A METHODOLOGICAL CRITIQUE OF THE LINZ-YAO REPORT: REPORT TO THE CITY OF TOLEDO, OH

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May 15, 2004

1. Introduction

Analyzing calls-for-service (CFSs) to the Toledo Police Department, plaintiffs' experts, Daniel Linz and Mike Yao report that:

> ... we found no evidence of negative effects in the form of crime including sex crimes, stemming from the adult cabarets or video/bookstores in Toledo. The evidence collected here shows that adult businesses in Toledo do not produce crime effects. Failure to find a greater occurrence of secondary effects for adult businesses is as serious challenge to the assumption that these businesses engender adverse secondary effects.[1]

A reanalysis of the Linz-Yao data, reported here, leads to the opposite conclusion. Subjected to a rigorous statistical analysis, the data demonstrate that, like sexually-oriented businesses (SOBs) in virtually every other U.S. city, Toledo's SOBs have large, significant crime-related secondary effects. Toledo's crime-related secondary effects involve the same sorts of crimes found in other cities, moreover, and apply generally to all of the SOB subclasses defined in Toledo Ordinance No. 2-03: adult bookstores, video arcades, dance clubs, *etc.*

Readers who lack statistical backgrounds may wonder how two teams of experts can analyze the same data with the same methods but, yet, arrive at radically different conclusions. Simply put, the stark differences between the original Linz-Yao analyses and our reanalysis, reported here, are due to (1) differences in the underlying statistical assumptions; and (2) differences in the interpretation of analytic results. Linz and Yao based their statistical analysis on wildly implausible assumptions, in our opinion, and then compounded the error through a misinterpretation of their results.

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[1]P. 47 of *Evaluating Potential Secondary Effects of Adult Cabarets and Video/Bookstores in Toledo, Ohio: A Study of Calls for Service to the Police.* Daniel Linz, Ph.D. and Mike Yao, February 15, 2004. Hereafter, we call this report as the "Linz-Yao Report" or "Linz and Yao."

We consider these two issues separately.

(1) *Statistical assumptions*. Every statistical analysis is predicated on a set of assumptions that, taken together, constitute a "model." If one or more of the predicate assumptions is unwarranted, the model will yield analytic results that are biased in some way. The consequences of this bias can be benign. Results predicated on "wrong" assumptions can still be approximately "right." But the consequences of bias are not always benign. In many instances, the accrued bias violated assumptions can have disastrous consequences.

(2) *Interpretation of statistical results*. Model assumptions not withstanding, the results of every statistical analysis must be interpreted. Except for results derived from randomized controlled trials (experiments), analytic results cannot be expressed as a single number. Results derived from quasi-experimental designs, as in this present instance, invariably consist of several numbers which must be integrated. This opens the door to subjectivity. Focusing exclusively on only one of several numerical results can lead to a misinterpretation of the larger set of results.

1.1 What Linz and Yao actually found

At best, the claim by Linz and Yao that they could find *no* "evidence of negative effects" is an exaggeration. Indeed, categorical descriptions of statistical results are almost always false (or trivial). Although we would dispute their claim, Linz and Yao would be on firmer ground had they admitted to finding *some* "evidence of negative effects" but, then, to claim that the evidence was *slim* or *fragile*.

The distinction between *no* evidence and *slim* evidence introduces the concept of effect size or magnitude. Was the secondary effect large, in other words, or small? A secondary effect can be large (or small) in either of two ways. It can be *substantively* large (or small) or it can be *statistically* large. Ideally, a substantively large (or small) effect will also be statistically large (or small) and vice versa. This convergence is not always guaranteed, unfortunately.

1.1.1 Substantive magnitude

In purely substantive terms, Linz and Yao found *strong* "evidence of negative effects." The effects ranged in size from 63 percent (for "serious" crime) to 11 percent (for drug crime). The effects were uniformly *adverse* and were largely indistinguishable from the effects reported in other studies. Finally, the effects reported by Linz and Yao apply generally to all of the SOB subclasses defined in Toledo Ordinance No. 2-03: adult bookstores, video arcades, dance clubs, *etc.*

The Linz-Yao statistical model compared five crime categories (measured by CFSs) across three neighborhood categories.   The five Linz-Yao crime categories include

- Personal crimes: assault, homicide, rape, robbery.
- Property crimes: arson, auto theft, burglary, larceny.
- Sexual crimes: prostitution, public lewdness, *etc.*
- Drug crimes: simple use, sale, *etc.*
- Other minor crimes: disorderly conduct, disturbing the peace, *etc.*

To these five categories, we added a sixth. The category of "serious" crimes is defined as the sum of personal and property crimes. The three neighborhood categories used by Linz and Yao include neighborhoods with one or more SOBs, neighborhoods with one or more taverns, and "control" neighborhoods that have neither SOBs nor taverns.

