

SECONDARY EFFECTS OF SEXUALLY-ORIENTED BUSINESSES:

REPORT TO THE OHIO ATTORNEY GENERAL

Richard McCleary, Ph.D.

Wendy Regoeczi, Ph.D.

March 12th, 2010

Introduction

The Attorney General has retained us to review the facts and materials and to formulate opinions on the secondary effects issues raised in *J.L. Spoons*.¹ We reviewed reports by Drs. Daniel Linz² and Lance Freeman³ for that purpose as well as other documents and references. Our assessment of these materials, along with our educational backgrounds and training, have lead us to develop three general opinions:

Opinion 1: The criminological theory of ambient crime risk, known as the “routine activity theory of hotspots,” predicts that sexually-oriented businesses (SOBs) will generate large, significant crime-related secondary effects. These effects are the result of three factors. (1) SOBs draw patrons from wide catchment areas. (2) Because they are disproportionately male, open to vice overtures, reluctant to report victimizations to the police, *etc.*, SOB patrons are “soft” targets. (3) The high density of “soft” targets at the site attracts predatory criminals, including vice purveyors who dabble in crime and criminals who pose as vice purveyor in order to lure or lull potential victims. The conjunction of these three factors generates an ambient public safety hazard in SOB neighborhoods.

Opinion 2: Although the “hotspot” model applies to all SOB subclasses, qualitative differences across subclasses may exist with respect to the nature of the hazard. In this particular instance, the secondary effects of SOBs that serve alcohol and offer on-site entertainment will differ qualitatively from the effects of other SOBs.

Opinion 3: In the last thirty years, the predicted theoretical relationships have been confirmed by empirical studies employing a wide range of quasi-experimental designs. Given that secondary effects are predicted by a strong theory, and given that the predicted relationships are corroborated consistently by a diverse empirical literature, it is a *scientific fact* that SOBs pose ambient crime risks.

“Secondary effects” include litter, noise, traffic, real estate values, crime, and the general quality

¹ *J.L. Spoons, Inc. v. Nancy J. Dragani, et al.* U.S. District Court, Northern District of Ohio, Cleveland, Case No. 04-00314. Hereafter, “*J.L. Spoons*.”

² Report of Daniel Linz, Ph.D., January 15th, 2010.

³ *Examining the Relationship between Adult Oriented Businesses and Surrounding Property Values in Ohio*. Lance Freeman and Derrick Hamilton, no date. It is our understanding that Dr. Hamilton will not be called as a witness in *J.L. Spoons*. Accordingly, hereafter, we refer to this report as “Report of Lance Freeman, Ph.D.”

of life. Although this report focuses on the secondary effects of SOBs on *crime* and *real estate*, virtually all secondary effects are correlated. Our opinions about crime and real estate secondary effects also extend to the broader, general category of secondary effects.

This report begins with an introduction to the criminological *theory* of secondary effects. Over the last forty years, the consensus finding has emerged that SOBs generate large, significant ambient secondary effects. This consensus finding is credible because it is *predicted* by a strong theory. The theory serves as both an explanation of *why* SOBs are expected to have secondary effects but, also, *how* to lessen these effects.

After developing the underlying theory, in §2 below, we review some of the early studies that corroborate the theory. Scientific theory leads us to expect to find secondary effects in SOB neighborhoods and that is exactly what we find. Since 1975, studies conducted across all types of geographical regions (rural, urban, suburban) and examining virtually all subclasses of SOBs have produced results corroborating the theory. Given that the strong theoretical expectation has been tested and corroborated, the consensus finding that SOBs pose large, significant secondary effects is a *scientific fact*.

Nevertheless, in the last decade, experts retained by SOB plaintiffs have argued that the early studies are methodologically flawed and that, when more rigorous methods are used, the empirical evidence demonstrates that SOBs have no secondary effects at all. This argument is incorrect, of course. In §3 below, we review some of the studies conducted since 2001. Since the plaintiff in *J.L. Spoons* serves alcohol and offers live adult entertainment, we concentrate on that SOB subclass. Contrary to the claims made by plaintiffs' experts, the consensus finding of earlier studies demonstrating large, significant secondary effects is further reinforced by those more recent studies.

In §4-5, we respond to the reports by Drs. Linz and Freeman. Dr. Linz's report concerns the secondary effects of SOBs on ambient crime and is organized around three general opinions which we paraphrase here as:

- No criminological theory predicts a correlation between SOBs and crime. The routine activity theory of hotspots, in particular, does not apply to SOBs.
- Secondary effects studies conducted in other jurisdictions generally find an SOB-crime correlation. These studies are unreliable, however.
- Secondary effects studies conducted by Dr. Linz and colleagues are reliable and lead to the conclusion that SOBs have no secondary effects. The State of Ohio ignored these studies.

We disagree with all three of these general opinions. Dr. Linz uses the results of several analyses to support his opinions. In each case, however, we interpret the analytic results differently and

disagree with Dr. Linz's conclusions.

In addition to relying on the broader secondary effects literature, the State relied on evidence collected in public hearings. We review this evidence in §4.6 below. Dr. Linz does not refute this evidence.

Dr. Freeman's report concerns the secondary effects of SOBs on real estate prices. To estimate the magnitude of this secondary effect, Dr. Freeman statistically adjusts the sales prices of homes in Cleveland, Columbus, Dayton and Toledo for some of the variables that determine the value of a home – number of bedrooms, baths, *etc.* – and then computes correlations between proximity to an SOB and adjusted prices. If proximity to an SOB adversely affects the value of a home, the correlations should be large and negative. Finding mostly small correlations, however – except in Columbus where he finds a large, *positive* correlation – Dr. Freeman concludes that there is no evidence to support the consensus opinion that SOBs have adverse secondary effects on real estate values.

Although Dr. Freeman's "hedonic method" is widely used in the planning literature, it is relatively novel in secondary effects research. In our opinion, salient differences between SOBs and other NIMBY ("not in my back yard") sites raise doubts about the utility of hedonic methods in secondary effects research. The large, positive correlation found in Columbus is symptomatic of the problems of the method. If one were to take Dr. Freeman's results seriously, then opening an SOB next door to a Columbus residence would raise the property's value by 127 percent. We know of no legislators who would find this inference reasonable. A more likely interpretation of Dr. Freeman's results is that they are an artifact of an inappropriate method.

A more widely used method for investigating the secondary effects of SOBs on property values relies on surveys of real estate appraisers. In §5.2, we report the result of a survey of real estate appraisers that is particularly relevant to *J.L. Spoons*. In the opinion of a large sample of real estate appraisers, opening an adult cabaret (the SOB subclass at issue in *J.L. Spoons*) within 500 feet of a residential or commercial property adversely affects the property's value. Opening a non-adult cabaret (liquor and live non-adult entertainment) within 500 feet will have a smaller adverse affect in the appraisers' opinions.

One might discount survey data on the grounds that they are subjective. They support a *reasonable* conclusion, however, and are optimally relevant to *J.L. Spoons*. Dr. Freeman's statistical results are *unreasonable*, in contrast, and not necessarily relevant. Although hedonic pricing methods have been used to model the secondary effects of other land uses (NIMBY sites, parks, *etc.*) and pollution point-sources, they are a novelty in this field. Theoretical differences between SOBs and these other land uses question the suitability of the methods. The perverse implications of Dr. Freeman's statistical results reinforce this view.

After discussing the opinions of Drs. Linz and Freeman, we address several important methodological issues. In §6, we discuss the use of 911 calls to measure ambient crime risk. As a general rule, Dr. Linz and other plaintiffs' experts prefer this measure. Criminologists prefer the

more traditional measure on statistical and substantive grounds. In §7, we discuss statistical hypothesis tests, particularly the conventions of statistical power. Dr. Linz and other plaintiffs' experts have conducted secondary effects studies that, in their opinion, demonstrate that SOBs have no secondary effects. When the widely accepted statistical power conventions are taken into consideration, however, these null findings are shown to be inconclusive. Finally, in §8, we list our references and authorities.

1. The Criminological Theory of Secondary Effects

It is a *scientific fact* that SOBs, as a class, pose large, statistically significant ambient public safety hazards. The public safety hazard consists not only of “victimless” crimes (prostitution, drugs, *etc.*) but, also, the “serious” crimes (assault, robbery, *etc.*) and “opportunistic” crimes (vandalism, trespass *etc.*) associated with vice.

Table 1 - Secondary Effect Studies Relied on by Legislatures

| | | | |
|------------------|------|-------------------|------|
| Los Angeles, CA | 1977 | Times Square, NY | 1994 |
| Whittier, CA | 1978 | Newport News, VA | 1996 |
| St. Paul, MN | 1978 | Dallas, TX | 1997 |
| Phoenix, AZ | 1979 | San Diego, CA | 2002 |
| Minneapolis, MN | 1980 | Greensboro, NC | 2003 |
| Indianapolis, IN | 1984 | Centralia, WA | 2003 |
| Austin, TX | 1986 | Daytona Beach, FL | 2004 |
| Garden Grove, CA | 1991 | Montrose, IL | 2005 |
| Manhattan, NY | 1994 | Sioux City, IA | 2006 |

The SOB-crime relationship qualifies as a “*scientific fact*” on several grounds. First, the crime-related effects of SOBs are predicted by a strong scientific theory. Second, these expected theoretical relationships have been corroborated empirically. On the second point, Table 1 lists eighteen empirical studies whose findings corroborate the claim that SOBs pose large, significant ambient public safety hazards. The extensive range of time-frames, locations, and circumstances represented by these studies suggests that the uniform finding is general and robust.

1.1 The Routine Activity Theory of Crime Hotspots

The consensus finding of this literature becomes *scientific fact* when it is interpreted in the context of a scientific theory. In this instance, the SOB-crime relationship is predicted by a mainstream theory of modern scientific criminology. The so-called routine activity theory⁴ answers the what-when-where questions of victimization risk. As applied to “hotspots of predatory crime,” such as SOB sites, the theory holds that ambient crime risk, generally defined as the number of crimes within 500-1000 feet of a site, is the product of four risk factors. This can be written as:

⁴ This theory is due to Cohen and Felson (1979; Felson and Cohen, 1980; Felson, 1998). The routine activity theory is one of the most validated theories in modern social science. In 2005 alone, according to the *Social Science Citation Index*, the 1979 Cohen-Felson article was cited 621 times. The “hotspot” application of the theory is due to Sherman, Gartin, and Buerger (1989) and to Brantingham and Brantingham (1981; 1993).

$$\text{Ambient Crime Risk} = \frac{N \text{ of Targets} \times \text{Average Value}}{\text{Police Presence}} \times \text{Offenders}$$

An increase (or decrease) in the number of targets at the site or in their average value yields an increase (or decrease) in ambient crime risk. An increase (or decrease) in police presence, on the other hand, yields a decrease (or increase) in ambient crime risk.

