A METHODOLOGICAL CRITIQUE OF THE LINZ-YAO REPORT: REPORT TO THE GREENSBORO CITY ATTORNEY

Richard McCleary, Ph.D.

December 15, 2003

Antonio Ohe, B.A. and Joanne Christopherson, M.A. provided research assistance for this report, including library searches and data management, and analyses.

EXECUTIVE SUMMARY

Analyzing calls-for-service to the Greensboro Police Department between 1999 and 2003, the plaintiffs' experts, Daniel Linz and Mike Yao, conclude:

> "... that there is no support for the City of Greensboro's theory that adult businesses produce adverse secondary effects. The results of our study show that adult businesses are not associated with crime events (p. 3)."

The detailed numerical results supporting this conclusion are scattered over 18 pages of computer output in an appendix of the Linz-Yao Report. When the actual numbers are examined, however, it is clear that Linz and Yao overstated the empirical basis of their strongly-worded conclusion. Put simply, their numbers contradict their words.

Table 1: The Linz-Yao Secondary Effect Estimates

	Controls	Books/Videos		Cabarets	
Crimes against persons	180.1	386.0	146.7%	258.3	143.4%
Crimes against property	1557.6	2455.3	157.6%	2028.7	130.2%
Drug-related crimes	84.7	112.1	132.3%	119.1	140.6%
Sex-related crimes	19.4	27.0	139.1%	29.3	151.0%
Disorderly conduct	121.1	181.3	149.7%	164.9	136.2%
Other minor crimes	596.3	1191.2	199.8%	878.2	147.3%

Table 1 summarizes the Linz-Yao secondary effect estimates. Each row of Table 1 (in green) corresponds to one of six crime-categories. The three shaded groups of columns in Table 1 report the estimated numbers of crimes for three neighborhood-types: those with no adult-oriented businesses ("Controls" in blue); those with adult-oriented bookstores or video arcades ("Books/Videos" in red), and those with adult-oriented cabarets ("Cabarets" in red). Percentages to the right of an effect expresses the estimated secondary effect as a proportion of the control mean; percentages larger than 100 imply adverse secondary effects. Contrary to their strongly-worded conclusion, Table 1 reveals that *the results reported by Linz and Yao amount to a consistent pattern of adverse secondary effects*.

After correcting for the effects of thirteen neighborhood-level crime risk factors, *e.g.*, Linz and Yao find that, compared to neighborhoods with no adult-oriented businesses, neighborhoods with adult-oriented bookstores and video arcades had, on average, 46.7 percent more crimes against persons (assault, homicide, robbery, and rape); 57.6 percent more property crimes (arson, auto theft, burglary, and theft); 32.3 percent more drug crimes; 39.1 percent more sex crimes; 49.7 percent more disorder crimes; and 99.8 percent more other minor crimes. Secondary effects estimates for neighborhoods with adult-oriented cabarets are similar.

Although the large adverse secondary effects summarized in Table 1 seem to contradict their conclusion, Linz and Yao are able to resolve the apparent contradiction with formal hypothesis tests. Only two of the effect estimates in Table 1 are statistically significant at the .05 level; ten estimates are not statistically significant and, thus, in the opinion of Linz and Yao, *not different than zero*. The two significant effect estimates, in their opinion, are aberrations, not to be trusted. Since twelve statistical analyses yield effect estimates that are either aberrant (in two cases) or not different than zero (in ten cases), Linz and Yao feel confident in their conclusion that "... adult businesses are not associated with crime events." This logic is flawed in two respects, however.

First, the outcome of a hypothesis test is sensitive to the elements of the quasi-experimental design. The Linz-Yao design is idiosyncratic in many respects, even compared to their prior work. Beginning with the crime indicator (calls-for-service) and ending with the statistical model (six independent multiple regressions), all key elements of the Linz-Yao design favor a null finding. The fact that large adverse secondary effect estimates persist in the presence of so many methodological challenges demonstrates the true strength of the effects.

Second, the several independent hypothesis tests conducted by Linz and Yao ignore the *pattern* of effects. Whereas twelve identically zero effect estimates are expected to yield random runs of small positive and negative numbers, what one sees instead is a run of twelve large, positive numbers. Tested one-by-one, none of the Linz-Yao effect estimates may achieve statistical significance – although two do. But tested jointly, the pattern of effect estimates may be highly significant.

Based on my critical analysis of the Linz-Yao design, including the choice of crime indicators (calls-for-service), choice of impact and control areas (Census Block Groups), choice of statistical model (co-variate adjustment by multiple regression), and choice of hypothesis test (six independent tests), the null finding reported by Linz and Yao underestimates the secondary effects of adult-oriented businesses in Greensboro. The true secondary effect estimates are on the order of those summarized in Table 1 – adverse and substantively large.

Given the constraints of time and resources, an independent study of secondary effects in Greensboro, based on a more conventional design, is unfeasible. Taking the Linz-Yao secondary effect estimates at face value, however, the debate reduces to the issue of statistical significance. If the pattern of effects in Table 1 is significant, the Linz-Yao conclusion is incorrect. In fact, a joint significance test of all six crime categories yields effect estimates that are statistically significant at the .05 level for crimes against persons and property – the so-called "serious" crimes – across both classes of adult-oriented businesses. *Even accepting their weak design, the analyses by Linz and Yao provide convincing evidence that adult-oriented businesses in Greensboro generate adverse secondary effects.*

Aside from conclusions based on analyses of Greensboro calls-for-service, Linz and Yao review the secondary effects literature used by the City in formulating adult-oriented business regulations. They conclude that:

> ... All of the studies that claim to show adverse secondary effects are lacking in methodological rigor. The studies that have been done either by government agencies or by private individuals that have employed the proper methodological rigor have universally concluded that there are no adverse secondary effects (p. 10).

This characterization of the empirical secondary effects literature is overly negative, in my opinion. Whereas some of the studies cited by the City may be weak, in terms of methodological rigor, others are quite strong. Overall, the Greensboro's adult-oriented business regulations are based on a solid empirical foundation.

I. Introduction

Analyzing a subset of calls-for-service (CFSs) made to the Greensboro Police Department (GPD) between January 1st, 1999 and September 30th, 2003, the plaintiffs' expert witnesses, Daniel Linz and Mike Yao, found that:

> ... The presence of adult cabarets and adult video/bookstores in "neighborhoods" was unrelated to sex crimes in the area. We found that several of an adult video/bookstore were located in high person and property crime incident "neighborhoods." We examined the "neighborhoods" and local areas surrounding the adult video/bookstores (1000 foot radius) further and we found that the adult video/bookstores were not the primary source of crime incidents in these locations.[1]

Based on these findings, Linz and Yao conclude

> ... that there is no support for the City of Greensboro's theory that adult businesses produce adverse secondary effects. The results of our study show that adult businesses are not associated with crime events.[2]

Based on my reading of the Linz-Yao Report; on my reading of the literature cited in the Report; on my analyses of their data and of Uniform Crime Report (UCR) data obtained from the GPD, and on my experience in this field, it is my opinion that the Linz-Yao Report's methodology fails to meet the normally accepted standards of scientific rigor for to meet normally accepted standards for statistical analyses.

In addition to conclusions drawn from empirical findings, Linz and Yao argue that the empirical secondary effects literature consists entirely of studies that find no adverse

---

[1] This quotation is found on p. 3 (counting the title sheet as p. 1) of *Evaluating Potential Secondary Effects of Adult Cabarets and Video/Bookstores in Greensboro: A Study of Calls for Service to the Police* by Daniel Linz, Ph.D. and Mike Yao, November 30th, 2003. In the text, I call this "the Linz-Yao Report," or "Linz and Yao." Professor Daniel Linz, the first author of the Linz-Yao Report, has written secondary effect reports with several co-authors. I will use "Linz *et al.*" to refer to reports written with co-authors other than Mike Yao.

[2] Linz and Yao, p. 3.

secondary effects and studies that are too flawed to be taken seriously:

> ... All of the studies that claim to show adverse secondary effects are lacking in methodological rigor. The studies that have been done either by government agencies or by private individuals that have employed the proper methodological rigor have universally concluded that there are no adverse secondary effects.[3]

Based on the perceived consistency of the secondary effects findings, Linz and Yao conclude that the factual predicate for Greensboro Ordinance Chapter 30 is invalid. But in fact, the methodological rigor of secondary effects studies ranges from strong to weak. One study cited by the City used the most rigorous possible design and found substantively large, statistically significant adverse secondary effects.[4] In my opinion, there is an ample factual predicate for Greensboro Ordinance Chapter 30.

To support their contrary argument, Linz and Yao cite two studies by Linz *et al.* that find *salutary* secondary effects:

> Recently, we have conducted independent, reliable, studies using census data and modern analytical techniques to examine whether "adult" entertainment facilities, and particularly exotic dance establishments engender negative secondary effects. Unlike many of the previous reports, these studies do not suffer from the basic methodological flaws that were enumerated in *Paul*. Unfortunately, the City Council of Greensboro did not consider these investigations despite the fact that the reports were available.
>
> These reports describe analyses of calls for service to the police in the City of Fort Wayne, Indiana, and Charlotte, North Carolina. In these studies there is no indication that, overall, crime rates are higher in the areas surrounding adult nightclubs. In fact, the data often show the reverse trend whereby crime incidents are lower in the areas surrounding the adult nightclubs compared to

---

[3] Linz and Yao, p. 10.

[4] This is the 1991 Garden Grove, CA study written by me and James W. Meeker: *Final Report to the City of Garden Grove: The Relationship between Crime and Adult Business Operations on Garden Grove Boulevard.*

control locations.[5]

The anomalous findings of *salutary* secondary effects in Fort Wayne and Charlotte reflect many of the same methodological flaws found in the Greensboro analyses. Each of these methodological flaws is sufficient to yield a spurious finding.

I.A What Linz and Yao *Actually* Found

Non-statisticians who read the Linz-Yao Report may miss a relevant fact: *Linz and Yao found substantively large adverse secondary effects associated with adult-oriented businesses (AOBs) in Greensboro.* This fact is easy to miss because it is buried in eighteen pages of computer output and mentioned in the Report's text only in passing. TABLE I below summarizes the results of the Linz-Yao statistical analyses. In Detail,

- ♦ Shaded columns of TABLE I correspond to the two major AOB-types: Books\Videos and Cabarets;
- ♦ Rows of TABLE I (in green) correspond to six crime categories: Crimes Against Person, Crimes Against Property, Drug-Related Crimes, Sex-Related Crimes, Disorder Types of Offenses, and Other Minor Offenses;
- ♦ Columns labeled "Effect" (in red) report secondary effect estimates for an AOB-type and crime category;
- ♦ Columns labeled "α" (in red) report the α-error rate for each secondary effect estimate.;

[5] Linz and Yao, p. 10.

- Columns labeled "Bars" (in blue) report the ratio of the estimated AOB effect to the estimated effect for bars and taverns.

To illustrate the interpretation of TABLE I, consider Crimes Against Person. Reading across the first row, areas of Greensboro Bookstores/Videos and Cabarets have 205.9 and 78.2 more crimes respectively than areas of Greensboro with no AOBs. With 95 percent confidence, the Bookstores/Videos estimate is statistically significant ($\alpha \leq .01$) but the estimate for Cabarets (α=.11) is not significant.