FIGURE 1.1.1 - Estimated Secondary Effects from the Linz-Yao Report

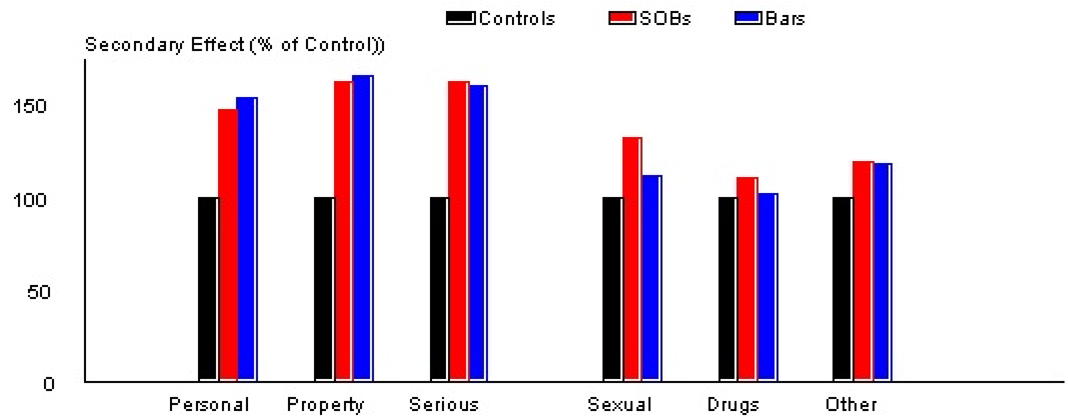

FIGURE 1.1.1 plots the secondary effect estimates reported by Linz and Yao.[2] The secondary effects Control neighborhoods (represented by black bars) are scaled to 100 so that the estimates for SOBs (represented by red bars) and taverns (represented by blue bars) can be interpreted as multiples of the controls. To illustrate, for "serious" crime, the risks in SOB and tavern neighborhoods is approximately 60 greater than in an "average" Toledo neighborhood.

The secondary effect estimates for taverns illustrate the concept of substantive effect size. To say that an effect estimate may be "twice as large as its standard error" says nothing about practical importance. To say that SOBs and taverns have roughly equivalent crime-related secondary effects, on the other hand, conveys an important piece of practical information to planners and police agencies. The ambient crime risk of taverns is so well known and so widely

[2] Linz and Yao assumed that every SOB subclass would have a unique secondary effect and that the effect would be inversely proportional to the number of subclass-specific SOBs in a neighborhood. In a later section, we tested these assumptions and find them unwarranted. The effects summarized in FIGURE 1.1.1 relax both assumptions.

accepted that taverns are a substantive "gold standard" of secondary effects research.[3] A claim that taverns are no riskier than other sites would be met with great skepticism. The fact that Toledo's SOBs and taverns have roughly equivalent crime-related secondary effects, then, is *strong* "evidence of negative effects" for SOBs.

1.1.2 Statistical magnitude

The failure of Linz and Yao to find statistical "evidence of negative effects" around Toledo's SOBs is, to a large degree, an artifact of unwarranted (and unrealistic) statistical assumptions. But a more realistic set of statistical assumptions would not necessarily have lead Linz and Yao to a more realistic conclusion. Whatever the statistical assumptions, analytic results must be *interpreted*. Linz and Yao interpreted their results so as to ignore substantive magnitude in favor of statistical magnitude.