1.1.1 Targets

SOB sites are crime hotspots because they attract potential victims, or targets, from wide catchment areas. In this respect, SOB sites are comparable to tourist attractions (Dimanche and Lepetic, 1999; Danner, 2003) and sporting events (Corcoran, Wilson and Ware, 2003; Westcott, 2006). However, compared to the targets found at these better known hotspots, the targets found at SOBs are exceptionally attractive to offenders. This reflects the presumed characteristics of SOB patrons. The patrons do not ordinarily live in the neighborhood but travel long distances to the site.⁵ They are disproportionately male, receptive to vice overtures, and carry cash. Most important of all, when victimized, they are reluctant to involve the police. From the offender's perspective, they are "perfect" victims.

1.1.2 Offenders

The crime-vice connection has been a popular plot device for at least 250 years. John Gay's *Beggar's Opera* (1728), for example, describes the relationship between MacHeath, a predatory criminal, and the vice ring composed of Peachum, Lucy, and Jenny. This popular view of the relationship between vice and predatory crime is reinforced by the empirical literature on criminal lifestyles and thought processes. In an ethnographic study of *London's Underworld*, Victorian reformer Henry Mayhew (1851, 75-88) describes a type of prostitute who conspires with predatory criminals to rob patrons. Seventy-five years later, sociologist Clifford R. Shaw (1930; Snodgrass, 1982) documents the life of "Stanley," a delinquent who lives with a prostitute and preys on her clients.

This routine activity theory of hotspots assumes there exists a pool of rational offenders who travel freely from site to site, opting to work the most attractive site available. These

⁵ In 1990, as part of an investigation, Garden Grove police officers ran registration checks on motor vehicles parked at SOBs. Virtually all of the vehicles were registered to addresses outside Garden Grove. The 1986 Austin, TX study arrived at the same finding. More recently, the Effingham County Sheriff's Department ran registration checks on motor vehicles parked at an SOB in the Village of Montrose. Except for employees' vehicles, all were from outside the county.

offenders lack legitimate means of livelihood and devote substantial time to illegitimate activities; they fit Sutherland's (1937) definition of "professional thieves." Outside of that, they are a heterogeneous group. Some are vice purveyors who dabble in crime. Others are predatory criminals who lure and lull their victims with offers of vice. Despite their heterogeneity, these offenders share in common a rational decision-making calculus that leads them to seek out adult business sites.

1.1.3 Target Value

Criminological thinking has changed little in the 75 years since Shaw's (1930) *Jack-Roller*. To document the rational choices of predatory criminals, Wright and Decker (1997) interviewed 86 active armed robbers. Asked to describe a perfect victim, all mentioned victims involved in vice, either as sellers or buyers. Three of the armed robbers worked as prostitutes:

From their perspective, the ideal robbery target was a married man in search of an illicit sexual adventure; he would be disinclined to make a police report for fear of exposing his own deviance (p. 69).

The rational calculus described by these prostitute-robbers echoes the descriptions of other predators (see Bennett and Wright, 1984; Feeney, 1986; Fleisher, 1995; Katz, 1988, 1991; Shover, 1996). Individuals involved in drugs as a form of vice are also rationally selected as robbery targets. As one robber noted in Jacobs' study (2000:29), "you can never report a drug-related robbery."

1.1.4 Police Presence

Holding constant the quantity and value of the targets at a site, rational offenders choose sites with the lowest level of visible police presence. In strictly physical terms, increasing (or decreasing) the number of police physically on or near a site reduces (or increases) ambient risk. However, police presence can also be virtual through remote camera surveillance and similar processes.

The *effectiveness* of both physical and virtual police presence can be affected – for better or worse – by broadly defined environmental factors. For example, due to the reduced effectiveness of conventional patrolling after dark, crime risk rises at night, peaking around the time that taverns close. Darkness has a lesser effect on other policing strategies, which raises the general principle of *optimizing* the effectiveness of police presence. One theoretical reason why SOB subclasses might have qualitatively different ambient risks is that they have different optimal policing strategies.

1.2 Theoretical Role of Subclasses

In lawsuits, SOB plaintiffs have argued that their narrowly-defined SOB subclass is exempt from criminological theory. But in fact, the relevant criminological theory applies to all

subclasses. To the extent that two SOB subclasses attract similar patrons from similarly wide catchment areas, theory predicts similar ambient crime risks. Put simply, similar causes (the presence of many high-value targets and low levels of police presence) have similar effects (*i.e.*, high ambient crime risk). Existing data support these predicted relationships. Although the theory applies equally to all SOB subclasses, at the same time, it allows for qualitative differences among the subclasses.

Subclass-specific risks arise in some instances. This occurs when the defining property of the subclass implies (or creates) idiosyncratic opportunities (or risks) for particular types of crime. Compared to the complementary subclass, for example, SOBs that serve alcohol present idiosyncratic opportunities for non-instrumental crimes, especially simple assault, disorderly conduct, *etc.* SOBs that provide on-premise entertainment present idiosyncratic opportunities for vice crime, customer-employee assault, *etc.* Criminologists call this etiological crime category “opportunistic.” There are many obvious examples and SOB regulations often treat subclasses differently because of their varying ambient opportunity structures.

Qualitative differences also arise when the defining property of the subclass compromises the effectiveness of common policing strategies. Policing SOBs that offer on-site entertainment (adult cabarets, peep shows, *etc.*) may require that police officers inspect the interior premises, for example. Because this places officers at risk of injury, policing on-site SOBs requires specially trained and equipped officers, prior intelligence, specialized backup manpower, and other resources. Furthermore, routine drive-by patrols to “show the flag” are less effective because such premises offer cover for potential offenders who can wait inside without arousing suspicion.

The optimal policing strategies for two subclasses are sometimes incompatible or even mutually exclusive. To illustrate, an optimal policing strategy for SOBs that do not offer on-site entertainment, such as adult video and book stores, often involves neighborhood patrols by uniformed officers in marked cars. Visibility is a key element of this strategy. For peep shows and adult cabarets, on the other hand, the optimal policing strategy often involves boots-on-the-ground deployments of plainclothes officers and unmarked cars. Avoiding visibility is a key element of this strategy. Obviously, neighborhood patrols by plainclothes officers driving unmarked cars would defeat a major purpose of drive-by patrols; likewise, sending uniformed officers into an adult cabaret would be an inefficient method of control and might pose a physical danger to the officers, patrons, and employees. As a general rule, distinct SOB subclasses may require distinct policing strategies to mitigate ambient crime risks.

To some extent, differences among the optimal policing strategies for SOB subclasses amount to differences in cost. In many (but certainly not all) instances, the least expensive policing strategy involves drive-by patrols by uniformed officers in marked cars. Beyond the deterrent value of visible drive-by patrols, patrol officers can keep watch for known offenders and suspicious activity. When potential problems are spotted, the patrol officers can forward the information to a specialized unit or, if necessary, handle it on the spot, requesting backup

resources only as needed.⁶ It is important to realize, nevertheless, that the implementation of a policing strategy is determined in large part by local exigencies.

1.3 Theoretical Role of Alcohol

Proximity to alcohol is a key component of the criminological theory of secondary effects. Alcohol aggravates an SOB's already-high ambient crime risk by lowering the inhibitions and clouding the judgments of the SOB's patrons. In effect, alcohol serves to further soften the already soft targets found at the SOB site. The available data corroborate this theoretical expectation in all respects. Predatory criminals prefer inebriated victims,⁷ and SOBs that serve alcohol or that are located near liquor-serving businesses pose accordingly larger and qualitatively different ambient public safety hazards.⁸ Governments rely on this consistent finding generated from crime-related secondary effect studies as a rationale for limiting nudity in liquor-serving businesses.

Though not explicitly represented as a risk factor in the criminological theory, laboratory research demonstrates a relevant adverse interaction between consumption of alcohol and adult entertainment. Experimental subjects who drank alcohol *and* viewed pornography were more aggressive than subjects who drank alcohol *or* viewed pornography (Norris *et al.*, 2002; Davis *et al.*, 2006). Relying on this research finding, several states have limited the availability of alcohol in SOBs.

1.4 Theoretical Role of Mitigation Strategies

The routine activity theory points to strategies for mitigating the crime-related secondary effects of SOBs. In principle, the effects of a mitigation strategy can be *direct* or *indirect*. *Direct* effects are typically realized through *direct* manipulation of the risk factors to reduce ambient risk. *Indirect* effects are realized by making the risk factors more efficient. In practice, of course, some of the strategies are expensive or otherwise impractical. Our review of these

⁶ See, *e.g.*, National Research Council. *Fairness and Effectiveness in Policing: The Evidence*. National Academies Press, 2004.

⁷ See, *e.g.*, Wright and Decker (1997, p. 87): “[E]ach of (the armed robbers) expressed a preference for intoxicated victims, who were viewed as good targets because they were in no condition to fight back.” (p. 70); “Several [armed robbers] said that they usually chose victims who appeared to be intoxicated because, as one put it, ‘Drunks never know what hit them.’”