TABLE I - SUMMARY OF THE LINZ-YAO FINDINGS*

	Bookstores/Videos			Cabarets		
	Effect	α	Bars	Effect	α	Bars
[a] Crimes Against Person	205.9	.01	6.6	78.2	.11	2.5
[b] Crimes Against Property	897.7	.01	2.3	471.1	.10	1.2
[c] Drug Related Crimes	27.4	.76	3.3	34.4	.58	4.1
[d] Sex Related Crimes	7.6	.63	1.2	9.9	.37	1.6
[e] Disorder Types of Offenses	60.2	.23	2.1	43.8	.21	1.5
[f] Other Minor Offenses	594.9	.09	7.2	281.9	.25	3.4

[a] Linz and Yao, Table 14 [b] Linz and Yao, Table 15 [c] Linz and Yao, Table 16
[d] Linz and Yao, Table 17 [e] Linz and Yao, Table 18 [f] Linz and Yao, Table 19
* *cf.*, Executive Summary, Table 1

The effect estimates in TABLE I show that Linz and Yao found adverse secondary effects for all six categories of crime and both types of AOBs. Only two of the twelve effect estimates in TABLE I are statistically significant, however. By convention, an effect estimate is *not statistically significant* (or *not significantly different than zero*) unless its associated probability is smaller than .05 – unless $\alpha \leq .05$, *i.e.* By this convention, the only significant effect estimates are for Crimes Against Person and Crimes Against Property in those areas of Greensboro where Bookstores/Videos are located. The other ten effect estimates in TABLE I are not statistically significant and, thus, presumably not different than zero.

Though *statistically* small, the effect estimates in TABLE I are *substantively* large. How large? The columns labeled "Bars" (in blue) to the right of each α-probability are ratios of the effect for AOBs to the effect for bars or taverns that do not feature adult-oriented entertainment.[6] The adverse secondary effects of AOBs are always larger than the adverse secondary effects of bars – as much as five times larger for some categories of crime. Given the well-researched and widely accepted relationship between bars and crime,[7] no matter how *statistically* small the secondary effect estimates TABLE I may be then, they are *substantively* large.

As it turns out, the substantively large adverse secondary effect estimates in TABLE I are statistically large as well – *i.e.*, statistically significant at the $\alpha \leq .05$ level. Readers who are interested only in this bottom line are directed to TABLE IV.2 where the α-error levels for a simultaneous hypothesis test are reported. To understand how Linz and Yao could have missed this bottom line, however, the reader must understand how the statistical power of a hypothesis test is related to the methodological underlying the

---

[6] In North Carolina, businesses that serve alcoholic beverages are private clubs. None of the bars or taverns in this contrast feature adult entertainment.

[7] See D.W. Roncek and M.A. Pravatiner. Additional evidence that taverns enhance nearby crime. *Social Science Research*, 1989, 73:185-188.

hypothesis test.

I.B Methodological Flaws in the Linz-Yao Report

Substantively large numbers can be made statistically small – though not *vice versa* – by the use of inappropriate or less than optimal methods. In my opinion, this is what happened in Greensboro. The Linz-Yao methodology is idiosyncratic in many key respects and, in every instance, the idiosyncracies have the effect of transforming substantively large effects into statistically small effects. The shortcomings of the Linz-Yao Report span all three elements of scientific methodology, including (1) the measures of public safety collected for the study; (2) the quasi-experimental design used to interpret the analytic results; and (3) the statistical models used to analyze the public safety measures.

(1) Measurement problems. The most serious flaw by far is the use of calls-for-service (CFSs) to measure public safety risk. There is virtually no precedent in the criminology literature for using CFSs to measure crime or crime risk. A review of national criminology journals over the last three years, *e.g.*, finds no published articles where CFSs are used to measure crime risk. Indeed, secondary effects studies cited by Linz and Yao do not use CFSs to measure crime but, rather, following convention, use Uniform Crime Reports (UCRs) to measure public safety risk.[8] Since the Linz-Yao

---

[8] Both the Ft. Wayne study (*Measurement of Negative Secondary Effects Surrounding Exotic Dance Nightclubs in Fort Wayne, Indiana*) and the Charlotte study (*Are Adult Dance Clubs Associated with Increases in Crime in Surrounding Areas? A Secondary Crime Effects Study in Charlotte, North Carolina*) use Uniform Crime Reports (UCRs) to measure crime risk. The confusion of CFSs and UCRs arises because CFSs have been used traditionally in liquor license reviews (see, *e.g.*, *A Study of CFSs to Adult Entertainment Establishments which Serve*

findings and conclusions are couched in terms of "crime events" or "crime incidents," and since CFSs do *not* measure crime, in the worst case, this flaw is sufficient to invalidates *all* of the Report's empirical findings and conclusions. In the best case, the flaw creates a bias in favor of a null finding.

(2) Design problems. The quasi-experimental design used by Linz and Yao in Greensboro, the so-called "static group comparison" design, lacks any before-after contrast. Accordingly, a leading authority on design rates the "static group comparison" as the weakest of all quasi-experiments.[9] Secondary effects studies that compare ambient crime before and after the opening of a new adult-oriented business (AOB) generally yield stronger – more valid – findings. Findings of secondary effects studies based on before-after designs are reviewed at later point. For the present, compared to secondary effect studies based on relatively weak "static group comparisons," the design of the Greensboro study is idiosyncratic in two crucial respects.

The first design idiosyncracy concerns the size of the impact and control areas. In theory, the impact of a criminogenic source – an AOB, *e.g.* – fades exponentially with distance from the source. "Noise" is a good analog. For both noise and crime risk, the farther one moves from the source, the weaker the sound. To accommodate this

---

Alcoholic Beverages by Capt. Ron Fuller and Lt. Sue Miller, Fulton County, GA Police Dept., June 13th, 1997). In this or any other context, however, CFSs measure the demand for police service, not crime risk.

[9] See pp.12-13, D.T. Campbell and J.C. Stanley, *Experimental and Quasi-Experimental Designs for Research.* Rand-McNally, 1963. This is the design authority cited by Linz *et al*. in the Fort Wayne and Charlotte reports.

property, researchers often define impacts area as a radius of 250 to 500 feet around a source. In the major component of their study, however, Linz and Yao define the impact areas as *Census Blocks*.[10] Since Census Blocks are neither circular nor small areas, even a large, significant secondary effect would be difficult to detect.

It is no surprise then that Linz and Yao fail to find statistically significant effects in Greensboro. Based on their recent work, however, it is surprising indeed that they would use Census Block areas.[11]

The second design idiosyncracy involves control comparisons. To estimate hypothetical secondary effects, Linz and Yao compare Census Blocks with at least one AOB to Census Blocks with no AOBs. Before making the comparison, however, they "statistically adjust" the impact and control Census Blocks for differences presumed to cause crime. Statistical adjustment is very technical issue, particularly in this context. Without discussing technical details, this aspect of the design represents a departure from their recent work.[12]

---

[10] Actually, Census Block *Groups*. Hereafter I say "Census Block" as a short-hand for the technically correct term.

[11] In the Charlotte study, impact areas were defined as a 500-foot circles around AOBs. A 500-foot circle has an area of approximately 785,400 square-feet, about 2.8% of a square-mile. In the Ft. Wayne study, impact areas were defined as 1000-foot circles, approximately 3,141,600 square-feet areas, about 11.3% of a square-mile. In my opinion, a 1000-foot circle is too large an impact area for detection of a secondary effect. This is why I advise planners to build 1000-foot distances into their AOB regulations.

[12] This particular method is not used in either the Ft. Wayne or Charlotte studies. In theory, statistical adjustment of impact-control differences is superior to other methods of control (at least for "static group comparisions"). The availability of data for the adjustment is always a problem, of course.

Both design features represent departures from the conventions of the secondary effects literature and, especially, from their own prior work. In addition to the unknown threats to internal validity posed by the two design idiosyncracies, they raise the specter of "fishing." In the jargon of scientific research, "fishing" refers to the practice of replicating a study several times. With just a few variations in measurements, statistical models, and quasi-experimental designs, a cynical researcher can capitalize on chance to produce any desired result. "Fishing" need not imply dishonesty or cynicism. On the contrary, scientific method recognizes that "fishing" can occur without the researcher's intent or awareness. In experimental research, "fishing" is controlled through explicit design structures, including placebos, blinding, *etc.* In quasi-experimental research, where these structures cannot be used, "fishing" is controlled by means of rigidly enforced design conventions. Departures from convention must be explained and justified. If they are not explained, the critical scientific reader must assume that findings and conclusions are an artifact of "fishing."

(3) Statistical problems. If one ignores the methodological problems posed by the idiosyncratic measure of crime risk and the idiosyncratic design, the manner in which Linz and Yao analyze their data poses yet another serious methodological problem. In prior research, Linz *et al*. have reported null findings – the absence of secondary effects – without reporting the associated probability of error.[13] With two exceptions, Linz and Yao report null findings in Greensboro (TABLE I) but fail to report that probability of error exceeds the conventional level for social science research by a very large factor. The

[13] The probability referred to here is the so-called "Type II" or "false negative" error rate.

unacceptably low statistical power of their null findings is due entirely to methodological idiosyncracies. Given the central question here – whether the adverse secondary effect estimates in TABLE I – questions of statistical power are at the focus of everything that follows.

I.C Outline of this Report

The salient methodological flaw in the Linz-Yao Report is the use of CFSs to measure crime. The correlation between CFSs and conventional measures of crime, such as Uniform Crime Reports (UCRs) is exceptionally weak. In Section II below, I use UCRs and CFSs for the year 2000 to estimate the correlation between CFSs and crime in Greensboro. The statistical reliabilities inferred from the CFS-UCR correlations never exceed .5, suggesting that more than 50 percent of the variance in GPD CFSs is due to factors other than crime – "noise." The consequences of adding "noise" to an indicator are well known. Adding "noise" reduces the statistical size of an effect.

After demonstrating the weak CFS-crime correlation, I discuss related problems with the misuse of CFSs by Linz and Yao. Because the addresses assigned to CFSs record the location of complainants, for example, CFSs cannot be used to analyze "hot spots." The Report's conclusion that the number of CFSs to AOB addresses is lower than the number of CFSs to other nearby addresses, thus, says nothing about the public safety risks of AOBs.

In Section III, I address the quasi-experimental design used by Linz and Yao. In one important respect, their design is unprecedented in the secondary effects literature. Crime risk diminishes exponentially with distance from a criminogenic point-source – an

AOB. Accordingly, secondary studies typically look for secondary effects in the area within 500 feet of the AOB. Since crime risk diminishes exponentially with distance from the criminogenic source, an excessively large impact area can obscure even the largest secondary effect. In prior studies, Linz *et al.* used 500-foot (Charlotte, *e.g.*) and 1000-foot circles (Fort Wayne, *e.g.*) for impact areas. Linz and Yao use irregular polygons (Census Blocks) that are ten to one-hundred times large than any that have been used in secondary effects studies.

Of course, one need not be a statistician to understand the consequences of using excessively large impact areas; it is the equivalent of throwing an needle into a haystack. Other design idiosyncracies raise the problem of "fishing." When a design can be picked from a modest menu of options, the statistical significance of a finding is meaningless. The sheer number of design idiosyncracies in the Linz-Yao Report are sufficient to invalidate the Report's empirical findings.

In Section IV, I discuss the problem of statistical power. Criticizing studies that claim to find adverse secondary effects of AOBs, Linz *et al.* often quote *Daubert*[14] on the importance of "error rates." When Linz *et al.* fail to find adverse secondary effects, on the other hand, or as in this instance, when they conclude that an adverse secondary effect is statistically small – see TABLE I – Linz *et al.* do not report the error rate for the statistical tests underlying their conclusion. Calculating the error rates in Section IV, I demonstrate that their conclusions lack the requisite validity that would make them admissible under *Daubert*.

---

[14] Daubert v Merrell Dow Pharmaceuticals 509 US 579 (1993).

In the concluding Section V, I review some of the literature used by Greensboro in the AOB ordinance process. At least one of the studies used by Greensboro meets the highest standard of validity. I also review two studies by Linz *et al.* that the City did not rely on in formulating its AOB ordinances. Contrary to the opinion of Linz and Yao, both studies have serious methodological shortcomings – many of which are found in their Greensboro study.