As one might guess, due largely to the unrealistic assumptions of the model, the effect estimates reported by Linz and Yao are modest compared to their substantive magnitudes. We will address this issue in the next section. For present purposes, the Linz-Yao effect estimates summarized in FIGURE 1.1.1 are reported in numerical form in TABLE 1.1.2. Each row of this table corresponds to a crime category: property, personal, "serious" (defined as the sum of property and personal), sexual, drug, and other minor crimes. The columns labeled "***B***" are log-

[3] The best known work in this area was done by our colleague, Dennis W. Roncek. See, *e.g.*, Roncek and R. Bell. Bars, blocks, and crime. *Journal of Environmental Systems*, 1981, 11:35-46; Roncek and M.A. Pravatiner. Additional evidence that taverns enhance nearby crime. *Social Science Research*, 1989, 73:185-188; Roncek and P. Maier. Bars, blocks, and crime revisited. *Criminology*, 1991, 29:719-753. For a treatment of the phenomenon from a public health perspective, see R. Scribner, D. Cohen, S. Kaplan, and S. Allen. Alcohol availability and homicide in New Orleans: conceptual considerations for small area analysis of the effect of alcohol density. *American Journal of Public Health*, 1999, 98:243-251.

scale secondary effect estimates. For technical reasons, Linz and Yao analyzed *logarithms*. To facilitate interpretation of these numbers, the columns labeled "***exp(B)***" report the secondary effect estimates in a more natural metric that was used in FIGURE 1.1.1.

TABLE 1.1.2 - Estimated Secondary Effects from the Linz-Yao Report

	Any SOB			**Any Bar or Tavern**		
Crime-CFSs	***B***	***exp(B)***	**α**	***B***	***exp(B)***	**α**
Log Violent	**.389**	**1.48**	**.10**	**.510**	**1.66**	**.00**
Log Property	**.488**	**1.63**	**.05**	**.434**	**1.54**	**.00**
Log Sexual	**.285**	**1.33**	**.17**	**.112**	**1.12**	**.18**
Log Drug	**.107**	**1.11**	**.36**	**.019**	**1.02**	**.73**
Log Other Minor	**.183**	**1.20**	**.06**	**.164**	**1.18**	**.00**

The column of TABLE 1.1.2 labeled "α" gives the statistical magnitude of the secondary effect estimate. By convention, values of α less than or equal to .05 are statistically large (or significant) while values larger than .05 are statistically small (or insignificant). By this convention, one of the secondary effects estimates for SOBs is large, five are small. For taverns, on the other hand, four secondary effect estimates are large, two are small. Although Linz and Yao interpret this result as "no evidence of negative effects," as we will now demonstrate, the result is an artifact of the unrealistic assumptions of their statistical model.

1.2 What we found

A superficial reading of the Linz-Yao Report left us wondering at the disparity between the substantive and statistical sizes of their secondary effect estimates. How could substantively large effect be so statistically small? A superficial reanalysis of their data answered our questions. Any reasonable statistical model returned parameter estimates that were large or significant in every sense of the word.

Table 1.2 - Estimated Secondary Effects from the Linz-Yao Report

	Any SOB			Any Tavern			
Crime-CFSs	***B***	***exp(B)***	**α**	***B***	***exp(B)***	**α**	R^2
Log Violent	**.363**	**1.44**	**.06**	**.423**	**1.53**	**.00**	**.75**
Log Property	**.568**	**1.76**	**.01**	**.351**	**1.42**	**.00**	**.60**
Log Sexual	**.513**	**1.67**	**.01**	**.055**	**1.06**	**.48**	**.57**
Log Drug	**.258**	**1.29**	**.24**	**-.086**	**0.92**	**.33**	**.49**
Log Other Minor	**.385**	**1.47**	**.04**	**.282**	**1.33**	**.00**	**.70**

One such set of parameter estimates is reported in Table 1.2. In terms of substantive magnitude, the effect estimates for SOBs grow larger in this model while the estimates for taverns grow smaller. In statistical terms, three of five of the SOB effect estimates are now large, one is small, and one is on the cusp of largeness. On the basis of these estimates, one must conclude that there is substantial "evidence of negative effects" in Toledo.

The estimates reported in Table 1.2 are *not* our final estimates. The "evidence of negative effects" is stronger even than these numbers suggest. Before developing this argument and presenting refined results, however, we should describe the model on which these secondary effect estimates rest. Since the technical details will be of little interest to many readers, our model is fully described in an appendix. In what follows here, the major differences are described in a non-technical manner with emphasis on the differences between our model and the Linz-Yao model.

Each of the major differences between our statistical model and the Linz-Yao model reflects a difference in statistical assumptions about secondary effects. Three of the assumptions made my Linz and Yao relate to non-SOB causes of crime risk. These include:

- A neighborhood's **area** must cause its crime risk;
- A neighborhood's crime **rate** does not measure its crime risk;
- Different types of crime are **independent**.