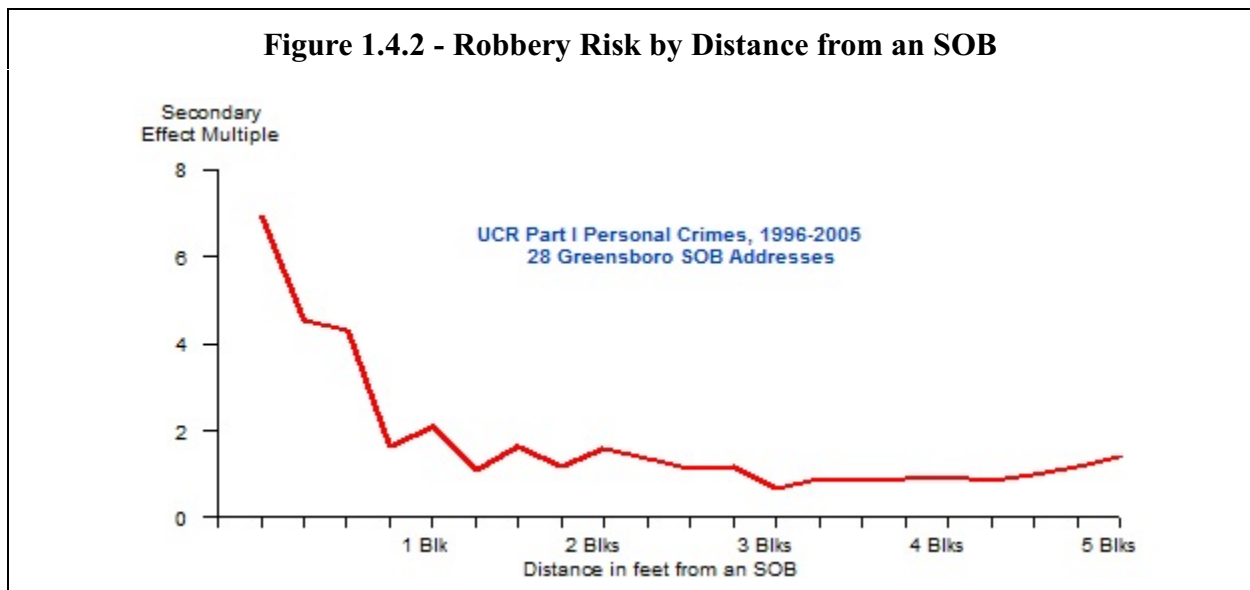
⁸ A 1991 study of Garden Grove, California by McCleary and Meeker found a large, significant increase on ambient crime risk when an alcohol-serving establishment opened within 500 feet (*ca.* one city block) of an SOB. Secondary effect studies in Greensboro (2003) and Daytona Beach (2004) found that alcohol-serving SOBs had larger secondary effects than retail alcohol outlets. These studies are reviewed in §2-3 but see, especially, footnote 21 below.

strategies begins with one of the most expensive, least practical mitigation strategies.

1.4.1 Increased Police Presence

The simplest, surest way to mitigate ambient crime risk is to assign more police to SOB neighborhoods. Although the relationship between police presence and ambient crime risk is complicated and complex, criminologists generally accept the aphorism: “more police, less crime.”⁹ Unfortunately, this simplest, surest mitigation strategy is expensive and impractical. From the government’s perspective, increasing the number of police patrols in a neighborhood is prohibitively expensive. From the perspective of the SOB and its patrons, police presence can be highly intrusive, bordering on “harassment.”

In principle, fixed levels of police presence can be made more effective by fine-tuning existing policing strategies. Police patrols can be made more visible, *e.g.*, by using uniformed officers in marked vehicles instead of plain-clothes officers in unmarked vehicles. Most police departments have already optimized their strategies, however. Police effectiveness can also be enhanced by incorporating rational enforcement policies into SOB codes. Several examples are described in subsequent sections.



⁹ See, *e.g.*, S.D. Levitt. Using electoral cycles in police hiring to estimate the effect of police on crime. *American Economic Review*, 1997, 87:270-290. “Increases in police are shown to substantially reduce violent crime but have a smaller impact on property crime. The null hypothesis that the marginal social benefit of reduced crime equals the costs of hiring additional police cannot be rejected.” (p. 270). Some “victimless” vice crimes are an exception to the rule, of course.

1.4.2 Increased Distance from Sensitive Uses

Reducing the density of targets in an SOB neighborhood is a more economical, practical mitigation strategy. As a rule, the most problematic secondary effects are associated with dense concentrations of SOBs (*e.g.*, Boston’s “combat zone” model). Accordingly, many governments require minimum distances between SOB sites (*e.g.*, the Detroit model). In addition to reducing per-site target density, in turn decreasing aggregate risk, this model minimizes many obstacles to routine policing.

Figure 1.4.2 demonstrates the rationale for a related mitigation strategy.¹⁰ The vertical axis of this “risk-distance function” is calibrated in units of Part I personal crime (homicide, aggravated assault, robbery, and rape) risk, relative to the neighborhood risk, for 28 Greensboro SOBs for 1996-2005. The horizontal axis is calibrated in distance from an SOB. The unit of distance is a city block which, in the Greensboro neighborhoods from which these data are taken, is approximately 400 feet.

Suppose that a person exits a building five city blocks (*i.e.*, 2,000 feet) from an SOB. As this person walks toward the SOB, his or her victimization risk rises. For the first few blocks, the risk increments are modest; thereafter, the risk increments grow large. At two blocks from the SOB, the person’s risk is double what it was at the start of the five-block walk. At one-half block, the risk is six times higher. If the person walks away from the SOB site, his or her victimization risk falls until, at a distance of three blocks from the site, the risk decrements are imperceptible.

Governments can take advantage of the risk-distance relationship plotted in Figure 1.4.2 by setting minimum distances between SOBs and other sensitive land uses. SOB patrons have no choice but to “run the gauntlet.” The victims of some ambient crime incidents are not SOB patrons, however, but rather, are neighborhood residents and passers-by. By setting minimum distances between SOBs and the land uses frequented by these people, the government mitigates the SOB’s ambient crime risk secondary effect.¹¹

1.4.3 Limited Hours of Operation

Another economical and practical strategy for mitigating the ambient crime risk of SOBs is

¹⁰ Risk-distance functions are revisited in §3-4 below.

¹¹ We are often asked to specify a distance sufficient to fully mitigate an SOB’s ambient crime risk. The correct answer to this question – “As far as possible” – is not helpful. Although the risk-distance function plotted in Figure 1.4.2 seems to answer this question, remember that it is the *average* of 28 SOB sites. By definition, some sites are “better,” some “worse.” Planners must assume a worst case scenario while balancing this assumption with practical (and legal) considerations.

to limit the hours of operation. Routine activities theory reduces to the aphorism, “more targets, more crime.” And in the overnight hours when businesses close and people go home, the crime rate drops. While the crime *rate* drops, however, the *per-target* risk rises. When a business stays open around-the-clock, its victimization risk rises steadily after sundown, peaking in the early morning. Darkness softens a target, increasing its attractiveness to predatory criminals.

Several mechanisms operate here but the most salient is that routine policing is more difficult and less effective in darkness. When bars and taverns close, police resources are stretched thinner, making already soft targets even softer. Governments typically mitigate this risk by closing high-risk public places (playgrounds, beaches, parks, *etc.*) from dusk to dawn; by imposing curfews on high-risk persons (teenagers, parolees, *etc.*); and by limiting the operation of high-risk businesses (bars, SOBs, *etc.*) during times of acute risk. Not surprisingly, this theoretical prediction is confirmed by the empirical evidence.

1.4.4 “Hardening” SOB Sites¹²

In principle, ordinances can mitigate ambient crime risk by requiring SOBs to “harden” their properties. Mandating outdoor lighting, parking lot surveillance cameras, and anti-“cruising” structures illustrate strategies for hardening the site’s exterior. There are very few options for exterior hardening, unfortunately; and although the effectiveness of exterior hardening strategies depends to some extent on local circumstances and conditions, there is little evidence that any of the typical options can mitigate ambient crime risk.

Regulating the interior configurations of SOBs, in contrast, has a stronger rationale in criminological theory. Moreover, interior hardening strategies are often less costly, more practical, and in theory, more effective. Three widely used strategies illustrate the general principle:

- Ordinances that eliminate interior blind spots
- Ordinances that prohibit closed viewing booths
- Ordinances that restrict entertainers to raised stage areas

Each of these strategies reduces the risk of on-premise victimization of patrons and employees.¹³ In some respects, the risk reduction mechanism is obvious. Removing blind spots and opening up closed booths obviously reduces the opportunity for lewd behavior, *e.g.* Though less obvious, to

¹² The classic statement on “hardening” is Oscar Newman’s *Defensible Space: Crime Prevention Through Urban Design*. (New York: MacMillan, 1973).

¹³ The strategies also facilitate routine enforcement while minimizing the risk of injury to police officers. Those topics are discussed separately in the next section.

the extent that patron-on-patron, patron-on-employee, and employee-on-patron confrontations are precipitated by lewd behavior, these strategies also reduce the risk of assault.

The highest risk of patron-on-patron, patron-on-employee, and employee-on-patron crime occurs inside SOBs that feature live entertainment; and of course, alcohol aggravates the risk. The risk can be mitigated by separating patrons and entertainers. Ideally, separation is achieved by mandated structures, such as raised stages. By creating a tangible “wall” between employees and patrons, raised stages reduce unintentional (or intentional) “touching,” thereby decreasing the risk of patron-on-employee and employee-on-patron crime.

1.4.5 Police Officer Safety

While assaults on police officers are rare, they are among the most serious crimes that occur inside SOBs. In theory, moreover, they are preventable. Since the risk of assault extends from the time officers enter the SOB until they leave, mitigation strategies are aimed at minimizing the number of times officers must enter SOBs and the length of time they must spend inside upon entry. Strategies that focus on the latter factor are more practical.

Police officers enter SOBs either in response to a reported crime incident or to inspect the premises as part of routine enforcement. By reducing the risk of on-premise crime incidents, the interior target-hardening strategies described in the preceding section reduce the number of times that officers must enter SOBs to respond to reported incidents. Otherwise, there are few options for reducing the number of times that officers must enter SOBs. Notwithstanding the risk to officers, routine inspection can be an effective mitigation strategy. By focusing attention on SOB sites, routine inspection reduces ambient risk through a complex set of pathways collectively referred to as “broken windows.”¹⁴

Regardless of how officers come to be inside an SOB, any strategy that minimizes the amount of time spent inside reduces the risk of injury. Ordinances aimed at improving interior visibility illustrate these strategies. In many instances, officers can accomplish their purpose with a quick visual inspection. If the interior of the SOB is well lit and obstacle-free, the inspection can be completed by a single officer in a minute or two. If the interior is dark and/or labyrinthian, the same inspection may require two (or more) officers for a longer period of time.