II. Measurement Problems in the Linz-Yao Report

Measurement is the *sine qua non* of science. Phenomena that cannot be measured cannot be studied scientifically. The adequacy of a measurement is summed up in the properties of *reliability* and *validity*.[15] To illustrate reliability, Linz and Yao counted 2,445 CFSs to addresses within 1000 feet of "Elm Street Video and News."[16] If another researcher counted the number of CFSs, the recount would probably not yield the same number because even simple counts vary randomly.[17] If the count-recount difference is reasonably small and random, however, the measurement is reliable and adequate for scientific research.

Reliability is probably not an important issue. I assume that the Greensboro data used by Linz and Yao are adequately reliable. Validity is a very different issue, however.

---

[15] For definitions, see H.M. Blalock's *Measurement and Conceptualization in the Social Sciences* (Sage, 1982). See also *Quasi-Experimentation: Design and Analysis Issues for Field Settings* by T.D. Cook and D.T. Campbell (Houghton-Mifflin, 1979).

[16] Linz and Yao, Table 23, p. 20.

[17] In his classic *On the accuracy of economic observations, 2nd Edition* (Princeton: Princeton University Press, 1965), Nobel laureate O. Morgenstern expressed this idea as "*Incipit numerare, incipit errare!*." Begin to count, begin to make mistakes!

The property of validity is associated with *non*random measurement errors. Nonrandom measurement errors consist of differences between the concrete items that one measures and the abstract concepts that these items intend to represent. The relationship between abstract intelligence and concrete IQ is often used to illustrate the property of validity. Although a person's IQ and intelligence are not identical, they are hopefully similar; and if so, IQ is a valid measure of intelligence. If the difference is large, on the other hand, then IQ is not a valid measure of intelligence.

In this instance, of course, we are interested in measuring the hypothetical *crime risk* of an AOB. Whatever measure is used, its validity will depend on how well it tracks crime risk over time and space. Contrary to the conventions established in criminology in the secondary effects literature, particularly the recent work of Linz *et al.*, Linz and Yao use police CFSs to measure crime. This idiosyncratic choice of measures has no precedent and *per se* invalidates their conclusions.

FIGURE II.1 - RELATIONSHIP BETWEEN CRIMES AND CALLS-FOR-SERVICE (CFSS)

II.A. CFSs Are *Not* Synonymous with Crime

Throughout their Report, Linz and Yao speak of "CFSs" and "crimes" as if these two terms were synonymous. In fact, however, while CFSs and "crimes" (or crime-like incidents) are correlated, the correlation is quite weak. This fact, widely known among criminologists, is depicted in FIGURE II. In any modern jurisdiction, CFSs to the police department outnumber crimes reported to the police by a large factor. This well known fact is represented by the relative areas of CFSs (in red) and crimes (in blue). The overlap between CFSs and crimes represents their correlation.

As depicted in FIGURE II, most of the crimes (or crime- like incidents) that come to the attention of the police are *not* initiated by CFSs from victims and witnesses. The police become aware of most crimes through routine patrolling; through directed (or proactive) patrolling; and through specialized unit activity. On the other hand, most of the citizens who call the police – thereby initiating a CFS – are not crime victims or witnesses; most CFSs not initiated by crimes (or crime- like incidents). Examples include duplicated or unfounded CFSs; CFSs that have no apparent basis; and CFSs that precipitated by false alarms.[18]

To investigate the scope of this problem for the Greensboro study, Uniform Crime Reports (UCRs) and CFSs for the same crimes were compared for the period beginning

[18] Of the 32,168 CFSs in 2000 that involved serious crimes, 19,974 (or 70.6 percent) were initiated by electronic alarms. More than 98 percent of all alarm-initiated CFSs in the year 2000 turned out to be false alarms – no crime, *i.e.* Since each of these CFSs resulted in a report, Linz and Yao included them in the analysis even though there was no crime involved. If 2000 is a typical year, one-in-three of the CFSs analyzed by Linz and Yao was a false alarm!

January 1, 2000 and ending December 31, 2000.[19] The five columns of TABLE IIA report the UCR category, total CFSs for that category, CFSs that resulted in an arrest or report (in red), UCRs (in blue), and the ratio of red CFSs to UCRs.

TABLE II.1 - GREENSBORO CFSS AND UCRS IN 2000

	Total CFSs	CFSs w/rpt	UCRs	CFS : UCR
Total Serious Crimes	32,168	28,304	15,492	1.83 : 1.00
Total Personal Crimes	3,311	6,864	1,867	3.68 : 1.00
Total Property Crimes	26,920	21,440	13,625	1.57 : 1.00
Assault	2275	991	816	1.21 : 1.00
Arson	0	0	73	1.00 : 49.0
Auto Theft	1801	1308	1308	1.00 : 1.00
Burglary	22230	17841	3020	5.91 : 1.00
Homicide	0	0	20	1.00 : 41.0
Larceny	2889	2291	9224	1.00 : 4.03
Rape	159	124	121	1.02 : 1.00
Robbery	3152	2317	910	2.55 : 1.00

Considering total serious crimes, CFSs appear to overstate Greensboro's crime risk by a factor of 83 percent. When total crimes are broken down into personal and property crimes, the overstatement persists. When total crimes are broken down into the eight UCR categories, however, a range of biases become apparent. As reported in the right-hand column of TABLE IIA, while CFSs overstate the risk for some crimes – burglary, robbery, *etc.* – CFSs understate the risk for other crimes – arson, larceny, *etc.* Bias in the CFS-crime relationship is not a simple multiplicative factor then. For some

[19] Part I UCR data were obtained from the GPD. The Part I (or serious) UCR categories are arson, assault, auto theft, burglary, homicide, larceny, rape, and robbery.

crimes, it is a *true* bias. A more important problem, however, is that for most crimes, CFSs appear to add random measurement error to the relationship.

II.B. CFS-Crime Correlations and Reliabilities

To estimate the correlation between CFSs and crime, BY-co-ordinates were selected at random from the CFSs and UCRs published by the GPD for 2000. Circles with radii of 500-feet were drawn around the BY-co-ordinates. The number of CFSs and UCRs inside the circles were counted and correlations were estimated from the counts. The results, reported in TABLE II.2, show that the correlations between UCR counts (in blue) and CFS counts (in red) are lower than what would ordinarily be expected or demanded from an indicator.

TABLE II.2 - CFS-UCR CORRELATIONS, ESTIMATED FROM 500-FOOT CIRCLES

	Asslt	Rob	Rape	Pers	Auto	Burg	Theft	Prop
Assault	.325	.122	.121	.300	.059	.123	-.006	.041
Robbery	.122	.674	-.019	.394	.257	.521	.250	.365
Rape	.054	-.109	.074	-.011	-.028	-.065	-.077	-.077
Personal	.236	.534	.062	.444	.212	.431		.273
Auto Theft	.081	.504	.114	.326	.637	.721	.519	.648
Burglary	.196	.332	.190	.325	.361	.541	.327	.433
Theft	.056	.518	.124	.317	.615	.703	.563	.670
Property	.065	.524	.129	.327	.624	.717	.566	.678
Reliability	**.106**	**.454**	**.071**	**.197**	**.406**	**.293**	**.317**	**.460**

The last row of TABLE II.2 list the squared correlation coefficients, or raw reliabilities, for each of the CFS categories. Reliabilities are interpreted geometrically as the intersection of the crime-CFS Venn diagrams in FIGURE II.1. The overlap between UCR assaults and assault CFSs (r^2 =.106) is interpreted to mean that the degree of overlap (or common variance) between the two indicators is 10.6 percent of the total. From the other perspective, 89.4 percent of the total variance in the two indicators is *unique* and, thus, has nothing to do with crime.

TABLE II.2 raises two questions. First, compared to data in other social science fields, how "good" are these reliabilities? Second, what are the practical consequences of using a low-reliability crime indicator? On the first question, reliabilities smaller than .75 are unacceptable for most social science applications. Since the median reliability in TABLE II.2 is approximately .305, testimony based on CFSs might be inadmissible under the *Daubert* standard. On the second question, the practical consequences of using a low-reliability crime indicator are well known. Adding measurement error in the outcome (or dependent) variable does *not* bias the effect estimate – substantively large effects persist in the face of measurement error – but *does* bias tests of significant in favor of the null finding.[20] As a practical matter, in other words, CFSs make substantively large effects statistically small.

II.C. CFS Addresses Are *Not* Crime Locations

Since CFSs are only weakly correlated with crime, using CFSs to measure crime risk is *per se* a fatal flaw. Even ignoring this threshold problem, however, it is nearly impossible to infer even the grossest spatial distribution of crime risk from CFS

---

[20] See, *e.g.*, Blalock's *Measurement and Conceptualization in the Social Sciences* (Sage, 1982).

addresses. The problem is most obvious when Linz and Yao analyze "hotspot" addresses within each Census Block:

> ...the adult bookstores are a negligible source of property crime events and do not appear to be the source of person crime events at all. The bookstores never rise above the 16th ranked address for property crime events (9 events) and are as low as the 205th rank (2 events) or cannot be ranked because there are zero crime events in their immediate vicinity.[21]

The fallacy in this reasoning is that the address recorded on a CFS is not necessarily the location of the precipitating incident. On the contrary, the CFS address tells the patrol unit where to find the caller. If X calls the GPD to complain about a disturbance at Y's house, in a majority of cases, the *CFS* goes to X's address. By the Linz-Yao logic, however, the "crime event" occurred at X's address.

If the proprietor of an business is familiar with this geo-coding convention, CFSs can be manipulated to make the business look more or less in need of police service or regulation. To build a case for more police services, the proprietor can complain to the police about problems that might otherwise be handled informally. Or to hide a public safety hazard, on the other hand, the proprietor can handle many problems informally, thereby recording fewer CFSs and making the business seem safer than it actually is. This is why criminologists do not use CFSs for "hotspot" analyses.[22]

II.D. Summary

Given its nominal purpose– to determine whether AOBs are criminogenic – the

---

[21] Linz and Yao, p. 31.

[22] For another reason, see "Uniform Crime Reports as organizational outcomes." (*Social Problems*, 1982, 29:361-372.). This article describes how a simple personnel change in an urban police department resulted in a thirty percent reduction in CFSs.

Linz-Yao Report should have analyzed crimes, not raw CFSs. The vast criminology literature has not even one precedent for using raw CFSs to measure crime. Criminologists invariably measure crime with UCRs or sample surveys of victims.[23] The smaller, unpublished secondary effects literature has also typically used UCRs or analogous crime statistics.[24] This is not to say that CFSs are not a useful statistic. On the contrary, all urban police departments, including the GPD, collect these data for use in budgeting.[25] But no police department uses CFSs to measure crime or public safety. Criminologists and police departments alike use *crime* to measure *crime*.

A final point, worth noting in this summary, is that the geo-codes on GPD records are too crude to be used for many purposes, including purposes intended by Linz and Yao. Finding two substantively large and statistically significant adverse secondary effects, *e.g.* – see TABLE I – Linz and Yao rely on analyses of "hotspot" addresses to discredit their own finding:

---

[23] See, e.g., *Measuring Crime* (D.L. MacKenzie, P.J. Baunach, and R.R. Roberg, State University of New York Press, 1990). The criminological literature is consistent on this point. A search of four national criminology journals (*Justice Quarterly*, *Criminology*, *Criminal Law and Criminology*, and *Journal of Quantitative Criminology*) for the last three years found not one study that used *CFS*s to measure crime.

[24] This includes studies conducted by Linz *et al.*, particularly the two studies cited in the Linz-Yao Report (*Measurement of Negative Secondary Effects Surrounding Exotic Dance Nightclubs in Fort Wayne, Indiana*; and *Are Adult Dance Clubs Associated with Increases in Crime in Surrounding Areas? A Secondary Crime Effects Study in Charlotte, North Carolina*). The Fort Wayne study uses UCR arrests; the Charlotte study uses UCR crimes.