Two other assumptions made by Linz and Yao relate to characteristics of SOBs cause crime risk. These include:

- Different SOB **subclasses** must have different secondary effects;
- A secondary effect must be **proportional** to the number of SOBs;

None of these five assumptions is warranted by the relevant theory or by the data. All are arbitrary in that sense and, not surprisingly, all bias the statistical result in favor of a null finding.

1.2.1 Non-SOB sources of crime risk

One of the most idiosyncratic statistical assumptions made by Linz and Yao is that the *areal size* of a neighborhood must *cause* of its crime risk; *i.e.*, large (or small) neighborhoods must have high (or low) crime risks. This assumption has no theoretical precedent in the criminological literature, however, or any support by the data.Report's methodology fails to meet the standards of scientific rigor mandated by *Daubert*.[4] Accordingly The raw correlations between area and crime counts in the Linz-Yao model range from small and negative, implying that large neighborhoods have *high* risks, to small and positive, implying that large neighborhoods have *low* risks. This apparent contradiction is resolved by noting that these

[4] The lack of precedent in the criminological literature is relevant under Daubert v Merrell Dow Pharmaceuticals (509 US 579 [1993]). One of the four Daubert criteria is asks whether the method, a statistical model in this instance, is widely accepted in the scientific community. The Linz-Yao statistical model is so idiosyncratic that it must fail this test.

correlations are so trivially small[5] that they deserve no representation in a statistical model.

The Linz-Yao assumption about crime *rates* is equally idiosyncratic and no less problematic. The convention in the criminological literature is to control for the size of the at-risk population by using crime *rates*. The simplest and most widely used crime rate is the ratio of total crimes to population; and to explain the variance in crime rates by a regression on analogously defined rates of poverty, disorganization, *etc.* Ignoring this ubiquitous convention, the Linz-Yao statistical model is so idiosyncratic as to render its results non-comparable to other results in the criminological literature.

The third idiosyncratic assumption made by Linz and Yao is that the five crime categories are *independent*; *e.g.*, that neighborhoods with high property crime risks do not necessarily have high violent risks. Once again, this assumption has no precedent in the criminological literature and is soundly refuted by the data. In fact, the zero-order correlation between property and violent crimes in Toledo's 347 neighborhoods is .91. The correlations among the all five crime categories are uniformly positive, so Toledo neighborhoods with high risks for one type of crime are likely to have high risks for the other four types.

There are several common themes here. Each of these three Linz-Yao model assumptions is unprecedented in the criminological literature; and each is refuted by the data. Accordingly, our model relaxes these assumptions. As a consequence, we arrive at a more parsimonious, more powerful statistical model. The immediate results are shown in TABLE 1.2.

1.2.2 SOB characteristics as sources of crime risk

The statistical assumptions related to non-SOB sources of crime risk are obvious to any

[5] Zero-order correlations for 347 Census Block Groups range from -.18 to +.16.

criminologist. The statistical assumptions related to SOB characteristics, on the other hand, are obvious only to criminologists who have experience with crime-related secondary effects research. Although these assumptions seem innocuous – even reasonable – on their face, they set out novel statistical criteria that an analysis must satisfy before one can conclude that SOBs pose a public safety hazard. Because Linz and Yao never make these novel statistic criteria explicit, moreover, they amount to a deception.

The first novel assumption is that different SOB classes must have different secondary effects.[6] In purely statistical terms, analytic confidence and/or power require that the number of SOB neighborhoods be "large" relative to the number of control neighborhoods. However many SOB neighborhoods there might be, assuming that each SOB subclass has a unique effect reduces the number by a factor of three. Since there potentially many SOB subclasses, this assumption poses a nearly insurmountable obstacle to the analysis; it biases the result in favor of a null finding.

Here again, the assumption has is no precedent either in criminological theory or in the secondary effects literature. The criminological theory of secondary effects, to be described at a later point, does not provide for subclass-specific effects. Other than studies conducted by Dr. Linz and his associates, moreover, there are no secondary effect studies that incorporate this assumption into a statistical model. Finally, of course, the data refute this assumption.