In SOBs that feature live entertainment, a raised stage reduces the risk of injury to police officers through the same mechanism. If an ordinance mandates, say, a six-foot distance between patrons and entertainers, absent a raised stage, enforcing (and/or detecting willful violations of)

¹⁴The best known statement of this effect is “Broken windows: The police and neighborhood safety.” by J.Q. Wilson and G.L. Kelling, *Atlantic Monthly*, 1982, 249:29-38. Wilson and Kelling argue persuasively that police visibility in a neighborhood can have a greater impact on victimization risk than police activities that target crime *per se*. Modern police methods are based on this theory.

the ordinance may require that several plainclothes officers spend an hour or more inside. With a raised stage, on the other hand, a comparable level of enforcement and detection of violations can be accomplished with shorter, more superficial inspections. Raised stages also facilitate self-enforcement. Lacking a raised stage, ensuring that patrons and entertainers comply with a distance rule demands constant attention and keen judgement by the SOB. A raised stage facilitates self-enforcement by the SOB, thereby reducing the risk of patron-patron and employee-patron confrontations.

1.4.6 Tailoring Regulations to Local Needs

The ideal SOB ordinance combines low compliance costs for the SOB with low enforcement costs for the government. To some extent, compliance and enforcement costs depend on local circumstances and conditions and these often dictate differences in codes and/or enforcement strategies. A code or strategy that is optimal for one set of circumstances may be less than optimal for another. If a local variation is aimed at rationalizing regulation and optimizing mitigation, it should be encouraged.

By definition, local conditions are too numerous to list. Nevertheless, the principle is straightforward. Legislatures adapt and modify codes to address local idiosyncracies. In most instances, modifications are designed to facilitate compliance and minimize enforcement costs. Toward that end, legislatures often consult local enforcement officers and, to the extent possible and appropriate, incorporate the views of experts into the regulations.

1.5 Concluding Remarks on Criminological Theory

The legal debate over the crime-related secondary effects of SOBs ignores the crucial role of criminological theory. *Without exception*, criminological theory predicts that SOBs will generate ambient public safety hazards. Witnesses for SOBs produce study after study to show that SOBs have *no* crime-related secondary effects or, sometimes, that SOBs have beneficial public safety impacts on their neighborhoods. We discuss the details of these studies at a later point. For present purposes, the criminological theory that we have described is internally consistent, compelling, and widely accepted.

The historical record indicates the wide acceptance of this theory. Recapitulating the opinions of experts in *Young v. American Mini-Theaters* (1976), *e.g.*, Justice Stevens outlines the crux of the theory:

In the opinion of urban planners and real estate experts who supported the ordinances, the location of several such businesses in the same neighborhood tends to attract an undesirable quantity and quality of transients, adversely affects property values, causes an increase in crime, especially prostitution, and

encourages residents and businesses to move elsewhere.¹⁵

Three decades later, in a case that has much in common with *J.L. Spoons*, the Illinois Supreme Court describes the theory in more explicit detail:

The City also adduced testimony from Dr. Wesley Skogan, a professor of political science at Northwestern University and a member of the University's Institute for Policy Research. According to Professor Skogan, research shows that establishments serving alcohol attract a significant amount of additional crime. Such establishments create the opportunity for crime by bringing the potential victim and the criminal together. Victims become more vulnerable because of alcohol's debilitating effects, thus creating an attractive situation for potential offenders.¹⁶

As it turns out, the criminological theory is well corroborated by data. For reasons that will become obvious shortly, it will be useful to separate the secondary effects studies into "earlier" and "later" categories conducted, respectively, prior to and after 2001. One major difference between earlier and later studies is that the former were government-sponsored for the most part while the latter were often sponsored by SOB plaintiffs. For reasons that are not yet apparent, secondary effects studies sponsored by governments and plaintiffs use different methodologies and arguments. When idiosyncratic differences are taken into account, however, the findings of the earlier and later studies are wholly consistent.

¹⁵ *Young v. American Mini-Theaters* (1976)

¹⁶ *City of Chicago v. Pooh Bah Enterprises, Inc.*, 865 N.E.2d 133 (Ill.2006)

2 Early Studies Corroborate the Theory

Scientific theory leads us to *expect* secondary effects in SOB neighborhoods and, in fact, *that is exactly what the early studies found*. Table 1 lists eighteen studies conducted over a 30-year period in rural, urban, and suburban settings; the studies span all regions of the U.S. and every conceivable SOB subclass. Despite this diversity, the eighteen studies have one thing in common: Each reports what we call the “consensus finding of the literature” – a substantively large, statistically significant crime-related secondary effect. Given the theoretical expectation and ample corroboration, *this consensus finding is a scientific fact*.

The eighteen studies listed in Table 1 are also *methodologically* diverse. Some of the studies use a before/after difference to estimate a secondary effect. Others use SOB-control differences for that purpose.¹⁷ Some of these SOB-control studies select control zones by “matching.” Others use statistical models (*e.g.* regression) to adjust irrelevant differences between the SOB and control zones. Methodological attacks on the literature typically focus on idiosyncratic design features of each study. Despite their methodological idiosyncracies, the studies all report remarkably similar findings. *This consensus renders any methodological challenge implausible*.

Ideally, one could read each of the eighteen studies listed in Table 1 and draw inferences from their similarities and differences. Given the broad consensus finding, however, there is little to learn from the minor details of specific studies. Our review will focus on SOB subclasses and, to a lesser extent, on methodological idiosyncracies. We revisit the methodological issues in subsequent sections.

2.1 SOB-Control Contrasts: Phoenix, 1979

In many respects, true experiments are the strongest designs.¹⁸ But since true experiments are not possible, crime-related secondary effect studies rely on *quasi-experimental designs*. Except for random assignment, quasi-experimental and true experimental designs use similar structures to control threats to validity. The strongest quasi-experimental design compares ambient crime risk at a site before and after the opening of an SOB. Before-after contrasts are

¹⁷ Our authority on quasi-experimental design is *Experimental and Quasi-Experimental Designs for Research* by D.T. Campbell and J.C. Stanley (Rand-McNally, 1966). Campbell and Stanley call before/after designs “pretest-posttest” designs; they call SOB-control designs “static group comparison” designs. In general, before/after comparisons are prone to fewer threats to internal validity and, hence, are “stronger” than SOB-control designs.

¹⁸ An experimental design controls common threats to validity by random assignment. To estimate the crime-related secondary effects of SOBs experimentally, *e.g.*, we would compile a list of the business sites in a jurisdiction and open SOBs in a random sample of sites. Random assignment (and hence, experimenting) is not possible, of course.

not always possible, unfortunately.

A somewhat weaker quasi-experimental design compares ambient crime risk at an SOB site to ambient crime risk at a control site. Though weaker in principle, SOB-control contrasts are often more practical. For this comparison to be valid, the two sites must be comparable on all other relevant causal variables. Barring outright dishonesty, the differences will be small and approximately random, thereby favoring neither side.

In 1979, the City of Phoenix conducted a study of crime-related secondary effects. To estimate the crime-related secondary effects of SOBs, the researchers compared crime rates in areas with SOBs to crime rates in “matched” control zones (*i.e.*, similar areas containing no SOBs). The comparisons are summarized in our Table 2.1. The property and personal crime rates reported in Table 2.1 were estimated from Uniform Crime Report (UCR) data. The percentages reported in the right-hand column (in red) are the secondary effect estimates derived from the crime rates. Compared to crime rates in the control zones, the UCR property crime rate was 39.8 percent higher; the UCR personal crime rate was 13.7 percent higher; and the UCR sex crime rate was 480.2 percent higher in the adult business areas. By any reasonable standard, these are *large, significant* crime-related secondary effects.

Table 2.1 - Secondary Effects in Phoenix, AZ

| | <i>SOB Areas</i> | <i>Control Areas</i> | <i>Secondary Effect</i> |
|----------------------------|----------------------|--------------------------|-----------------------------|
| <i>Property Crime Rate</i> | 122.86 | 87.90 | 139.8 % |
| <i>Personal Crime Rate</i> | 5.81 | 5.11 | 113.7 % |
| <i>Sexual Crime Rate</i> | 9.40 | 1.62 | 580.2 % |

Source: ADULT BUSINESS STUDY, City of Phoenix Planning Department, May 25, 1979; Table V

Over the last 30 years since this study was conducted, legislatures around the U.S. have accepted and relied upon its findings. Witnesses retained by SOBs and SOB plaintiffs, on the other hand, have argued that the 1979 Phoenix study is “fatally flawed” and that its findings are wholly implausible. It is our opinion that this position is incorrect. Although the design of this study leaves much to be desired – especially by today’s standards – many of the study’s methodological shortcomings function to produce smaller effect sizes. If anything, a stronger design would have produced a *larger* effect estimate.

2.2 Before-After Contrasts: Garden Grove, 1991

Prior to 1990, virtually all crime-related secondary effect studies compared crime rates in police districts with SOBs to crime rates in districts without SOBs.¹⁹ By contemporary standards, this is a weak study design. Existing police districts covered several square miles, *e.g.*, and sometimes contained several SOBs. Researchers handled these problems as best they could by matching and, infrequently, by statistical adjustment. To some extent, the wide use of weak “static group comparison” designs was dictated by economics. Prior to 1990, relatively few police departments had sophisticated information management systems.

Citing these methodological flaws, experts retained by SOB plaintiffs characterized these studies as illustrations of “shoddy research” whose findings are not to be trusted. Ironically, the methodological flaws in these early studies favor a *null* finding.²⁰ If anything, stronger designs would most likely have produced larger, more significant effect estimates. This issue aside, the “static group comparison” design assumes that SOB and control neighborhoods are equivalent on relevant crime risk factors. If this assumption is unwarranted, observed secondary effects cannot be attributed to the SOBs. The surest, simplest way to control this threat to validity is to use a before-after design.

In the late 1980s, Richard McCleary and James W. Meeker conducted a secondary effects study in Garden Grove, CA designed to correct many of the methodological “flaws” identified by SOB plaintiffs.²¹ The Garden Grove design used location-coded crime data so that crime rates could be estimated in 500-foot circles centered on an SOB, *e.g.*; the design used a full decade of crime data, so that relatively stronger before/after contrasts could be used; and finally, the design took advantage of several nearly ideal control businesses for controls. Due in large part to these design innovations, two decades later, the 1991 Garden Grove study is considered to be the most rigorous, valid study of secondary effects in the literature.²²

¹⁹ Studies in Los Angeles (1977), Amarillo (1977), Whittier (1978), St. Paul (1978), Phoenix (1979), Indianapolis (1984), and Austin (1986) used this design.