[25] These valid uses of *CFS*s are discussed in undergraduate policing texts. See, *e.g.*, *Police Administration* by O.W. Wilson and R. McLaren (McGraw-Hill, 1978); *Police and Society* by R.R. Roberg, J. Crank and J. Kuykendall, (Wadsworth, 1999) or *Police Administration* by C. Swanson, L. Territo, and R. Taylor (Macmillan, 1993). All of these texts make the same points that I have made about CFSs.

> The bookstores never rise above the 16th ranked address for property crime events (9 events) and are as low as the 205th rank (2 events) or cannot be ranked because there are zero crime events in their immediate vicinity. For crimes against person events the findings are even more striking — there is only one such event among the eight 1000 foot areas surrounding the video/bookstores.

But in virtually all cases, GPD “hotspot” addresses are spurious. In any year, *e.g.*, one Greensboro address accounts for two to three percent of all serious crime reported to the GPD. The address (2400 Van Story) belongs to the Four Seasons Mall. Other are made into “hotspots” by chronically malfunctioning electronic alarms. Of the 148,155 property crime CFSs analyzed by Linz and Yao, 67,530 (45.6 percent) were precipitated by burglar alarms, mostly false. Due to many similar problems, analyses of “hotspot” address in the Linz-Yao Report are not to be taken seriously.

III. Design Flaws in the Linz-Yao Study

“Design” refers generally to the set of methods, or methodology, used to collect, analyze, and interpret data. One aspect of the Linz-Yao design, the use of CFSs to measure crime risk, has already been critiqued. Measurement is the *sine qua non* of valid inference. Because CFSs are *not* an acceptable crime risk measure, inferences about crime drawn from CFSs are invalid. If Linz and Yao were to replicate the Greensboro study using UCR crimes (*vs.* CFSs), however, there would still be three fundamental problems with their design:

- ♦ Lack of before-after contrasts;
- ♦ Excessively large impact areas;
- ♦ Inadequate controls.

Any of these three shortcomings would be sufficient to invalidate the findings of a secondary effects study. Though not obvious, moreover, all three shortcomings favor a null finding. To the extent that these shortcomings represent departures from designs used in the prior work of Linz *et al.*, furthermore, they raise the specter of "fishing."

III.A Before-After Contrasts

The quasi-experimental design used by Linz and Yao in the Greensboro study is a simple variation of the so-called "static group comparison."[26] Using a variation of the standard notation, this design is diagramed as

Impact Area	.	X	$\text{Crime}_{\text{Impact}}$
Control Area	.	.	$\text{Crime}_{\text{Control}}$

The X in this diagram represents the presence of an AOB in the impact area – but not in the control area. The hypothetical secondary effect is estimated as the difference of the two crime measures. *I.e.*,

$$\text{Secondary Effect} = \text{Crime}_{\text{Impact}} - \text{Crime}_{\text{Control}}$$

If the impact and control areas are identical in every respect except the presence of an AOB, the secondary effect estimate is valid. If the two areas differ in any relevant way, on the other hand, the secondary effect estimate is invalid.

The "static group comparison" design is strengthened considerably when a before-after contrast is added. Using the same notation,

---

[26] Linz *et al.* cite a work by Campbell and Stanley, *Experimental and Quasi-Experimental Designs for Research*, as their authority on quasi-experimental design; *cf.* footnote #10 above. To maintain consistency, I use the same authority. In my opinion, Linz *et al.* have misread Campbell and Stanley.

Impact Area	$\text{Crime}_{\text{Impact, Before}}$	X	$\text{Crime}_{\text{Impact, After}}$
Control Area	$\text{Crime}_{\text{Control, Before}}$	.	$\text{Crime}_{\text{Control, After}}$

The hypothetical secondary effect is now estimated as the before-after difference in the impact area. *I.e.*,

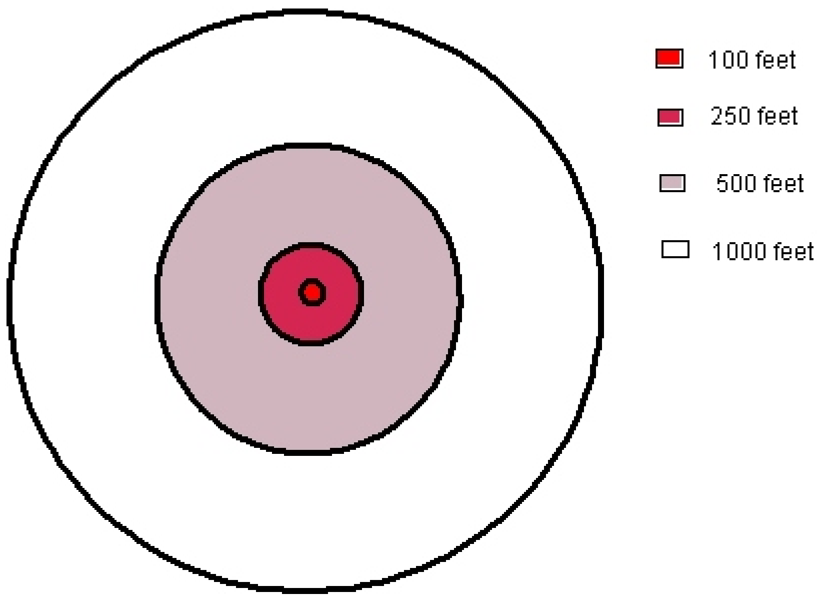
$$\text{Secondary Effect} = \text{Crime}_{\text{Impact, After}} - \text{Crime}_{\text{Impact, Before}}$$

The analogous difference for the control area serves as a benchmark for assessing the validity and significance of the secondary effect. In the before-after design, crime in the impact and control areas is compared to crime in the areas prior to the opening of an AOB in the impact area.

The superiority of the before-design over the “static group comparison” design lies in the nature of their control comparisons. Over short time periods, say one or two years, impact and control areas are likely to remain stable in relevant ways. If the stability assumption holds, before-after differences are immune to the garden variety validity threats that plague static impact-control differences. If change scores are standardized – as percent changes, *e.g.*, or standard Normal scores – before-after secondary effect estimates are relatively robust to minor differences between impact and control areas.

Whether the stability assumption holds or not, however, or whether change scores can be easily standardized, before-after designs are inherently stronger than “static group comparison” designs. I will expand on this theoretical point shortly. In subsequent sections, I will report the results of several secondary effect studies that use before-after designs. For the most part, the validity of these studies cannot be challenged. And at least one of these studies served as the empirical basis for Greensboro's AOB ordinance.

FIGURE III.1 - THEORETICAL DISTRIBUTION OF CRIME AROUND ITS SOURCE



III.B Impact Areas in the Linz-Yao Study

Measuring a secondary effect is complicated by the fact that crime is a statistically rare event. Over the last two centuries, criminologists have observed that the temporal and spatial distributions of crime follow simple mathematical laws.[27] When

---

[27] Motivated by the problem of describing the distribution of crime among Paris neighbor-hoods, the French mathematician S.D. Poisson (1781-1840) discovered a probability distribution that bears his name. See, *e.g.*, F. Haight, *Handbook of the Poisson Distribution* (John Wiley and Sons, New York 1967) for not only the history but, also, for technical details. Briefly, a Poisson distribution has two parameters, λ and p. For a fixed period of time – say, one year – in a given place, the individual's risk of criminal victimization is λ. If p individuals live in the place that year, the product λp is the annual crime rate. According to Poisson theory, the waiting-time (or distance) between crimes follows an exponential distribution with mean λp. The exponential distribution is of waiting times is the important point.

crime is "generated" at a fixed site, the density of crimes around the site diminish exponentially with distance from the site. This is represented conceptually (though not to a mathematically precise scale) by concentric circles in FIGURE III.1. In this depiction, the impact of the criminogenic source or "hotspot" is most intense within 100 feet of the source. Though less intense, the impact is still noticeable within 250 feet of the "hotspot." At 500 feet, the effect is still detectable with an adequately powerful design and statistical model. At 1000 feet, however, the effect exists but is no longer detectable with typical designs and models.

"Noise" is a good analog to criminogenic impacts. Whereas a loud party is easily detected by neighbors on the same block or across the street, residents two blocks away will not notice the noise unless they listen carefully.[28] Four blocks away, exotic sound detection equipment may be needed to detect the noise. The analog to sound detection equipment in secondary effects research is statistical power. This technical topic is discussed in detail at a later point. For present purposes, it is sufficient to note that problems of inadequate statistical power can be resolved by design – i.e., by defining the impact and control areas as 250-foot or 500-foot circles.

The use of existing Census Block areas for the impact and control areas constitutes a major flaw in the design of the Greensboro study. For the design of secondary effect studies, Census Block areas pose two problems. First, Census Blocks

---

[28] City blocks in the older urban areas of Greensboro are approximately 250 feet long. In the newer suburban areas, city blocks are approximately 1000 feet long. Though approximate, these distances are a good rule-of-thumb for interpreting secondary effects.

are not circular areas centered on an AOB. If the AOB is located near the border of a Census Block then, its hypothetical impact may contaminate neighboring blocks. Otherwise, if the AOB is not near the center of the block, its hypothetical impact may not permeate the entire area of the block, creating "control" islands in the block. A more serious problem is that Census Blocks are often larger than the optimal size for impact and control areas.

TABLE III.1 - GREENSBORO CENSUS BLOCKS

Area	Mean	Range	Mean/Ideal	AOBs	Controls
≤0.2 km^2	.1524	.07 - .2	2.1	0	17
≤0.5 km^2	.3388	.21 - .5	4.6	7	53
≤1.0 km^2	.6873	.52 - .99	9.4	8	29
≤2.0 km^2	1.5050	1.07 - 2	20.6	5	11
≤5.0 km^2	2.9910	2.05 - 4.23	41.0	0	20
≥5.0 km^2	9.1143	5.06 - 19.24	124.9	4	19

TABLE III.1 reports the areas and statuses (impact *vs*. control) of the 173 Greensboro Census Blocks used by Linz and Yao.[29] To put these areas in context, the ideal 500-foot circular impact area is approximately 7.3 percent of a square kilometer. The fourth column of TABLE III.1 (in red) gives the ratio of the ideal impact area to the mean area of the Census Blocks. In the best case, where Census Blocks range from .21 to .5 km^2, 4.6 ideal impact areas would fit inside one Census Block. In the worst

[29] TABLE III.1 was generated from a file named "greensboro blk grp 11-26-03.sav" that Linz and Yao sent to the defendants on December 8th, 2003. There are several uncertainties about the file. Non-hierarchical regressions, estimated with SPSS, are reported in an Appendix. Area units (the variable "area") in this file are unlabeled. TABLE III.1 assumes that the units are square kilometers. One could ordinarily resolve these uncertainties through the Census Bureau website. Unfortunately, the Census website was down in the second week of December, 2003.

case, Census Blocks are 124.9 times larger than the ideal. Even in the best case, the impact areas are so vast that they could hide even the largest secondary effect.[30]

III.C Statistical Control in the Linz-Yao Study

The Achilles heel of the "static group comparison" design is the requirement that impact and control areas be virtually identical on all relevant risk factors. When identical impact and control areas are unavailable, impact-control differences can be adjusted by statistical means – in theory, *i.e.* In practice, unfortunately, the covariates required for statistical adjustment are available only for arbitrarily defined areas, such as Census Tracts, Blocks, *etc.*, in decennial years. Since most criminological theories operate on specific spatio-temporal scales – see Figure III.1, e.g. – these data are not ideally suited to criminological research.