The second novel assumption made by Linz and Yao is that the ambient crime risk in a neighborhood will be proportional to the number of SOBs in the neighborhood. Although this

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[6] Stated as a logical implication, this amounts to "If SOBs have secondary effects, then each SOB subclass will have a unique secondary effect."

assumption may seem reasonable, it is predicated on a *unit property* that SOBs do not have. In terms of permits or licenses, one can indeed "count" the number of SOBs in a neighborhood. In terms of ambient crime risk, on the other hand, a simple "count" of SOBs will lead to an invalid inference. An SOB that draws many patrons into a neighborhood, e.g., will pose a larger risk than, say, two SOBs that draw only a few patrons into the neighborhood.

When the unit property assumption is warranted, it yields a substantial return in statistical confidence and power. When the assumption is unwarranted, on the other hand, it biases the analysis in favor of a null finding. In this particular instance, the data refute the unit property assumption. There is no reason to believe, then, that "all SOBs are created equal" – at least with respect to their secondary effects.

2. How Linz and Yao were able to miss the evidence

A critical reading of the Linz-Yao left us two strong impression. First, although their Report obscured the fact, Linz and Yao had found substantively large crime-related secondary effects for Toledo's SOBs. How large? Reading from the first two rows of TABLE 1.1.2, they found that, after controlling for all other causes of crime, "serious" crime in SOB neighborhoods was roughly 50 percent higher than expected.

Second, however, no matter how *substantively* large the effects might be, Linz and Yao found that they were statistically *small*. Linz and Yao argued, fallaciously, that the statistical size of the estimates meant that Toledo's SOBs had *no* crime-related secondary effects.[7] We disagree

[7] A statistician and a lawyer went to lunch. The lawyer ordered a garden salad and a diet coke. The statistician ordered soup, salad, entree, and wine. When the meal was finished, the statistician suggested that each pay half of the $150 check. The lawyer protested, pointing out that the statistician's meal accounted for $140 of the total. The statistician patiently explained that the difference between $75 and $10 was not statistically significant.

with both the premise and the conclusion of this argument. In our opinion, the modest statistical size of the secondary effects reported by Linz and Yao can be traced to idiosyncracies of their statistical model and study design. In addition to the statistical assumptions already discussed, these include:

- The use of calls-for-service (CFSs) to measure crime risk;
- The use of an over-parameterized model;
- Fishing.

Each of these idiosyncratic design feature represents a radical departure from the conventions of the literature and, more important, each biases the analysis in favor of the null hypothesis.

2.1 Linz and Yao use calls-for-service (CFSs) to measure crime risk

Criminologists use crime *incidents* (or "crimes known to the police") to measure crime risk. Given this well established convention, the decision by Linz and Yao to use CFSs begs justification:

> We employ calls for service in this study for four reasons: 1) The use of these indicators of crime is compatible with criminology research; 2) Studies of secondary effects relied on by the City of Toledo have also employed this measure. It is possible, therefore, to directly compare the findings of the present study to these studies; 3) CFS are known to be consistent with victimization data; 4) The Justice Department endorses their use as indicators of criminal activity.[8]

But in fact, 1) criminologists *never* use CFSs to measure crime risk; 2) *very few* of the secondary effect studies relied on by the City used CFSs for any purpose whatsoever; 3) CFSs are *not* consistent with victimization data; and 4) the U.S. Department of Justice has *never* endorsed the use of CFSs as a measure of "criminal activity." Lacking details, the third and fourth rationales

[8] Linz and Yao, p. 16

are apocryphal and cannot be taken seriously. The first and second rationales, on the other hand, are easily refuted.

Excluding studies conducted by Dr. Linz and his colleagues, *very few* secondary effects studies use CFSs for any reason. TABLE 2.1A lists eighteen secondary effect studies that have been (or will be) cited in this case. Twelve use Uniform Crime Reports (UCRs, in blue). Four studies use CFSs exclusively (in red) but, of these, three were written by Dr. Linz and his colleagues. Notably, two studies use both UCRs and CFSs (in green), reflecting the simple fact that, in the opinion of the authors, UCRs and CFSs measure different things.