²⁰ “Null finding” means “finding that SOBs have no secondary effects.”

²¹ *Final Report to the City of Garden Grove: The Relationship between Crime and Adult Business Operations on Garden Grove Boulevard*. October 23, 1991. Richard McCleary, Ph.D. and James W. Meeker, J.D., Ph.D.

²² *E.g.*, in *City of Chicago v. Pooh Bah Enterprises* (224 Ill.2d 390, 865 N.E.2d 133): “According to Professor [Wesley G.] Skogan ... establishments serving alcohol attract a significant amount of additional crime ... by bringing the potential victim and the criminal together. Victims become more vulnerable because of alcohol’s debilitating effects, thus creating an attractive situation for potential offenders ... Professor Skogan discussed a number of studies and articles showing the relationship between alcohol consumption and crime ... The Garden

Examining ambient crime before and after an SOB opened, McCleary and Meeker found that crime risk rose whenever an SOB *opened* for business; when an SOB *closed*, crime risk fell. The validity of a before/after design requires that other plausible explanations for the rise and fall of crime be ruled out. The change may be a coincidence, *e.g.*; the observed patterns may reflect more general trends of crime rising or falling throughout the city. To control these common “threats to internal validity,” McCleary and Meeker replicated each before/after analysis for other SOBs in Garden Grove. If a rise or fall in ambient crime were a coincidence and reflective of a more general trend, the effect would be observed at other Garden Grove SOBs. Failing to observe the effect at these control sites, on the other hand, the effect could be attributed confidently to the newly opened SOB.

| | <i>Test Sites</i> | | | <i>Control Sites</i> | | |
|---------------------|-------------------|--------------|-------------|----------------------|--------------|-------------|
| | <i>Before</i> | <i>After</i> | | <i>Before</i> | <i>After</i> | |
| March, 1982 | 71 | 106 | 1.49 | 76 | 78 | 1.03 |
| March, 1986 | 31 | 68 | 2.19 | 80 | 92 | 1.15 |
| August, 1988 | 32 | 50 | 1.56 | 41 | 40 | 0.98 |
| Total | 134 | 224 | 1.67 | 197 | 210 | 1.06 |

Source: *Final Report to the City of Garden Grove*, pp. 26-28

Secondary effects for three business openings are reported in Table 2.2. When a new SOB opened, the total number of “serious” crimes in a 500-foot radius around the site increased, on average, 67 percent. To control for the confounding effects of city-wide crime trends, changes in police activity, and other common threats to internal validity, these before-after differences were compared to the analogous differences for the addresses of existing SOBs. Total “serious” crimes in a 500-foot radius around these “control” sites rose, on average, only six percent. The secondary effect observed when new SOBs open is, thus, substantively large and statistically significant.

Social scientists and their government clients learned two things from the 1991 Garden Grove study. First and foremost, the availability of relatively stronger before-after quasi-experimental designs produces the same ambient public safety hazards. The Garden Grove findings corroborate the findings in the Los Angeles (1977), Phoenix (1979), and Indianapolis

Grove study is particularly noteworthy because it showed that the combination of retail alcohol sales and adult uses created a higher incidence of crime than resulted from retail alcohol sales or adult uses operating in isolation.”

(1984) studies. Second, however, the 1991 Garden Grove study demonstrated how expensive a crime-related secondary effect study can be. We will have more to say about this at a later point.

2.3 What We Learned from the Early Studies

By contemporary standards, the earliest secondary effects studies were relatively unsophisticated. In particular, the early studies compared ambient crime risk in existing police precincts rather than in the smaller impact areas suggested by criminological theory. The use of weak quasi-experimental designs in these early studies was dictated by fiscal reality.²³ Yet despite their design weaknesses, these studies produced a consistent picture that came into sharper focus as stronger, more sophisticated studies added to the consensus finding of large, statistically significant crime-related secondary effects generated by SOBs.

²³ In their 1991 Garden Grove study, McCleary and Meeker spent more than \$100,000 (adjusted for inflation) for a stronger, more sophisticated quasi-experimental design. The study's cost was a minor scandal in some city departments.

3. Adult Cabarets

The relatively weak designs used in the early literature opened the door to charges by SOB plaintiffs that the strong consensus finding of the literature is an artifact. Had the studies used stronger designs, according to the plaintiffs' argument, all would have arrived at the opposite conclusion.²⁴ But in fact, the very consistency of the early literature rules out an artifactual explanation. First, virtually all design weaknesses bias the study in favor of the *null* finding. Second, more recent studies that use stronger, more sophisticated designs yield the same finding as the weaker, less sophisticated early studies.

In addition to their greater methodological sophistication, secondary effect studies conducted after 2001 differ from the earlier studies in two respects. First, many of the later studies were commissioned by SOB plaintiffs. Second, reacting to legal arguments by SOB plaintiffs, many of the later studies investigated the secondary effects of specific SOB subclasses. Following this recent development, our review of the modern literature focuses on adult cabarets, the oldest and most interesting SOB subclass. And since the plaintiffs in *J.L. Spoons* is an adult cabaret, it is the relevant SOB subclass.

In principle, estimating the secondary effect of an adult cabaret is straightforward. If we agree that live nude entertainment is the essential difference between adult cabarets and other businesses that sell alcohol by the drink (or "taverns"), the secondary effect can be estimated by comparing the ambient crime rates for adult cabarets and taverns. Although the differences between adult cabarets and taverns are often more complicated than conceded by this simplest, straightforward design, several studies have used taverns as controls for adult cabarets. *All find that adult cabarets have higher ambient crime rates than taverns.*

3.1 Greensboro, 2003

In 2003, Dr. Daniel Linz conducted a crime-related secondary effect study in Greensboro, NC.²⁵ Analyzing 911 calls-for-service Dr. Linz concluded 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 (*sic*) adult video/bookstore were located in high person and property crime incident "neighborhoods." We examined the "neighborhoods" and local areas surrounding

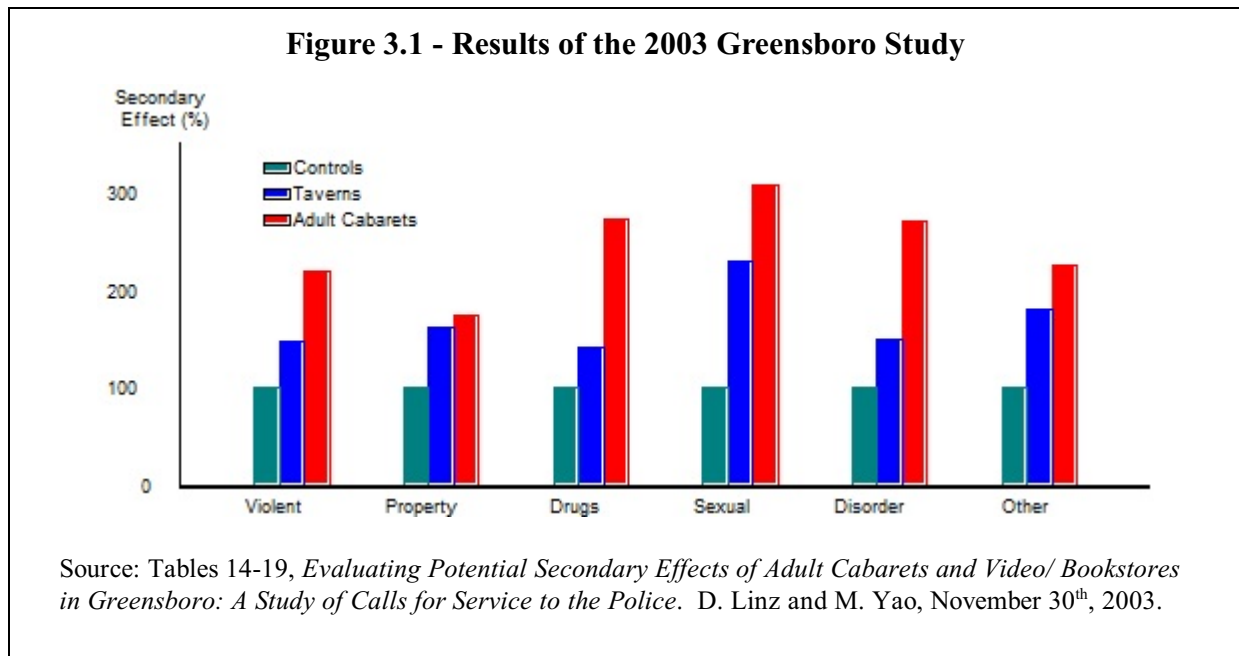
²⁴ The best-known statement of this view is B. Paul, D. Linz, and B.J. Shafer. Government regulation of 'adult' businesses through zoning and anti-nudity ordinances: de-bunking the legal myth of negative secondary effects. *Communication Law and Policy*, 2001, 6:355-391.

²⁵ *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.

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 ... (T)here 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.²⁶

Due to the technical nature of Dr. Linz’s statistical analyses, the City of Greensboro retained Richard McCleary to “translate” Dr. Linz’s numerical results into plain words.²⁷

Dr. Linz’s report was a difficult read, even for statisticians. The numbers on which his conclusion was based were scattered across 18 pages of computer output in an appendix. Few report readers consult appendices under any circumstances. In this particular instance, a critical reading of the report’s appendices required technical skills (that most of the report’s readers lack) and great tolerance for numerical detail. When the actual numbers were finally examined, it became clear that Dr. Linz had exaggerated the basis of his strongly-worded conclusion. Put simply, Dr. Linz’s numbers contradicted his words.



The results of Dr. Linz’s analyses are plotted in Figure 3.1. The green bars show the

²⁶ P. 3 (counting the title sheet as p. 1) of the Linz-Yao Greensboro *Study*.