Nevertheless, the availability of Block-level decennial Census data was a major factor in the decision by Linz and Yao to use Census Blocks for the impact and control areas:

> Variables that have been investigated and have been found to be most important as predictors of crime activity include measures of racial composition (number of African Americans and racial heterogeneity), family structure (as measured by number of single-parent households, female headed households, or householders with children), economic composition (as measured family income), and the presence of motivated offenders, primarily males between the ages of 18 and 25 (see, e.g., Miethe & Meier, 1994).[31]

---

[30] The "dirty little secret" of social science research is that anyone with a modest research background can design a study that guarantees a null finding. The second most widely quoted sentence in Isaac Newton's *Principia Mathematica* is "*Negativa non Probanda*." In this present context, Newton's observation can be paraphrased as "Finding nothing proves nothing."

[31] Linz and Yao, p. 20.

But in fact, the co-variation of these variables with CFSs has little basis in theory or fact. With respect to criminological theory, crime rates for macro-level social units – cities, counties, *etc.* – do appear to co-vary with demographics. But there is no theoretical reason to expect the same covariation in Greensboro, however, or to expect the same covariation for all CFS-types.

Some of the more technical aspects of this issue will be discussed in Section IV below. For present purposes, however, two broader, conceptual aspects of the Linz-Yao statistical adjustment warrant comments here. First, the regression models used by Linz and Yao to statistically adjust differences among Greensboro's Census Blocks use of *areal rates* as both outcome and explanatory variables. To illustrate, all of the Linz-Yao regression equations have the general form,

$$\text{CRIMES / AREA} = \alpha + \beta \text{ POPULATION / AREA}$$

where CRIMES, AREA, and POPULATION are defined respectively as the number of CFSs (over the period, 1999-2003), the surface area (in km^2) of a Census Block, and population (in 2000) of a Census Block; and where α and β are regression weights.

One minor problem with these equations is that "CFSs per square kilometer" has no relevant interpretation.[32] Because a Census Block's area appears on both the left- and right-hand sides of their regression equations, however, Linz and Yao inject spurious covariance into their models. Concerning model "fit," Linz and Yao claim:

---

[32] For personal crimes – assault, homicide, *etc.* – the unit of risk is the individual. The conventional rate is, thus, "CFSs per population." Since area is *not* the unit of risk – except in some bizarre crime like "land theft" – there is no precedent in the criminological literature for a rate like "CFSs per unit of area." I can think of no reason why Linz and Yao would define a rate of this sort.

> In the final analysis we are able to account for crime events in Greensboro (crimes against person, property crimes, sex crimes, drug-related crime and general disorder incidents) with a moderate to high level of accuracy (explaining from 30 to 60 percent of the variability in crime events across block groups, depending upon the type of crime event).[33]

While technically correct, much of this “accuracy” is due to the unorthodox use of areal rates on both sides of the equation. In exchange for this accuracy, unfortunately, Linz and Yao sacrifice statistical power in their hypothesis tests, particularly those tests that relate to cabaret-type AOBs.[34]

The second conceptual problem, put simply, is that Linz and Yao include too many adjustment variables in their regression models. Although each of the variables included in the models is justified by criminological theory, according to Linz and Yao, many of the explanatory variables have statistically insignificant weight in the regression models. The practical consequences of including statistically insignificant explanatory variables in a multiple regression equation are well known and, given the central issue here, not at all surprising. Each incremental adjustment sacrifices statistical power; an adjustment by a insignificant variable is a pure waste.

III.D The Specter of “Fishing” in the Greensboro Study

In scientific research, “fishing” describes the practice of conducting a study with several slightly different variations. Just a few measures, models, and designs, will produce the entire spectrum of findings – positive, null, and negative. The scientific

---

[33] Linz and Yao, p. 2.

[34] Because the cabarets are concentrated in the larger Census Blocks. The statistical power problem is discussed in Section IV below.

community controls "fishing" through design conventions. Design conventions serve, first, to enhance the comparability of research findings. A more important function in this instance, however, is to minimize "fishing" opportunities. Although researchers can depart from convention when necessary, significant departures must be explained and justified. Otherwise, the critical scientific reader assumes that the findings and conclusions are an artifact of "fishing."[35]

TABLE III.2 - DESIGNS OF THREE RECENT SECONDARY EFFECT STUDIES

	Greensboro	**Fort Wayne**	**Charlotte**
Crime Measure	*CFS*s	UCR Arrests	UCR Crimes
Impact area	Census Blocks with AOBs	1000-foot radius around AOB	500- and 1000-foot radii around AOBs
Control area	Census Blocks without AOBs	1000-foot circle in a non-contiguous "matched" area	500- and 1000-foot radii around other businesses
Covariates	Demographics	None	Crime rates

The potential for "fishing" in the Greensboro study is demonstrated by comparing the designs of three recent secondary effects studies by Linz *et al.*: the Greensboro study, the Fort Wayne study, and the Charlotte study. Although these three studies were completed over two-year period by the same research teams, lead by Professor

[35] See pp. 42-3 in *Quasi-experimentation: Design and Analysis Issues for Field Settings* by T.D. Cook and D.T. Campbell (Chicago: Rand-McNally, 1979) for a discussion of "Fishing and the error rate problem.." Note further that *Daubert* addresses this issue implicitly in its discussion of "the known or potential rate of error."

Linz, the basic designs vary radically. TABLE III.2 summarizes some of the obvious design differences.

Although all three of these studies were conducted during the same period by the same investigators, the design differences are striking. These include:

- ♦ Three different crime measures (*CFS*s, UCR arrests, and UCR crimes);
- ♦ Three different definitions of the impact areas (Census Blocks, 1000-foot radii, and 500-foot radii); and
- ♦ Three different types of controls (statistically adjusted Census Blocks strips, "matched" circles, and other businesses).

Considering only these three design elements, there are at least (3x3x3=) 27 different ways to conduct a secondary effects study. With this many "bites of the apple," finding a result to support any position becomes a near certainty.

Although "fishing" artifacts are not easily calculated,[36] the problem should be intuitively clear. No evidence suggests that the findings and conclusions of the Linz-Yao Report are the product of a "fishing" expedition. Given the controversial nature of the findings and conclusions, on the other hand, as well as the pattern of departures from design convention listed in TABLE III.2, healthy skepticism is in order.

IV. Statistical Power in the Linz-Yao Report

Each of the measurement and design problems discussed in Sections II and III

---

[36] "Fishing" biases the research by inflating the false-positive and false-negative error rates. Error rates in the next section. Because the many possible design variations are not independent, however, the degree of bias is difficult to calculate.

above has the same result: making a substantively large effect statistically small. In light of these threshold problems, each of which is sufficient to invalidate the empirical findings, a critique of statistical power in the Linz-Yao Report might be moot. The issue of statistical power lies at the very heart of the secondary effects debate, however, and in light of TABLE I, at the heart of the Linz-Yao Report's findings.

IV.A Science and Decision Errors

Since every hypothesis must be *either* true *or* false, statisticians deal with two distinct types of decision error: "false positives" and "false negatives."[37] This logical dichotomy is not an accurate description of empirical hypothesis testing, unfortunately. Linz and Yao organize their analyses as a logical dichotomy. If the null hypothesis

H_0: Crime rates in impact and control areas are equal.

is rejected, Linz and Yao will conclude, to a nominal level of statistical confidence, that the alternative hypothesis

H_A: Crime rates in impact and control areas are *not* equal.

is true. In pure logic, of course, if H_0 is true, then H_A must be false (and vice versa). In the empirical realm, however, every hypothesis test has three possible outcomes – a trichotomy!

The jury trial depicted in FIGURE IV is a useful analog. An AOB stands accused of posing an ambient crime risk. After hearing the evidence, the jury convicts, acquits, or hangs. When the jury hangs, there was no decision and, hence, no error. If the jury

[37] False-positives are also called "Type I" or "alpha-type" errors. False negatives are called "Type II" or "beta-type" errors. The terms "false positive" and "false negative," which come from the field of public health screening, are widely used in popular discourse.

convicts or acquits, on the other hand, there is always a small probability that the jury convicted an innocent AOB or acquitted a guilty AOB.

FIGURE IV - TWO TYPES OF DECISION ERROR

	But in Reality, the Defendant is ...	
	Guilty	**Not Guilty**
The Jury Convicts	**95% Confidence**	**5% False Positives**
The Jury Hangs	**?**	**?**
The Jury Acquits	**20% False Negatives**	**80% Power**

In real-world courtrooms, the probability of false verdicts is unknown. Courts enforce strict procedural rules to minimize the probability but we can only guess at the size of an error. In science, on the other hand, we know the *exact* probability of an error. Scientists accomplish this by adopting rigid definitions of certainty. To convict, the jury must have 95 percent certainty in the guilty verdict. This 95 percent level of certainty is called statistical "confidence." To acquit, the jury must have 80 percent certainty in the not-guilty verdict. This 80 percent level of certainty is called statistical "power." The two correct decisions are painted blue in FIGURE IV.

To ground the 95 percent confidence and 80 percent power levels in concrete meaning, the definitions are tied to a theoretical process of replication. In theory, if the case were tried again and again, in the case of a conviction, 95 percent of the juries would return the same guilty verdict; in the case of an acquittal, 80 percent would return the same not-guilty verdict.

The nominal levels of confidence and power imply that five percent of all convictions are false-positive errors and 20 percent of all acquittals are false-negative errors. The incorrect decisions are painted red in FIGURE IV. Errors are never a good thing but at least scientists know the error rates. Error rates can be set higher to make justice more certain, of course, but the level of certainty required for conviction is always set higher than the level required for acquittal.[38]

IV.B TABLE I Revisited

In Section I above, I commented on the discrepancy between the numerical results of the Linz-Yao analyses and their prose description of the numerical results. Whereas the numbers amounted to substantively large adverse secondary effects, the text portrayed these numbers as supporting the null hypothesis – or using the jury trial analogy, of acquitting the AOBs:

> From these analyses we are able to reliably conclude that once we control for variables known to be related to crime there is not a relationship between the presence of an adult cabaret or video bookstore in a

[38] The most comprehensive authority on statistical power is Chapter 22 of *The Advanced Theory of Statistics, Vol. 2, 4th Ed.* by M. Kendall and A. Stuart (Charles Griffin, 1979). J. Cohen's *Statistical Power Analysis for the Behavioral Sciences, 2nd Ed.* (L.E. Erlebaum Associates, 1988) and M. Lipsey's *Design Sensitivity: Statistical Power for Experimental Research.* (Sage Publications, 1990) are better known. Cohen (pp. 3-4) and Lipsey (pp. 38-40) set the conventional false-positive and false-negative rates at .05 and .2. The rates can be set lower, of course, but the ratio of false-positives to false-negatives is always 4:1, implying that false-positives are "four times worse than" false-negatives. The 4:1 convention, which dates back at least to 1928 (J. Neyman and E. Pearson, "On the use and interpretation of certain test criteria for purposes of statistical inference." *Biometrika*, 1928, 20A:175-240), reflects a view that science should be conservative. In this instance, *e.g.*, the 4:1 convention works in favor of the plaintiffs.

neighborhood and crime events.[39]

Accepting the hypothesis – or acquitting – assumes the false-positive rate associated with the secondary effect estimates are no higher than the nominal .2 level. Since Linz and Yao did not report false-positive rates for their hypotheses, I calculated them.

TABLE IV.1 - ERROR RATES FOR THE LINZ-YAO REGRESSION ANALYSES

	Books/Videos			Cabarets		
	Effect	α	β	Effect	α	β
Crimes Against Person	205.9	.01	.04	78.2	.11	.58
Crimes Against Property	897.7	.01	.08	471.1	.10	.63
Drug Related Crimes	27.4	.76	.88	34.4	.58	.92
Sex Related Crimes	7.6	.63	.83	9.9	.37	.86
Disorder Types of Offenses	60.2	.23	.46	43.8	.21	.76
Other Minor Offenses	594.9	.09	.27	281.9	.25	.76

α: false positive rate; β: false-negative rate

The effect estimates in TABLE IV.1 are taken directly from the Linz-Yao Report (Tables 14-19). The consistently large, positive estimates are interpreted as adverse secondary effects. The blue numbers immediately to the right of the estimates are the false-positive or α-error rates reported by Linz and Yao. Linz and Yao used these rates to test null hypotheses. Since ten of the twelve rates are larger than .05, Linz and Yao accepted the null hypotheses in ten cases – ten acquittals, in other words.[40] Last but not least, immediately to the right of false-positive rates, in red, are the false-negative or

[39] Linz and Yao, p. 32

[40] Using analyses of CFS addresses, Linz and Yao concluded that the two estimates with α-error are rates smaller than .05 were aberrations.

