serves as a lower bound for all v . Thus, if no more than 10% of respondents are misclassified, the lifetime prevalence rate is bounded to lie with [0.54, 0.64]. [Kreider and Pepper \(forthcoming\)](#) formalize the assumption that the rate of illicit drug use is higher among inaccurate reporters. While these assumption narrow the bounds, there remains much uncertainty about the rates of use and, as shown in [Pepper \(2001\)](#), identifying trends in use can be even more problematic.

ILLUSTRATION 4: THE AGGRAVATED ASSAULT RATE. Mixing models of measurement error are also a natural way to address imputation errors in UCR reported crime figures. While most police agencies provide UCR data to the FBI, [Maltz and Targonski \(2002\)](#) and [Lynch and Jarvis \(2008\)](#) find that a nontrivial and nonrandom portion of UCR data are imputed. Consider, for example, the problem of inferring the 2005 aggravated assault rate in the United States from the rate of 0.0029 in the UCR (see Table 18.1).¹⁶ Without additional information, the corrupt sampling bounds reveal that violent crime rates lie between zero and the imputation rate. So, for example, if crime rates are imputed for 5% of the population, then the 2005 aggravated assault rate is only known to lie between [0, 5%]. Information identifying which records are imputed may narrow these bounds, but not by an appreciable amount.

By contrast, information on the direction of the imputation bias might be informative. [Maltz and Targonski \(2002\)](#) argue, for example, that nonresponse is likely to occur in periods where there is little reported crime. If so, this would lead to an upward imputation bias, so that the observed rate of 0.0029 serves as an upper bound on the 2005 aggravated assault rate. Unfortunately, this assumption seems unsubstantiated. Arguably, some police departments do

¹⁶In this discussion, we are concerned with drawing inferences on the true rate of aggravated assault reported to the police. Inferences regarding the overall rate of aggravated assault – known and unknown to the police – are complicated by the proxy variables problem discussed previously.

not respond (or under-report) in periods where crime rates are inflated, in which case there would be a downward imputation bias (Mosher et al. 2002).

Maltz and Targonski (2002) have argued that imputation errors may bias conclusions drawn from analyses conducted at fine geographic levels, such as counties, but will be less important for inferences about state and national-level crime rates. The data alone do not support this conclusion. Unless one is willing to make strong and seemingly unsubstantiated assumptions, even small amounts of imputation errors can lead to substantial uncertainties about true crime rates and trends.

CONCLUSION: THE LAW OF DECREASING CREDIBILITY

Measurement errors continue to frustrate attempts to draw credible inferences from data used to track the extent and expression of crime in the United States. Lack of detailed information on the degree and nature of measurement errors in major national crime datasets, namely the UCR and NCVS, is especially troubling. In the absence of direct information on these errors, inferences on crime rates and trends, and on the impact of policy on crime, are largely speculative. Although it might be – as some have suggested – that misreporting rates are stable over time and unrelated to policies of interest, this conjecture seems implausible and is unsupported by evidence. Either way, measurement errors are likely to be both substantial and systematic in survey data on crime and illicit behavior.

Though these problems do not imply that the data are completely uninformative, they do imply that researchers must choose between the unpleasant alternatives of either tolerating a certain degree of ambiguity in inference, or imposing strong assumptions about unobserved measurement errors. The problem, of course, is that weak assumptions may lead to indeterminate conclusions, whereas strong assumptions may be inaccurate and yield flawed conclusions (Pepper 2001; Manski 2007; Manski et al. 2002). Manski (2007) refers to this fundamental trade-off as the *Law of Decreasing Credibility*: stronger assumptions yield sharper but less credible inferences.

This trade-off should not be easily dismissed. Imposing convenient assumptions does not resolve the measurement error problem, but simply exchanges uncertainty over unobserved errors with uncertainty over the accuracy of the model. Assumptions that data errors are exogenous or “classical,” for example, are in many applications untenable. As we have noted in this chapter, relaxing the central assumptions of the classical errors-in-variable model has substantive implications for the conclusions we might draw from the data. Inferences are highly sensitive to even small amounts of measurement and modeling errors.

There are practical solutions to this predicament. If stronger assumptions are not imposed, the way to resolve an indeterminate finding is to collect richer data. More detailed information on the nature of data error problems might supplement the existing data and help to suggest credible assumptions about error processes. Alternatively, efforts to increase the valid response rate may directly reduce the potential effects of these problems. Even with the best survey sampling methods, however, researchers must confront the fact that data on such sensitive topics as crime and victimization will always be subject to poorly behaved measurement errors, and inferences drawn using these data will be impacted by such errors. Failure to seriously address data error problems can only lead to decreased credibility and potentially costly mistakes in drawing inferences relevant to crime policy.

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