²⁷ R. McCleary. *A Methodological Critique of the Linz-Yao Report: Report to the Greensboro City Attorney*. December 15, 2003.

ambient crime levels²⁸ for Greensboro’s “control” neighborhoods that have no taverns and no SOBs. The blue and red bars report the ambient crime levels for neighborhoods with taverns and neighborhoods with adult cabarets, respectively. To facilitate interpretation, we have fixed the ambient crime levels in control neighborhoods at 100 percent. This permits an easy interpretation of the ambient effects in tavern neighborhoods (blue bars) and adult cabaret neighborhoods (red bars) as multiples of the control neighborhood effects (green bars).

Since the social, demographic, and economic variables that are presumed to “cause” crime vary across neighborhoods, unadjusted crime levels may be deceiving. To control for these confounding effects, Dr. Linz adjusted his raw numbers with a statistical model whose technical details will not be discussed here. As the adjusted effects plotted in Figure 3.1 show, Dr. Linz found that ambient crime in tavern neighborhoods (blue bars) range from 148 percent (violent crimes) to 229 percent (sexual crimes) of the ambient crime in control neighborhoods. Since tavern neighborhoods are the criminological “gold standard” of ambient crime, that result was expected.²⁹ What Dr. Linz did not expect, however, was that adult cabaret neighborhoods (red bars) would have more crime than the tavern neighborhoods (blue bars).

Crime-related secondary effects in Greensboro’s adult cabaret neighborhoods ranged from 175 percent (for property crime) to 307 percent (for sexual crime) of the ambient crime levels in control neighborhoods. These effect estimates are large in every sense and, of course, they are not surprising. To us, the only surprise was that the estimates in Figure 3.1 were reported in a study commissioned by a consortium of SOB plaintiffs.

3.2 Daytona Beach, 2004

In 2004, Dr. Linz collaborated with Dr. Randy D. Fisher on a Daytona Beach secondary effect study.³⁰ With minor exceptions, the design of the Daytona Beach study was identical to the Greensboro design.³¹ Analyzing 911 calls once again, Drs. Linz and Fisher concluded that

²⁸ We use the term crime “levels” because, strictly speaking, crime “rates” are difficult to tease out of 911 calls.

²⁹ Most of the research on the relationship between taverns and ambient crime risk is due to our late colleague, Dennis W. (“Denn”) Roncek. Roncek and Pravatiner (1989) and Roncek and Maier (1991).

³⁰ *Evaluating Potential Secondary Effects of Adult Cabarets in Daytona Beach, Florida: A Study of Calls for Service to the Police in Reference to Ordinance 02-496* by Daniel Linz, Ph.D., Randy D. Fisher, Ph.D. and Mike Yao, April 7th, 2004. Dr. Fisher is Associate Professor of Psychology at the University of Central Florida.

³¹ Since the Daytona Beach SOBs were adult cabarets, Linz, Fisher, and Yao excluded bookstores and video arcades from the study. Instead of defining “neighborhoods” as Census

adult cabarets, had no significant crime-related secondary effects:

We are able to account for crime events in Daytona Beach with a moderately high level of accuracy using variables found by other researchers to be related to crime... The social disorganization variables and especially the presence of an (*sic*) alcohol beverage retail sale establishments in the blocks (that did not feature adult entertainment) accounts largely for this explanatory power. The presence of an adult cabaret in the census block explained only (*sic*) a trivial amount of variability in crime incidents when these other variables were considered ... 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 meaningful relationship between the presence of an adult cabaret in the neighborhood and crime events.³²

This conclusion was worded more cautiously than the conclusion in Greensboro. Indeed, the authors went so far in the Daytona Beach report as to admit that, as in Greensboro, the Daytona Beach results demonstrate statistically significant crime-related secondary effects:

There are analyses reported below where there are small but statistically significant relationships due to the exceptionally large N (sample size) employed in the analyses (at times over 1,100 census blocks)...[But] we favor “strength” over a technical “significance.”³³

This is an esoteric statistical issue, of course. In our opinion, Drs. Linz and Fisher misunderstand the assumptions of their model as well as the statistical problem of an “exceptionally large N” that, in their opinion, obviates the statistical model. Put simply, they are incorrect.

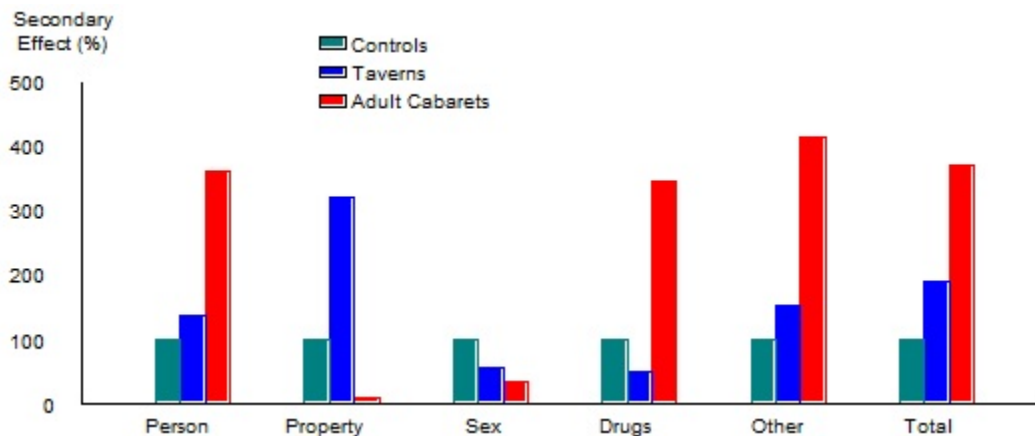
Notwithstanding the large *statistical* size of their effect estimates, the effect estimates reported by Drs. Linz and Fisher in Daytona Beach are *substantively* large. Figure 3.2 plots the results of the Daytona Beach analyses using the same conventions used in Figure 3.1 (for Greensboro). Once again the ambient crime levels in control neighborhoods (green) are fixed at 100 percent so that the levels in tavern neighborhoods (blue) and adult cabaret neighborhoods (red) can be interpreted as multiples of the controls. With two exceptions, adult cabaret neighborhoods have higher ambient crime levels than tavern neighborhoods. Given the well-known relationship between taverns and ambient crime, the Daytona Beach analyses corroborate the consensus finding of the literature. Like SOBs generally, adult cabarets pose large, statistically significant ambient public safety hazards.

Block Groups, in Daytona Beach, Linz, Fisher, and Yao used Census Tracts. The Greensboro and Daytona Beach designs are otherwise identical.

³² P. 36 (counting the title sheet as p. 1) of the Linz-Fisher-Yao Daytona Beach study.

³³ P. 23 (counting the title sheet as p. 1) of the Linz-Fisher-Yao Daytona Beach study.

Figure 3.2 - Results of the 2004 Daytona Beach Study



Source: Tables 7-19, *Evaluating Potential Secondary Effects of Adult Cabarets in Daytona Beach, Florida: A Study of Calls for Service to the Police in Reference to Ordinance 02-496*. D. Linz, R.D. Fisher, and M. Yao, April 7th, 2004.

Figure 3.2 speaks for itself. Tavern neighborhoods (blue) have 90 percent more total crime than control neighborhoods (green). Adult cabaret neighborhoods (red) have 270 percent more total crime than control neighborhoods (green). In substantive terms then, taverns have *large* secondary effects and adult cabarets have even *larger* secondary effects. The fact that these effect estimates are also *statistically* large adds little to our understanding of Figure 3.2.

The estimates *are* statistically large, of course – *i.e.*, statistically *significant* – and that poses a dilemma for Drs. Linz and Fisher. If the estimates were statistically small, Drs. Linz and Fisher could argue that they were due to chance (regardless of their substantive size). Since they cannot make this claim, Drs. Linz and Fisher argue that statistical significance is an artifact of an “exceptionally large N.” This is a specious argument, however, on two grounds. First, samples of 1,100 are not large enough to obviate the statistical model used by Drs. Linz and Fisher. But second, if samples of 1,100 *were* large enough to obviate the statistical model, as claimed, *all* of effect estimates would be statistically significant. In fact, of the 84 parameter estimates reported by Drs. Linz and Fisher, 42 are statistically significant and 42 are not. We will return to this issue in §3.4 below.

3.3 Palm Beach County, 2004

Comparing 911 calls to the addresses of nine adult and seven non-adult cabarets in Palm Beach County, FL, Dr. Terry A. Danner found that the adult cabaret addresses had fewer crime-related 911 calls (2.5 per month vs. 2.9 for SOB addresses) but more order-related 911 calls (3.1 per month vs. 2.0 for SOB addresses). Based on these comparisons, Dr. Danner concluded that the contrast “does not provide compelling evidence that the addition of various levels of nude dancing to the ‘nightclub type environment’ produces a pattern of crime and public disorder that

appears to be uniquely attributable to the adult cabaret category of business.”³⁴

Dr. Danner’s idea of comparing adult and non-adult cabarets makes good sense. The legal difference between adult and non-adult cabarets is, after all, the quantity and/or quality of clothing worn by employees. Changing the quantity and/or quality of clothing changes the non-adult cabaret into an adult cabaret and *vice versa*. The logical implication of this argument is that ambient crime rate differences between adult and non-adult cabarets must be due to nudity.

Aside from the idea of comparing adult and non-adult cabarets, however, Dr. Danner’s study is problematic in two respects. First, Dr. Danner used 911 calls to measure ambient crime risk. Although 911 calls are *correlated* with ambient crime risk, however, the correlation is weak at best. Second, Dr. Danner considered only the subset of 911 calls to the immediate addresses of the adult and non-adult cabarets. Calls to *nearby* addresses were excluded. If, as predicted by criminological theory, ambient crime risk “seeps out” across the adult cabaret neighborhood, excluding these calls will of course bias the secondary effect estimate in an unknown way.