β-error rates for the effect estimates.[41]

By convention, false-negative rates in the social, behavioral, and biological sciences must be $\beta \leq .2$ before a null hypothesis can be accepted. But the false-positive rates in TABLE IV.1 range from .27 (for Other Minor Offenses in areas of Greensboro with Books/Videos AOBs) to .92 (for Drug Related Crimes in areas with Cabaret AOBs). These false-negative rates are much too large to be ignored. Failure to report false-negative rates as high as these challenges the threshold credibility of the Report. But even granting Linz and Yao the benefit of the doubt, these false-negative rates are much too high to warrant accepting even one null hypothesis. The record is not twelve acquittals, as Linz and Yao argue, but rather, two convictions and ten hung juries.[42]

IV.C Summary

In purely substantive terms, the secondary effect estimates in TABLE IV.1 are large enough to worry any urban police department. How can numbers be substantively large but, yet, statistically small? The numbers are made smaller by a series of design choices that have the effect of reducing statistical power. Unfortunate design choices begin with the use of CFSs – a "noisy" measure of crime at best – and end with an idiosyncratic statistical adjustment by multiple regression.

---

[41] These rates were estimated with PASS (J. Hintze, NCSS and PASS, Number Cruncher Statistical System, Kayesville, UT, 2001. www.ncss.com). All estimates assume α=.05 and that variables were entered in the exact order reported in Tables 14-19 of the Linz-Yao Report.

[42] But in fact, all twelve effect estimates in TABLE IV are positive. The probability of twelve independent analyses yielding twelve positive estimates, significant or not, would be infinitessimally small – unless the numbers being estimated were positive (*vs*. zero). I address this issue explicitly in the next section.

Given the constraints of time and resources, some of these unfortunate design choices can be addressed only in terms of strong mathematical or statistical theory. The problem of multiple independent hypothesis tests, on the other hand, can be rectified. The α-error rates reported by Linz and Yao, summarized in TABLE IV.1, assume among other things, that the six crime categories are independent. Of course, this assumption is incorrect. Greensboro's "high-crime" neighborhoods are likely to have high rates of all types of crime. As a consequence, the α-error rates reported by Linz and Yao lack the conventional nominal interpretation – they are wrong, *i.e.*

TABLE IV.2 - SIGNIFICANCE TESTS FROM "SEEMINGLY UNRELATED REGRESSIONS"

	Books/Videos		Cabarets	
	Effect	α	**Effect**	α
Crimes against person	**220.8**	**.001**	**88.7**	**.048**
Crimes against property	**1027.5**	**.004**	**411.3**	**.089**
Drug-related crimes	**66.34**	**.312**	**16.7**	**.723**
Sex-related crimes	**21.9**	**.070**	**7.8**	**.351**
Disorderly conduct	**69.2**	**.081**	**34.1**	**.226**
Other minor crimes	**837.5**	**.002**	**205.0**	**.302**

Significant at α<.05 **Significant at α<.10**

TABLE IV.2 reports secondary effect estimates and α-error rates for the six Linz-Yao regression equations. The difference between these numbers and the numbers reported by Linz and Yao (in TABLE IV.1, *e.g.*) is that the numbers in TABLE IV.2 were estimated under the assumption that the six crime categories are correlated across Census Blocks. The results of this regression, reported in the Appendix, support this assumption. Beyond that obvious point, however, the α-error rates in TABLE IV.2 show that, in terms of crimes against the person – assault, homicide, rape, and robbery – both categories of AOBs have substantively large and statistically significant adverse secondary effects.

V. The Linz-Yao Literature Review

In reviewing the literature that the City of Greensboro relied on in writing its AOB ordinances, Linz and Yao conclude that there is a consistent relationship between the methodological rigor of a study and it findings:

> All of the studies that claim to show adverse secondary effects are lacking in methodological rigor. The studies that have been done either by government agencies or by private individuals that have employed the proper methodological rigor have universally concluded that there are no adverse secondary effects.[43]

In addition to relying on literature that they characterize as methodologically unsound, Linz and Yao faulted the City for ignoring the work of Linz *et al.* in Fort Wayne and Charlotte:

> Recently, we have conducted independent, reliable, studies using census data and modern analytical techniques to examine whether "adult" entertainment facilities, and particularly exotic dance establishments engender negative secondary effects. Unlike many of the previous reports, these studies do not suffer from the basic methodological flaws that were enumerated in *Paul*. Unfortunately, the City Council of Greensboro did not consider these investigations despite the fact that the reports were available.[44]

On these two grounds, Linz and Yao conclude that the City's AOB ordinance had no legitimate factual predicate:

---

[43] Linz and Yao, p. 10.

[44] Linz and Yao, p. 10.

> Consequently, the City of Greensboro had no reasonable basis for enacting the adult ordinance based on the information before it.[45]

In my opinion, Linz and Yao overstate both grounds. First, while the broader secondary effect literature includes studies that lack scientific rigor, it also includes studies that satisfy reasonable standards of validity. These more rigorous studies figured prominently in the Greensboro's AOB ordinance process. Second, contrary to the characterization of Linz and Yao, the Fort Wayne and Charlotte studies by Linz *et al.* suffer from many of the same problems cited in the preceding sections.

V.A The 1991 Garden Grove Study

In the early 1990s, James W. Meeker and I conducted a series of secondary effect studies in the city of Garden Grove, CA. These studies found large, significant crime-related secondary effects associated with AOBs on one of the city's main streets. Although CFSs were available, as criminologists, we were aware of the problems with these data and chose to use UCRs instead. Our understanding of crime "hotspots" lead us to define impact and control areas as 250-foot and 500-foot radii around the AOBs. To avoid the validity problems associated with "static group comparison" designs, we used a simple before-after quasi-experimental design. Finally, as a comparison standard, or control, we used other Garden Grove AOBs. Summarizing the Garden Grove studies:

- ♦ Crime measure: UCRs
- ♦ Impact and control areas: 250-foot and 500-foot radii around AOBs

[45] Linz and Yao, p. 14.

- Design: Before-after quasi-experiment
- Controls: Other AOBs in the same neighborhood

In terms of its scientific rigor, the Garden Grove study is the most comprehensive, authoritative study in the secondary effects literature. Nevertheless, Linz and Yao fault the Garden Grove study on several grounds:

> The Garden Grove study fails to use the proper control comparisons. The study attempted to examine the effects of expansion of an adult business. It employed an average of adult businesses that did not expand as a control without attempting to determine if these businesses matched the test business in terms of demographics or other neighborhood features related to crime. Consistently, the authors do not find effects for "Type II" crimes, which include sex crimes. Identical effects are found for alcohol serving establishments that do not feature adult entertainment as those effects found for adult entertainment facilities. Finally, since business expansion was the focus of the study, a failure to examine the effects of other business expansions on crime rate due to increased customer traffic renders the study difficult to interpret.[46]

None of the grounds cited by Linz and Yao are correct. Because the impact and control AOBS were in the same Census Block, *e.g.*, their demographics were identical. Part II (not "Type II") UCRs were included in the study and Part II impacts were found. Finally, business expansion was not the "focus of the study," although several AOB expansions were investigated. Linz and Yao could not have read the Garden Grove report carefully.

Figure V.1 reports a typical result of the Garden Grove study. In March, 1986, an AOB called the "Bijou" opened for business. Compared to the year before, Part I violent UCRs (assault, homicide, rape, robbery), Part I property UCRs (arson, auto theft, burglary, and theft), and Part II UCRs (including "victimless" crimes) rose significantly in

---

[46] Linz and Yao, p. 9.

the 500-foot impact area. The one-year before-after differences for the impact area are plotted as red bars in FIGURE V.1. During the same period, Part I and Part II UCRs at control areas – other AOBs – remained constant. The one-year before-after differences for the control, plotted as blue bars in FIGURE V.1, are nearly invisible – zero, *i.e.*

FIGURE V.1 - CRIME BEFORE AND AFTER AN AOB OPENS

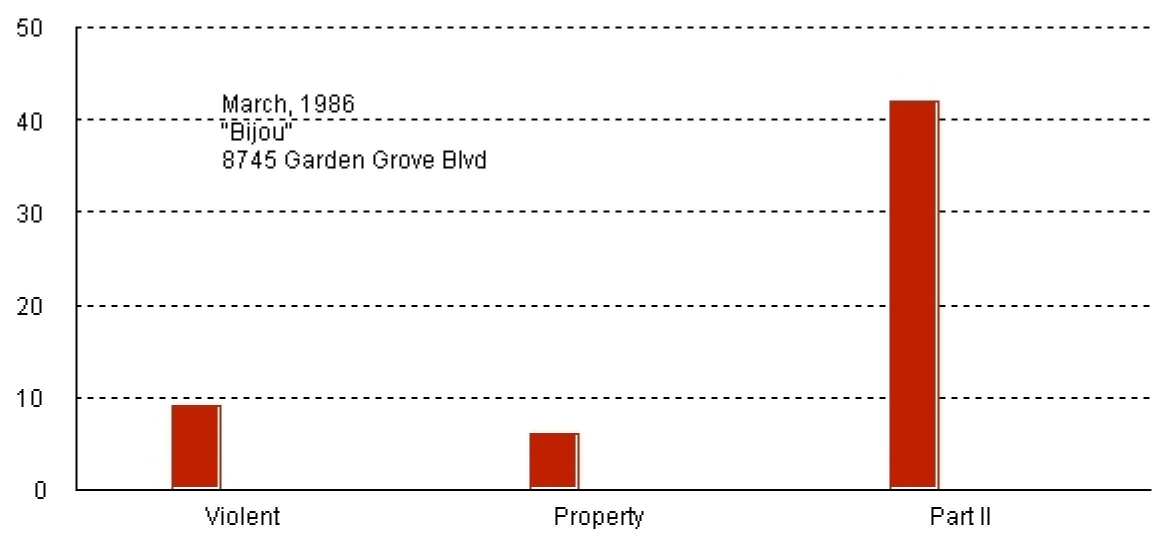
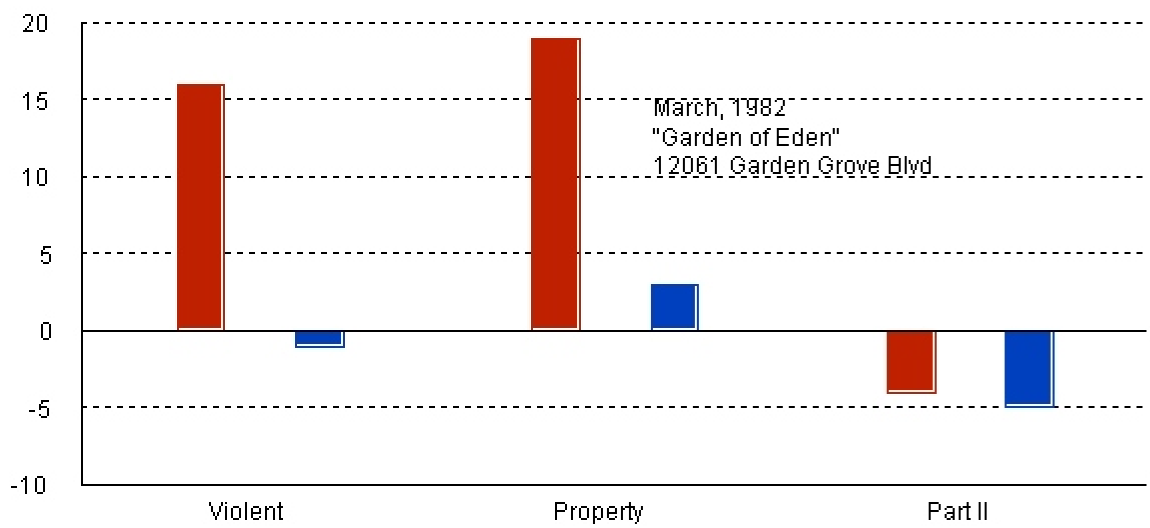