TABLE 2.1A - Crime-related Secondary Effect Studies

Los Angeles. CA (1976)	**Newport News, VA (1996)**
Whittier, CA (1978)	**Fulton County, GA (1997)**
Phoenix, AZ (1978)	**Dallas (1997)**
St. Paul, MN (1978)	**Fort Wayne, IN (2001)***
Minneapolis, MN (1980)	**Charlotte, NC (2001)***
Indianapolis, IN (1984)	**San Diego, CA (2002)***
Austin, TX (1986)	**Centralia, WA (2003)**
Garden Grove, CA (1991)	**Greensboro, NC (2003)***
Times Square, NY (1994)	**Toledo, OH (2004)***

UCRs **CFSs** **Both**

*** Studies conducted by Dr. Linz and colleagues**

TABLE 2.1B makes the same point for the published criminological literature. The shortcomings of CFSs are so well known and widely accepted that no criminological journal publishes research in which CFSs are used as a general measure of crime risk. During the most recent five-year period, *e.g.*, four national criminology journals, *Criminology*, *Justice Quarterly*, the *Journal of Quantitative Criminology*, and the *Journal of Criminal Justice* published 705 bibliographic items, primarily articles. Most of the articles were either non-empirical (theoretical essays, reviews, *etc.*) or else, analyzed phenomena other than crime (police behavior, sentencing decisions, *etc.*). Of the 254 articles that analyzed a crime statistic, 134 (52.8 percent) analyzed UCRs; 119 (46.8 percent) analyzed victim or offender surveys. Only five articles (1.9 percent) analyzed CFSs. These data reflect the consensus view among criminologists that CFSs are not the *best* – or even a *good* – measure of crime.[9]

TABLE 2.1B - Crime Statistics Used in National Criminological Journals, 2000-2004[10]

	Total Articles	w/Crime Statistics	UCR	Survey	CFS
Criminology	193	52	**37**	16	**0**
Justice Quarterly	152	48	**23**	23	**2**
J of Quantitative Criminology	95	47	**30**	17	**0**
J of Criminal Justice	265	107	**44**	63	**3**
	(705)	(254)	**(134)**	(119)	**(5)**

Since CFSs are rarely used for *any* purpose in either the unpublished secondary effects literature or the published criminology literature, one might wonder *why* Linz and Yao insist on using CFSs. The answer is that CFSs bias the outcome of a statistical hypothesis test in favor of a null finding. Put another way, CFSs effectively reduce the statistical size of a secondary effect estimate.

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[9] The best known statement of the consensus view is "Measurement errors in calls-for-service as an indicator or crime" by D. Klinger and G.S. Bridges, Criminology, 1997, 35:529-41.

[10] The data reported in TABLE 2.1B were compiled by eight students. Inter-rater reliability among the eight was nearly .95. Because some of the 254 articles analyzed multiple statistics, the rows may sum to more than 100 percent.

ore important, each biases the analysis in favor of the null hypothesis.

2.1.1 How CFSs bias the analysis in favor of a null finding

Although Linz and Yao use terms like "criminal activity," "crime incidents" and "crime events"[11] to describe their results, in fact, CFSs are not synonymous with crime. CFSs and crime are correlated, of course, because large cities (New York, Chicago, *etc.*) have more CFSs *and* more crime than Toledo. But the correlation is weak.

FIGURE 2.1 - Relationship between Crimes and Calls-for-Service (CFSs)

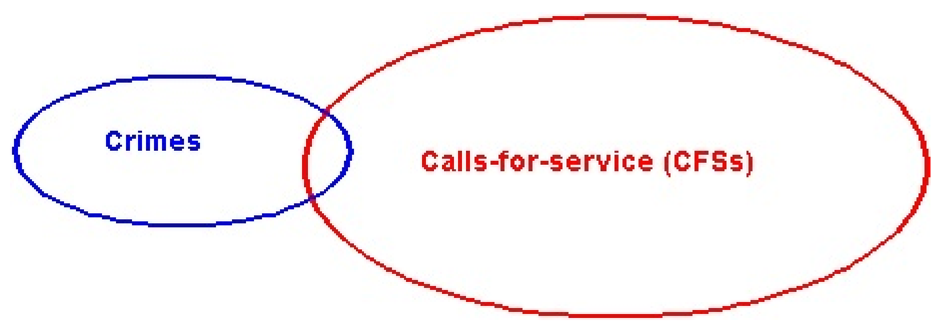

FIGURE 2.1 illustrates the statistical facts. In Toledo, CFSs to the police (in red) outnumber crimes known to the police (in blue) by a large factor. Not all CFSs are associated with crimes, moreover, and not all crimes are associated with CFSs. On the contrary, most CFSs to the Toledo Police Department are not initiated by crimes (or crime-like events); most 911 calls are duplicates, unfounded, groundless, or (in the case of burglary and robbery) precipitated by false alarms. Likewise, most of the crimes known to the Toledo Police Department were not initiated by 911 calls from victims and/or witnesses; most were discovered through routine