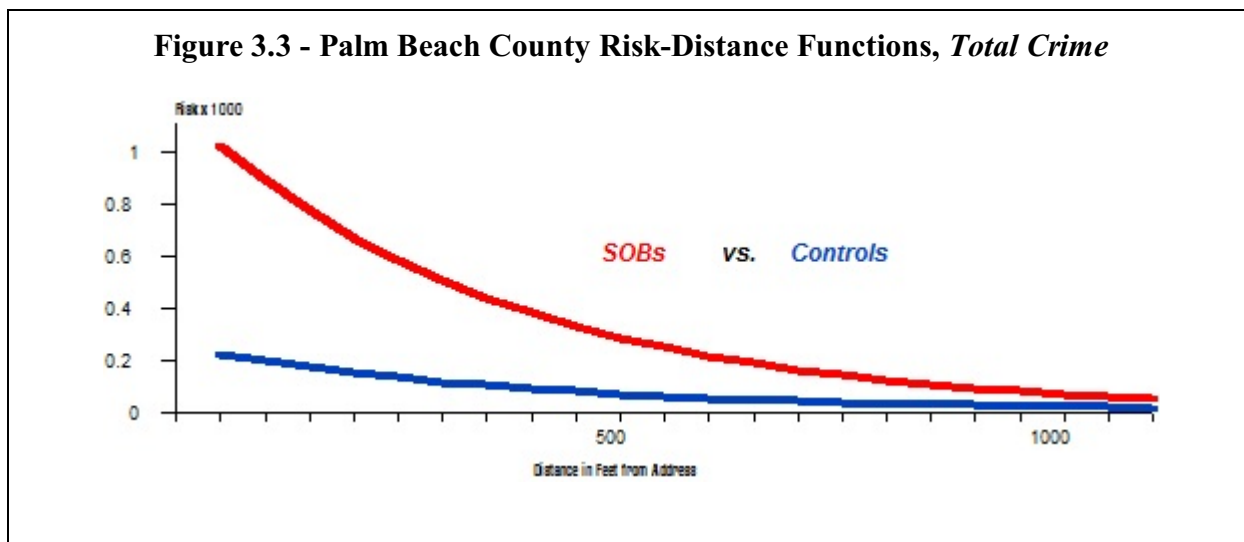
Drs. McCleary, Valerie Jenness, and James W. Meeker were retained by Palm Beach County to evaluate and, if necessary, replicate Dr. Danner’s study. Given the problematic use of address-specific 911 calls, they questioned Dr. Danner’s conclusion.³⁵ Their replication used the same adult and non-adult cabaret sites. Instead of using 911 calls, however, they used crime incident reports; and instead of restricting the analyses to the specific addresses, they included all crime incidents that occurred within 1,100 feet of the adult and non-adult cabarets.

The results of the replication are plotted in Figure 3.3. In terms of total crime, SOBs (in red, nine adult cabarets) and controls (in blue, seven non-adult cabarets) are both risky places. Moving toward an “average” site, whether SOB or control, victimization risk rises. Moving away, risk diminishes. With that said, compared to control sites, SOB sites are much riskier on average. How much riskier? At 500 feet (approximately one long city block), ambient risk at the SOB is four times greater. At 1,000 feet, the risk is substantially lower for all sites. But even at that distance, SOB sites are 3.5 times riskier than control sites.

³⁴ P. 8, *The Crime-related Secondary Effects of Adult Cabarets in Palm Beach County* by Terry A. Danner, Ph.D. Report submitted in *Palm Beach County v. Casablanca East*, CA-02-03813 AF, Circuit Court, Fifteenth Judicial Circuit, Palm Beach County, 2005. Dr. Danner is professor of criminal justice at St. Leo’s University.

³⁵ *Crime-Related Secondary Effects of Sexually-Oriented Businesses: Report to the County Attorney, Palm Beach County, Florida*. Valerie Jenness, Ph.D., Richard McCleary, Ph.D., and James W. Meeker, J.D., Ph.D. August 15, 2007.

Figure 3.3 - Palm Beach County Risk-Distance Functions, Total Crime



Although risk-distance plots have been widely used to document the ambient crime risks at “nuisance” sites, including SOBs,³⁶ few have included statistical significance tests of the plots. Given the quantity and quality of data that were available in Palm Beach County, Drs. Jenness, McCleary and Meeker were able to calculate confidence intervals for the risk-distance functions plotted in Figure 3.3. At the conventional 95 percent confidence level, both the SOB (red) and control (blue) functions are statistically significant as is their difference. Rejecting both null hypotheses then, the functions plotted in Figure 3.3 have the obvious interpretation.

3.4 Concluding Remarks

The three studies of adult cabarets reviewed here employ a range of designs. Two use 911 calls, one uses crime incident reports. Two compare SOB and control neighborhoods, one compares adult and non-adult cabarets. Yet all three studies support the conclusion that adult cabarets have large, statistically significant secondary effects.

Nevertheless, there is a remarkable difference in how the studies interpret their findings. Whereas the Greensboro and Daytona Beach studies find large, statistically significant secondary effects, the authors give their findings the opposite interpretation. The secondary effect study summarized in Figure 3.2 was commissioned by the plaintiffs in *Daytona Grand v. City of Daytona Beach*.³⁷ Drs. Fisher and Linz used a two-prong argument to challenge the City’s secondary effects evidence. First, they argued that the studies relied on by the City were

³⁶ See, e.g., *An Analysis of the Relationship between Adult Entertainment Establishments, Crime, and Housing Values*. M. McPherson and G. Silloway. Minnesota Crime Prevention Center, Inc. October, 1980.

³⁷ *Daytona Grand, Inc. v. City of Daytona Beach*, 410 F. Supp. 2d 1173 (M.D. Fla. 2006).

methodologically flawed. Second, they claimed local data showed that neighborhoods with adult businesses had the same number of 911 calls as other neighborhoods. To refute these arguments, the City cross-examined the experts. The trial court was unimpressed, however, and struck down those parts of the Daytona Beach ordinance that regulated nudity.

The trial court's decision in *Daytona Grand* provoked a mild panic among Florida governments. Two years later, however, the U.S. Eleventh Circuit reversed the trial court.³⁸ The Eleventh Circuit decision reaffirmed the *Renton* standard in the most crucial respect: If the government's interpretation of its secondary effects evidence is "reasonable," there is no need to show that its interpretation is the *only* reasonable interpretation. The fact that plaintiffs can draw alternative conclusions from the evidence does not bar the government from "reaching other reasonable and different conclusions."

The Eleventh Circuit addressed three other relevant issues. First, the panel explicitly rejected the methodological arguments of Paul, Linz and Shafer.³⁹ Second, the panel rejected the use of 911 calls to demonstrate the absence (but *not* the presence) of a secondary effect. Third, the panel noted, as we have, that several of the secondary effect estimates reported by Drs. Linz and Fisher were statistically significant.

The experts are no doubt correct that factors other than the presence of adult theaters affect crime rates in Daytona Beach: crime is plainly caused by many factors. But that does little to undermine the City's conclusion that adult theaters *also* affect crime rates, especially when the experts' own analysis shows a statistically significant correlation between adult theaters and increased crime in half of the areas in the study.⁴⁰

This observation by the Eleventh Circuit panel is consistent with Figure 3.2 above.

³⁸ *Daytona Grand, Inc. v. City of Daytona Beach*, 490 F.3d 860 (11th Cir. 2007).

³⁹ Paul, Linz, and Shafer (2001) argue that the government's secondary effects evidence must satisfy *Daubert* admissibility criteria.

⁴⁰ *Daytona Grand, Inc. v. City of Daytona Beach* (2007) at 47-48

4. The Opinions of Daniel Linz, Ph.D.

The factual predicate of the State of Ohio’s Rule 52 includes secondary effects studies conducted in other jurisdictions and times. In Dr. Linz’s opinion, these studies are not reliable evidence:

THERE IS NO RELIABLE EVIDENCE FROM STUDIES CONDUCTED IN OTHER COMMUNITIES THAT ADULT BUSINESSES CREATE SUFFICIENT ADVERSE SECONDARY EFFECTS TO JUSTIFY THE TYPE OF RESTRICTIONS INTRODUCED BY THE STATE OF OHIO.⁴¹

The word “sufficient” and the phrase “the type of restrictions” seem to distinguish this opinion from the opinions expressed by Dr. Linz in other suits. In addressing this opinion, we assume that the word “sufficient” does not imply a logical property of the evidence; and that “the type of restrictions” is roughly synonymous with “regulations like Rule 52 that govern alcohol and adult entertainment.” With that understanding, we disagree with Dr. Linz’s opinion. The voluminous, diverse collection of secondary effects studies relied on by the State supports the consensus view that SOBs have large, significant secondary effects; and that these effects are aggravated by proximity to alcohol.

In addition to its reliance on studies conducted in other jurisdictions and times, Dr. Linz faults the State of Ohio for ignoring secondary effects studies commissioned by SOB plaintiffs. Specifically:

ABSENT FROM THE STATE OF OHIO’S JUSTIFICATION FOR RULE 52 ARE A LARGE NUMBER OF SCIENTIFICALLY SOUND STUDIES THAT SHOW NO ADVERSE EFFECTS OF ADULT BUSINESSES.⁴²

Assuming that Dr. Linz refers to secondary effects studies commissioned by SOB plaintiffs and conducted by Dr. Linz and his colleagues, we disagree with this opinion as well. None of these studies is more “scientifically sound” (or reliable) than the government-sponsored studies relied on by the State. The results or findings reported in these studies are entirely consistent with the results or findings reported in government-sponsored studies.

4.1 Dr. Linz’s Methodological Authority

Both of Dr. Linz’s opinions rest on the authority of a 2001 article written by Drs. Paul and Linz, and Mr. Shafer. The methodological rules endorsed in the Paul-Linz-Shafer article are derived from the four criteria suggested by Justice Blackmun’s opinion in *Daubert v. Merrell*

⁴¹ Report of Daniel Linz, Ph.D., p. 1.

⁴² Report of Daniel Linz, Ph.D., p. 1.