FIGURE V.2 reports result for the expansion of an existing AOB. In March, 1982, an existing AOB tripled its size by acquiring adjacent store fronts. Compared to the year before expansion, Part I UCRs rose sharply in the impact area but not in the control area. Part II UCRs declined in both areas. This unitary decline in Part II UCRs may explain the Linz-Yao comment about “Type II” crime. Because Part II UCRs, which include the so-call victimless crimes, are heavily influence by enforcement policy, their use as secondary effect indicators is problematic.[47]

FIGURE V.2 - CRIME BEFORE AND AFTER AN AOB EXPANDS



In addition to the findings reported in FIGURE V.1-2, the Garden Grove study investigated the relationship between alcoholic beverage serving businesses and AOBs and the effects of architectural retrofits designed to mitigate adverse secondary effects. Since neither issue is relevant to Greensboro, those components of the study need not be reported here. The important point, in my opinion, is the straightforward interpretation supported by before-after designs. Contrasting crime risk after an AOB opens (or expands) to crime at the same address before the AOB opens (or expands)

---

[47] When a police department hires more homicide detectives, the homicide rate does not rise precipitously. Hiring more vice officers will generally lead to more vice arrests, however. The same principle holds for narcotics, traffic, and other Part II UCR crimes. This is the salient difference between Part I and Part II UCRs.

leaves little doubt about the nature of the relationship.

V.B The Fort Wayne and Charlotte Studies

The Fort Wayne and Charlotte studies, in contrast, are made difficult to interpret on several grounds. First, instead of using before-after designs, both studies used weak “static group comparison” designs. Second, both studies relied on controversial, non-intuitive control strategies. In Charlotte, *e.g.*, Linz *et al.* compared eight AOBs to two fast-food restaurants (a KFC and a McDonald’s) and a mini-mart. In Fort Wayne, Linz *et al.* compared UCRs in a 1000-foot radius around and AOB to UCRs in a “matched” 1000-foot circle. A larger problem, however, is that both studies found large, significant *salutary* secondary effects in AOB areas. These salutary secondary effects extended to all three dimensions:

- ♦ Crime was *lower* in AOB areas, compared to control areas.
- ♦ Real estate values were higher in AOB areas, compared to control areas. And in Charlotte,
- ♦ Residents of AOB areas were happier than residents of control areas.

These effects were so unexpected, so counter-intuitive, and so large, that Linz *et al.* had to speculate on the underlying mechanism. First, according to Linz *et al.*, AOB owners take proactive steps to protect customers.

> The extensive management of the parking lots adjoining the exotic dance nightclubs, in many cases including guards in the parking lots, valet parking and other control mechanisms, reduces the possibility of disputes in the surrounding area. In addition, unlike other liquor serving establishments (bars and taverns), disputes in the areas surrounding

> these exotic dance clubs between men regarding unwanted attention by other males to dates or partners are minimal due to the fact that the majority of patrons attend the clubs without female partners. Further, security measures inside the clubs reduce the potential for skirmishes among customers.[48]
>
> ... the establishments themselves have evolved more closely into businesses – establishments with management attention to profitability and continuity of existence. To meet these objectives, it is essential that the management and/or owners of the clubs provide their customers with some assurance of safety. Accordingly, adult nightclubs, including those in Charlotte, typically have better lighting in their parking lots and better security surveillance than is standard for non adult-nightclub business establishments.[49]

If this explanation is correct, it would appear that AOB regulations aimed at public safety – lighting, security guards, *etc.* – have a legitimate basis. More generally, according to Linz *et al.*, broader regulation of AOBs has been effective, at least in Charlotte:

> As noted in the introduction to this paper, adult nightclubs have been subjected to over two decades of municipal zoning restrictions across the country and they usually must comply with many other regulations as well.[50]

These rationales pose a dilemma for Linz *et al.* If AOBs have the miraculous salutary effects claimed by Linz *et al.*, it is because the regulation of AOBs has been effective. But on the other hand, if the salutary effects are an artifact of design idiosyncracies, AOBs are in need of regulation.

The second horn of the dilemma is more plausible. Except that neither the Fort

---

[48] p. 18., Daniel Linz and Bryant Paul, "Measurement of Negative Secondary Effects Surrounding Exotic Dance Nightclubs in Fort Wayne, Indiana." February 13, 2001.

[49] Land, K.C., Williams, J.R., and M.E. Ezell. *Are adult Dance Clubs Associated with Increases in Crime in Surrounding Areas?* p. 31-2.

[50] p. 31-32 of the Charlotte study.

Wayne or Charlotte studies used CFSs, they suffer from the same methodological flaws found in the Greensboro study.[51] TABLE III.2 above lists the salient elements of design in Fort Wayne and Charlotte. Although the two studies were conducted during the same period by the same people, the differences in design are striking. In every study, Linz *et al.* select design elements from a cafeteria of options. Because no two Linz *et al.* designs are even roughly comparable, the credibility of their findings are haunted by the specter of "fishing."

VI. Conclusion

Although the Linz-Yao Report was commissioned by the plaintiffs, the Report's findings contradict the plaintiffs' claim that Greensboro's AOBs pose no crime-related secondary effects. In fact, as reported in TABLES I and IV.1 above, the large adverse secondary effects span both classes of AOBs and six categories of crime. As reported in TABLE IV.2, moreover, the substantively large effects for four serious crimes against persons – assault, homicide, rape, and robbery – are also statistically significant at the nominal $\alpha \leq .05$ level for both classes of AOBs. The relative magnitude of secondary effects reported by Linz and Yao warrant special emphasis. As shown in TABLE I, the secondary effects of AOBs in Greensboro range from 120 to 720 percent higher than the analogous crime effects for bars and taverns.

To conclude that neighborhoods with and without AOBs have statistically similar

---

[51] On p. 11, Linz and Yao seem to claim the Fort Wayne study used CFSs: "The number of calls to the police from 1997-2000 in the areas surrounding the exotic dance nightclubs was compared to the number of calls found in the matched comparison areas." But in fact, the Fort Wayne study used UCRs cleared-by-arrest (vs. all UCRs as was used in Charlotte).

crime rates – a null finding, *i.e.* – Linz and Yao had to overcome a formidable obstacle; two of their twelve secondary effect estimates were statistically significant at the nominal $\alpha \leq .05$ level. Linz and Yao urged the reader not to take these effects seriously because there were relatively few CFSs to AOB addresses. This argument ignores the fact that CFS addresses are not the locations of crime sites, of course, and attempts, subtly, to redefine the terms of debate.[52]

Having dealt with the two statistically significant effect to their satisfaction, Linz and Yao turn their attention to the ten remaining effects. Because these ten estimates are *not* statistically significant, according to Linz and Yao, no matter how substantively large they may be, they must treated *as if* they were zero. And if they are zero, Linz and Yao argue, the difference between neighborhoods with and without AOBs is zero – no difference, in other words.

The flaw in this argument is statistical power. To reject a null hypothesis, as Linz and Yao urge, false-negative error rates for the hypothesis test must be no larger than 20 percent (*i.e.*, $\beta \leq 2$). As reported in TABLE IV.1, of course, none of the Linz-Yao false-negative rates come even close to the conventional level required for social, behavioral, and biological science research.

The unacceptably low statistical power in the Linz-Yao hypothesis tests is a function methodological flaws, of course, spanning measurement, design, and analysis. All of these idiosyncracies have the effect of weakening the statistical foundation of the

[52] The adverse secondary effects of AOBs are *ambient*. As depicted in FIGURE III.1, they radiate outward, diminishing exponentially with distance. Linz and Yao attempt to re-define the secondary effect as something that is necessarily limited the immediate premises or address.

hypothesis tests, making it more difficult to detect an adverse effect. That the adverse secondary effects persisted in the face of so many methodological challenges hints at how strong the adverse secondary effects in Greensboro really are.

Nevertheless, at least one of the methodological flaws in the Linz-Yao analyses can be addressed after the fact. The α-error rates reported by Linz and Yao assume that the six categories of crime are independent when, as a matter of empirical fact, they are highly correlated. TABLE IV.2 reports a set of α-error rates that take the correlations into account. When the inter-crime correlations are assumed, the large adverse effects for violent crimes achieve statistical significance at the nominal $\alpha \leq .05$ level for the two classes of AOBs. This ends the debate.

Finally, the opinions of Linz and Yao on the methodological rigor of the secondary effects literature used by Greensboro to formulate adult-oriented business regulations are at least overstated. *Some* of the methodological criticisms raised by Linz and Yao about *some* of the studies cited by the City are reasonable; but *other* criticisms about *other* studies are unreasonable and, apparently, incorrect. Some of the studies used by Greensboro are based on sound methodologies; and these studies document a mix of adverse secondary effects associated with AOBs. Taken as a body, this literature constitutes a solid empirical foundation for AOB regulations. In my opinion then, Linz and Yao are wrong. The City had an ample factual predicate for its regulations.

APPENDIX

1. Descriptive statistics for six dependent (outcome) variables and 13 independent (explanatory) variables used by Linz and Yao. All statistics were generated by SPSS from the file "greensboro blk grp 11-26-03.sav" emailed to the defendants by Mike Yao.

Var Label	Var Name	Min	Max	Mean	Std. Deviation
Crime: Person	PER_DENS	.00	1153.33	196.8618	234.20536
Crime: Property	PRO_DENS	.00	8900.00	1635.7824	1469.06826
Crime: Drug	DRG_DENS	.00	1577.27	89.0940	225.89693
Crime: Sex	SEX_DENS	.00	261.90	20.6177	37.25911
Crime: Disorderly	DIS_DENS	.00	883.33	127.0375	168.53584
Crime: Other	OTH_DENS	.00	6877.27	646.2676	1038.36874
Population Density	POP_DENS	114.66	13571.43	2599.0934	2022.21626
14-24 Year Olds	AGE15_24	34.00	2977.00	267.6185	340.57068
Median Age	MEDIAN_A	16.5	53.7	35.445	6.8148
Non-whites	NONWHITE	3.00	3494.00	716.9827	659.54439
Fem household w/children	HH_FEMC	0	411	54.54	52.323
Non-family households	HH_NONFA	20	1473	258.83	212.888
In-household unmarried	INHH_NON	5	481	101.88	86.972
Renter occupied household	OCCHU_RE	13	1659	272.65	274.734
Vacant housing	HU_VACAN	4	300	48.29	44.337
Owner vacancy rate	OWNER_VA	.0	14.3	2.022	2.1833
Private clubs (alcohol)	GBNC_BAR	0	11	.37	1.057
AOBs: Books/Videos	GBNC_BKS	0	2	.05	.237
AOBs: Cabarets	GBNC_CLB	0	2	.09	.328

2. Regression models estimated with SPSS from "greensboro blk grp 11-26-03.sav."

A. Summary Statistics for Six Models

Outcome Variable	R	R^2	Adj R^2	SE	F	df
Crime: Personal	.716	.512	.472	170.11259	12.848	13,159
Crime: Property	.798	.637	.607	920.77204	21.449	13,159
Crime: Drug	.637	.407	.358	181.05700	8.365	13,159
Crime: Sex	.563	.317	.261	32.02594	5.677	13,159
Crime: Disorder	.791	.625	.594	107.35378	20.378	13,159
Crime: Other	.708	.501	.461	762.54190	12.303	13,159