[11] Linz-Yao Report, p.2

patrolling; through directed (or proactive) patrolling; and through specialized unit activity.[12]

2.1.2 Correcting for the bias associated with CFSs

One reason why Linz and Yao might prefer to measure crime risk with CFSs is that the overwhelming majority of vice crimes are not initiated by 911 calls; CFSs underestimate crimes like prostitution by a large factor. Another reason why Linz and Yao might prefer CFSs is that the CFS system can be manipulated make a business site look more or less hazardous. But the most important reason why Linz and Yao might prefer CFSs is that these data bias the statistical analysis in favor of a null finding.

Although the bias attributable to the use CFSs has many consequences, the most relevant is its impact on the statistical size of a secondary effect estimate. When CFSs are used as an indicator of crime, as they are in the Linz-Yao analyses, the standard error of crime is related to the standard error of its indicator the equation,

$$SE(Crime) = \sqrt{1-\rho^2}\,SE(CFS)$$

where *SE(CFS)* and *SE(Crime)* are measures of statistical size and where ρ is a reliability (or correlation) coefficient. The value of ρ can be used to correct the statistical size of the Linz-Yao secondary effect estimates. Although we do not know the exact value of ρ is not known, we can use values observed in another city to approximate the Toledo reliabilities. The results are reported in Table 2.1.2.

12 Rather than reiterate here what we have written elsewhere, readers who need details about the CFS-crime nexus are directed to our San Diego report, *A Methodical Critique of the Linz-Paul Report: A Report to the San Diego City Attorney's Office.* March 12, 2003. See also, "Uniform Crime Reports as organizational outcomes." (*Social Problems*, 1982, 29:361-372.).

Table 2.1.2 - Estimated Secondary Effects from the Linz-Yao Report

Crime-CFS Category	*B*	*t*	α	*1*-ρ^2	*t*	α
Log Violent	.363	1.88	.06	.803	2.34	.01
Log Property	.568	2.63	.01	.540	4.90	.00

APPENDIX A - THE MULTIPLE REGRESSION MODEL

$$\text{Log Crimes}_{k} = \beta_0 + \beta_1 X_{1k} + \ldots + \beta_{12} X_{13k} + \gamma_1 Z_{1k} + \ldots + \gamma_4 Z_{4k} + U_{k}$$

		Mean	S.D.
X_1	Area	0.26	0.27
X_2	Population	965.22	458.91
X_3	N of non-whites	227.89	288.26
X_4	N of female head-of-households w/o husbands	66.11	44.80
X_5	N of married households	155.76	98.66
X_6	Median age	34.14	7.84
X_7	Household median income (thousands)	34.40	15.68
X_8	Median value of owner-occupied housing (thousands)	73.19	41.66
X_9	N of families below poverty level	32.62	35.65
X_{10}	N of adults with no high school diploma	28.16	26.33
X_{11}	N of adults with at least a bachelor's degree	72.31	79.78
X_{12}	N of vacant housing units	32.68	27.76
X_{13}	N of owner-occupied housing units	241.60	134.19
Z_1	N of Alcohol serving clubs	0.77	1.43
Z_2	N of adult cabarets	0.02	0.14
Z_3	N of adult book/video stores	0.03	0.19
Z_4	N of other SOBs	0.02	0.23

$$\text{Log Crime Rate}_{k} = \beta_0 + \beta_3 X_{3k} + \ldots + \beta_{13} X_{13k} + \gamma_1 Z_{1k} + \gamma_2 Z_{2k} + U_{k}$$

X_1	Area	———	———
X_2	Population	———	———
X_3	PC non-whites	247.556	302.33
X_4	PC female head-of-households w/o husbands	9.43	37.28
X_5	PC married households	156.85	61.72
X_6	Median age	34.14	7.84
X_7	Household median income (thousands)	34.40	15.68
X_8	Median value of owner-occupied housing (thousands)	73.19	41.66
X_9	PC families below poverty level	34.57	32.97
X_{10}	PC adults with no high school diploma	30.63	28.88
X_{11}	PC adults with at least a bachelor's degree	68.73	59.66
X_{12}	PC vacant housing units	36.34	36.81
X_{13}	PC owner-occupied housing units	251.46	99.80
Z_1	Alcohol serving club?		
Z_2	SOB?		