B. Parameter Estimates for Six Models

	B	Std. Error	Beta	t	Sig
Crime: Person	262.474	119.183		2.202	.029
Population Density	5.554E-02	.008	.480	6.799	.000
15-24 Year Olds	-.236	.055	-.343	-4.268	.000
Median Age	-4.579	2.836	-.133	-1.615	.108
Non-whites	1.417E-02	.041	.040	.342	.733
Fem household w/children	.370	.519	.083	.712	.477
Non-family households	-.405	.202	-.368	-2.002	.047
In-household unmarried	-.104	.341	-.039	-.305	.761
Renter occupied household	.283	.170	.333	1.666	.098
Vacant housing	-.490	.563	-.093	-.870	.385
Owner vacancy rate	9.273	6.786	.086	1.367	.174
Private clubs (alcohol)	31.179	14.811	.141	2.105	.037
AOBs: Books/Videos	204.593	73.334	.207	2.790	.006
AOBs: Cabarets	79.035	47.496	.111	1.664	.098

	B	Std. Error	Beta	t	Sig
Crime: Property	1766.936	645.106		2.739	.007
Population Density	.419	.044	.577	9.471	.000
15-24 Year Olds	-1.725	.299	-.400	-5.762	.000
Median Age	-27.329	15.350	-.127	-1.780	.077
Non-whites	.433	.224	.194	1.929	.056
Fem household w/children	-5.730	2.811	-.204	-2.039	.043
Non-family households	-2.128	1.096	-.308	-1.942	.054
In-household unmarried	.725	1.847	.043	.392	.695
Renter occupied household	1.832	.921	.343	1.989	.048
Vacant housing	-2.145	3.046	-.065	-.704	.482
Owner vacancy rate	34.942	36.730	.052	.951	.343
Private clubs (alcohol)	390.320	80.170	.281	4.869	.000
AOBs: Books/Videos	954.246	396.938	.154	2.404	.017
AOBs: Cabarets	376.245	257.080	.084	1.464	.145

	B	Std. Error	Beta	t	Sig
Crime: Drugs	243.139	126.851		1.917	.057
Population Density	4.290E-02	.009	.384	4.933	.000
15-24 Year Olds	-.147	.059	-.221	-2.495	.014
Median Age	-5.992	3.018	-.181	-1.985	.049
Non-whites	-3.742E-02	.044	-.109	-.849	.397
Fem household w/children	1.685	.553	.390	3.048	.003
Non-family households	-.247	.215	-.232	-1.144	.254
In-household unmarried	-.963	.363	-.371	-2.652	.009
Renter occupied household	.250	.181	.304	1.381	.169
Vacant housing	1.312E-02	.599	.003	.022	.983
Owner vacancy rate	3.616	7.222	.035	.501	.617
Private clubs (alcohol)	7.204	15.764	.034	.457	.648
AOBs: Books/Videos	50.556	78.052	.053	.648	.518
AOBs: Cabarets	20.495	50.551	.030	.405	.686

	B	Std. Error	Beta	t	Sig
Crime: Sex	6.335	22.438		.282	.778
Population Density	8.623E-03	.002	.468	5.607	.000
15-24 Year Olds	-3.074E-02	.010	-.281	-2.953	.004
Median Age	-8.626E-02	.534	-.016	-.162	.872
Non-whites	9.428E-03	.008	.167	1.209	.229
Fem household w/children	-8.778E-02	.098	-.123	-.898	.371
Non-family households	-4.395E-02	.038	-.251	-1.153	.251
In-household unmarried	-3.905E-02	.064	-.091	-.608	.544
Renter occupied household	2.228E-02	.032	.164	.696	.488
Vacant housing	7.252E-02	.106	.086	.685	.495
Owner vacancy rate	1.573	1.278	.092	1.231	.220
Private clubs (alcohol)	6.981	2.788	.198	2.504	.013
AOBs: Books/Videos	7.730	13.806	.049	.560	.576
AOBs: Cabarets	9.059	8.942	.080	1.013	.313

	B	Std. Error	Beta	t	Sig
Crime: Disorder	236.652	75.214		3.146	.002
Population Density	4.747E-02	.005	.570	9.207	.000
15-24 Year Olds	-.154	.035	-.312	-4.423	.000
Median Age	-5.890	1.790	-.238	-3.291	.001
Non-whites	-2.950E-02	.026	-.115	-1.128	.261
Fem household w/children	.430	.328	.133	1.311	.192
Non-family households	-.290	.128	-.367	-2.274	.024
In-household unmarried	.510	.215	.263	2.367	.019
Renter occupied household	9.926E-02	.107	.162	.924	.357
Vacant housing	-.179	.355	-.047	-.503	.616
Owner vacancy rate	1.529	4.282	.020	.357	.721
Private clubs (alcohol)	27.870	9.347	.175	2.982	.003
AOBs: Books/Videos	66.218	46.279	.093	1.431	.154
AOBs: Cabarets	33.995	29.973	.066	1.134	.258

	B	Std. Error	Beta	t	Sig
Crime: Other	1450.149	534.247		2.714	.007
Population Density	.236	.037	.460	6.457	.000
15-24 Year Olds	-.981	.248	-.322	-3.957	.000
Median Age	-32.081	12.712	-.211	-2.524	.013
Non-whites	-7.424E-03	.186	-.005	-.040	.968
Fem household w/children	4.579	2.328	.231	1.967	.051
Non-family households	-1.635	.908	-.335	-1.801	.074
In-household unmarried	-3.086	1.530	-.259	-2.017	.045
Renter occupied household	1.349	.763	.357	1.768	.079
Vacant housing	-.238	2.522	-.010	-.094	.925
Owner vacancy rate	19.261	30.418	.040	.633	.528
Private clubs (alcohol)	81.963	66.393	.083	1.235	.219
AOBs: Books/Videos	645.549	328.726	.147	1.964	.051
AOBs: Cabarets	204.534	212.902	.065	.961	.338

C. Parameter Estimates for Six-Equation Model. Parameters were estimated with the Stata 8 SUREG routine from “greensboro blk grp 11-26-03.sav.”

Equation		Obs	Parms	RMSE	"R-sq"	chi2	P
1.	per_dens	173	8	165.7808	0.4960	169.74	0.0000
2.	pro_dens	173	10	892.2249	0.6290	308.84	0.0000
3.	drg_dens	173	7	175.9497	0.3898	119.37	0.0000
4.	sex_dens	173	5	31.64325	0.2745	63.86	0.0000
5.	dis_dens	173	8	104.6981	0.6118	287.57	0.0000
6.	oth_dens	173	9	744.327	0.4832	193.60	0.0000

1. per_dens

	Coef.	Std. Err.	z	P>|z|	[95% Conf.	Interval]
pop_dens	.0583978	.007305	7.99	0.000	.0440803	.0727154
age15_24	-.2567067	.0444481	-5.78	0.000	-.3438234	-.16959
median_a	-5.213533	2.296833	-2.27	0.023	-9.715243	-.7118229
hh_nonfa	-.3614153	.123571	-2.92	0.003	-.60361	-.1192206
occhu_re	.2351458	.0966512	2.43	0.015	.0457129	.4245787
gbnc_bar	23.88785	10.27709	2.32	0.020	3.745121	44.03058
gbnc_bks	220.7782	63.91651	3.45	0.001	95.50411	346.0522
gbnc_clb	88.73834	44.8434	1.98	0.048	.8468936	176.6298
_cons	300.7545	95.42885	3.15	0.002	113.7174	487.7916

2. pro_dens

	Coef.	Std. Err.	z	P>|z|	[95% Conf.	Interval]
pop_dens	.4332474	.0397826	10.89	0.000	.3552749	.51122
age15_24	-1.845983	.2598476	-7.10	0.000	-2.355275	-1.336691
median_a	-32.90447	12.85666	-2.56	0.010	-58.10306	-7.705876
nonwhite	.4246629	.1454843	2.92	0.004	.1395189	.709807
hh_femc	-7.76403	1.777136	-4.37	0.000	-11.24715	-4.280906
hh_nonfa	-1.657183	.7851411	-2.11	0.035	-3.196031	-.1183348
occhu_re	1.71995	.6753602	2.55	0.011	.3962684	3.043632
gbnc_bar	340.2704	58.94667	5.77	0.000	224.737	455.8037
gbnc_bks	1027.469	353.2097	2.91	0.004	335.191	1719.748
gbnc_clb	411.2909	242.0976	1.70	0.089	-63.21155	885.7934
_cons	2037.614	536.4461	3.80	0.000	986.1989	3089.029

3. drg_dens

	Coef.	Std. Err.	z	P>|z|	[95% Conf. Interval]	
pop_dens	.0452171	.0076608	5.90	0.000	.0302022	.0602321
age15_24	-.1919905	.0463141	-4.15	0.000	-.2827646	-.1012165
median_a	-6.907077	2.394045	-2.89	0.004	-11.59932	-2.214836
hh_femc	1.400736	.2308506	6.07	0.000	.9482775	1.853195
inhh_non	-.7488683	.1661038	-4.51	0.000	-1.074426	-.4233108
gbnc_bks	66.34121	65.56554	1.01	0.312	-62.16489	194.8473
gbnc_clb	16.75276	47.19064	0.36	0.723	-75.7392	109.2447
_cons	263.0482	105.2732	2.50	0.012	56.71663	469.3798

4. sex_dens

	Coef.	Std. Err.	z	P>|z|	[95% Conf. Interval]	
pop_dens	.0090135	.0012701	7.10	0.000	.0065241	.0115029
age15_24	-.0310079	.0077629	-3.99	0.000	-.0462228	-.0157929
gbnc_bar	4.199698	2.047474	2.05	0.040	.1867219	8.212674
gbnc_bks	21.943	12.12063	1.81	0.070	-1.813004	45.699
gbnc_clb	7.841639	8.411152	0.93	0.351	-8.643916	24.32719
_cons	2.195358	4.159126	0.53	0.598	-5.95638	10.3471

5. dis_dens|

	Coef.	Std. Err.	z	P>|z|	[95% Conf. Interval]	
pop_dens	.0495055	.0045935	10.78	0.000	.0405023	.0585087
age15_24	-.1883981	.0278271	-6.77	0.000	-.2429382	-.1338581
median_a	-6.734602	1.441599	-4.67	0.000	-9.560084	-3.90912
hh_nonfa	-.1552782	.0538132	-2.89	0.004	-.2607502	-.0498062
inhh_non	.502001	.1362557	3.68	0.000	.2349448	.7690572
gbnc_bar	19.13064	5.260491	3.64	0.000	8.820268	29.44101
gbnc_bks	69.20503	39.71134	1.74	0.081	-8.627768	147.0378
gbnc_clb	34.13895	28.22523	1.21	0.226	-21.18149	89.4594
_cons	263.1086	61.50757	4.28	0.000	142.556	383.6612

6. oth_dens

	Coef.	Std. Err.	z	P>|z|	[95% Conf. Interval]	
pop_dens	.2487034	.0319462	7.79	0.000	.18609	.3113168
age15_24	-1.108898	.1933126	-5.74	0.000	-1.487784	-.7300124
median_a	-34.49121	9.209862	-3.75	0.000	-52.54221	-16.44021
hh_femc	3.917426	.7792158	5.03	0.000	2.390191	5.444661
hh_nonfa	-.6974107	.3322596	-2.10	0.036	-1.348628	-.0461938
inhh_non	-2.265352	.7496765	-3.02	0.003	-3.734691	-.7960132
occhu_re	.5386552	.2699186	2.00	0.046	.0096245	1.067686
gbnc_bks	837.5213	276.1655	3.03	0.002	296.247	1378.796
gbnc_clb	204.9952	198.4869	1.03	0.302	-184.032	594.0224
_cons	1512.252	399.746	3.78	0.000	728.7647	2295.74