Dow. Dr. Linz paraphrases these criteria as:

Studies of secondary effects should be examined in order to determine whether they answer the following questions which reflect important principles guaranteeing reliable information. Colloquially, these questions are: (1) “Compared to what?” (2) “Is this just a one-time fluke?” (3) “Is crime measured according to a reliable source?” and “did the government go looking for more crime to justify its legislation?” and (4) “Did the investigators talk only to people who would give them answers they wanted to hear?” These criteria are neither difficult nor cumbersome to apply, nor are they novel in nature – they are, however, essential features of any project attempting to gain reliable knowledge about secondary effects.”⁴³

We disagree with Dr. Linz’s opinion both in general and in specific. In general, legislatures would indeed find it “cumbersome” to apply any set of strictly academic methodological rules – much less this set of rules – to the fact-finding process. Forcing a legislature to conduct its fact-finding in compliance with academic rules would bring representative democracy to a halt. The courts seem to have arrived at this same conclusion.⁴⁴

Judged by purely academic standards, the specific methodological rules endorsed by Paul-Linz-Shafer are not a necessary-sufficient methodological canon. The rules are not derived from the primary authorities on quasi-experimental design or statistics but, rather, from out-of-print sophomore-level psychology texts.⁴⁵ Although the article is well known to SOB plaintiffs, it has had virtually no impact on the social science literature. Excluding citations by Dr. Linz and his colleagues, as of May, 15, 2007, the Paul-Linz-Shafer article had been cited only twice in peer-reviewed journals. We know of no natural or social scientists who use or endorse the Paul-Linz-Shafer methodological canon. Although Dr. Linz claims that the U.S. Tenth Circuit Court of Appeals “specifically cited [the Paul-Linz-Shafer] article ... as a basis for *overturning* the evidence relied upon” by the plaintiff in *Abilene Retail #30, Inc. v. Board of Commissioners of Dickinson County, Kansas*, we do not find a citation to the Paul-Linz-Shafer article in the Court’s published decision.

⁴³ Report of Daniel Linz, Ph.D., p. 3.

⁴⁴ See, e.g., *G.M. Enterprises, Inc. v. Town of St. Joseph, Wisconsin*.

⁴⁵ In discussing sampling, e.g., Paul-Linz-Shafer cite the authority of *The Practice of Social Research 8th Ed.* (E.R. Babbie, Wadsworth, 1998) and *Approaches to Social Research, 2nd Ed.* (R.A. Singleton et al., Oxford, 1993). Both texts were written explicitly for sophomore-level social psychology courses. Both were out-of-print by 2001.

4.2 The Paul-Linz-Shafer Validity Tests in Practice

Dr. Linz's methodological critiques of government-sponsored secondary effects studies consist of *identifying* some weakness in a study's design and then *characterizing* the weakness as a "fatal flaw." This style of argument reflects a basic misunderstanding of quasi-experimental design.⁴⁶ Since *all* secondary effect studies use quasi-experimental designs – including those conducted by Dr. Linz and his colleagues – *all* have potential shortcomings. Most shortcomings have trivially small impacts on the study's findings or, else, bias the findings in favor of a null finding – in favor of the plaintiff's argument. But a more serious problem with Dr. Linz's methodological critiques of government-sponsored secondary effects studies is that the Linz-Paul-Shafer validity tests are arbitrary in practice.

4.2.1 The Paul-Linz-Shafer Rules are Arbitrary in Practice

To be of any use, a methodological rule must have objective (numerical) benchmarks that characterize the relative validity of a study design. Lacking objective benchmarks, a rule cannot be used *a priori* to design a study with optimal validity or *a posteriori* to evaluate the validity of a study's findings or results. The four Paul-Linz-Shafer rules lack objective benchmarks.

The Paul-Linz-Shafer "compared-to-what" test illustrates this point. The gist of this test is that SOB and control areas must be "statistically adjusted" (or "matched") to control for crime risks unrelated to the hypothesized secondary effects of SOBs. Assuming that the design *uses* control areas – and not all quasi-experimental designs use control areas – no methodological authority would disagree with this rule. As defined by Paul-Linz-Shafer, however, the rule is wholly arbitrary. In their critique of a 1979 Phoenix study, *e.g.*, Paul-Linz-Shafer declare the SOB-control matching to be "unacceptable."⁴⁷ They reveal no objective numerical threshold value that separates "acceptable" SOB-control matching and "unacceptable" matching.

The Paul-Linz-Shafer "one-time fluke" test suffers from the same lack of objectivity. The sense of the "one-time-fluke" test is that the secondary effect (*e.g.*, ambient crime risk) should be estimated over a long enough period of time to ensure that a finding satisfies conventional levels of statistical confidence. Again, while no methodological authority would disagree with this rule, Paul-Linz-Shafer do not specify the length of time required to pass the "one-time-fluke" test. In the 1979 Phoenix study, Paul-Linz-Shafer decide that the duration of the study was not "sufficient" to pass the test.⁴⁸ But they do not tell us how long a duration would be required to

⁴⁶ "Quasi-experimental" refers to any design lacking random assignment. Our authorities on quasi-experimental design are Campbell and Stanley (1966), Cook and Campbell (1979), and Shadish, Cook, and Campbell (2002).

⁴⁷ Paul, Linz, and Shafer (2001), p. 380

⁴⁸ Paul, Linz, and Shafer (2001), p. 380

pass the test. One year? Five years?

In this particular instance, fortunately, the Central Limit Theory provides an objective benchmark for the “one-time fluke” test. Consideration of this objective benchmark highlights another weakness of the Paul-Linz-Shafer test: *studies that fail the “one-time fluke” test invariably yield findings that favor the plaintiff.*

4.2.2 Violations of the Paul-Linz-Shafer Tests Favor the Null Finding

Since every (quasi-experimental) secondary effects study has several methodological shortcomings, a proper methodological critique must show, first, that the consequences of the shortcoming are *not trivially small*. If the consequences of a methodological shortcoming are trivially small or benign, the shortcoming can be ignored without cost to either side. Second, assuming that the consequences of a methodological shortcoming are non-trivial, a critique must show that the consequences create a *bias against the plaintiff’s interests*. If the consequences create a bias in favor the defendant’s interests, on the other hand, the plaintiff’s experts can ignore the methodological shortcoming.

As it turns out, the methodological shortcomings associated with the four Paul-Linz-Shafer validity tests almost always bias a study’s results in favor of the null hypothesis. That is, *they almost always work to make the secondary effects of SOBs appear smaller than they really are*. In the case of the “one-shot fluke” test, *e.g.*, the Central Limit Theorem implies that the statistical significance of a secondary effect is inversely proportional to the duration of the study. The shorter the study duration, in other words, the less significant the finding. When a study fails the “one-time fluke” test, the study is likely to conclude (erroneously) that there is no difference between SOB and control areas – *i.e., no significant secondary effects*.

Likewise, studies that fail the Paul-Linz-Shafer “compared-to-what” test are biased in favor of the plaintiff’s interests and, hence, are likely to find no differences between SOB and control areas. A mathematical proof of the postulate that “matching” errors inflate the standard error of the SOB-control difference is straightforward.⁴⁹ As the proportion of total variance due to matching error rises – which is to say, as the quality of the match weakens – secondary effect estimates approach zero while the standard error of the estimate inflates. As a result, the study is likely to find (erroneously) secondary effects that are substantively and statistically small.

4.2.3 The “Looking-for-More-Crime” Test

The Paul-Linz-Shafer “looking-for-more-crime” test is not a validity test in the normal sense but, rather, a claim that an SOB-control difference in ambient risk can be a spurious artifact of proactive policing. In a 1977 Los Angeles study, Paul-Linz-Shafer note that, while SOB areas

⁴⁹ See, *e.g.*, Table 1 and supporting text in Cochrane, W.G. Matching in analytical studies. *American Journal of Public Health*, 1953, 43, 684-691.

had higher crime rates than matched control areas, the difference could have been an artifact of heightened police presence in the SOB areas:

Although the findings...suggested high levels of criminal activity [in SOB areas], any implication that this is connected to the presence of adult businesses is invalidated by the fact that the researchers admitted to “stepped up” surveillance within these areas. Put simply, the police most likely found greater amounts of crime in the adult establishment areas because they were trying harder to find it.⁵⁰

Most criminologists would disagree. Although a “looking-for-more-crime” hypothesis *might* be plausible for *some* crimes, it is *wholly implausible* for *most* crimes. Vice crimes (prostitution, drugs, *etc.*) often arise through the activity of specialized undercover police who are indeed “looking-for-more-crime.” Crimes like robbery, vandalism, and auto theft, on the other hand, are ordinarily initiated – and hence, known to the police – through victim complaints. Higher levels of police presence in an area generally *reduces* the ambient risk of these crimes. The 1977 Los Angeles study reports higher levels of homicide, robbery, and assault in SOB areas.

4.2.4 The “Known Error Rate” Rule

Though not one of their four explicit validity tests, Paul-Linz-Shafer dismiss the findings of “nearly all” government-sponsored studies on the grounds that the studies failed to report “an error rate—a test of the reliability of findings in science.”⁵¹ The sense of the “known error rate” rule is that studies should demonstrate the reliability of their findings by reporting error rates (or probabilities). Assuming that error rates are defined and appropriate – and this is not always the case – no methodological authority would disagree. No methodological authority would endorse the manner in which Paul-Linz-Shafer apply the rule, however.

Our first objection is that Paul-Linz-Shafer apply the “known error rate” only to studies that report significant secondary effects. Studies that report null findings – *i.e.*, that fail to find significant secondary effects – are given an implicit pass on the test. Since this issue is relatively technical and esoteric, we postpone its discussion until §6 below.

Our second objection is that, as Paul-Linz-Shafer apply the rule, secondary effects evidence not collected with formal quasi-experimental methods is excluded. No matter how useful, informative, or valid the evidence might be, under the Paul-Linz-Shafer rule, secondary effects evidence collected with other methods is non-admissible.

Among the excluded categories of secondary effects evidence is first-hand observations.

⁵⁰ Paul, Linz, and Shafer (2001), p. 379

⁵¹ Paul, Linz, and Shafer (2001), p. 355. “Known error rate” is one of the four criteria suggested by Justice Blackmun in *Daubert*.

Especially in cases where formal quasi-experimental studies are not feasible or economical, secondary effects can be documented with first-hand observations or archival records. Because it is difficult (if not impossible) to calculate an error rate for these data, the Paul-Linz-Shafer rule excludes them. Most scientific fields admit these data, however. The medical case studies and ethnographies published in prestigious scientific journals (*New England Journal of Medicine*, *The Lancet*, etc.), e.g., are collected without benefit of probability sampling, controls, or other formal quasi-experimental design structures.

Although it is difficult (if not impossible) to calculate an error rate for observational data, this is not to say that there are no methodological conventions for assessing their reliability. On the contrary, scientists, clinicians, legislators, and judges use the same obvious criteria to assess the relative credibility (*i.e.*, reliability) of first-hand observations. Other things equal, e.g., first-hand observations are more reliable than second-hand observations; expert observations are more reliable than non-expert observations; corroborated observations are more reliable than uncorroborated observations; and so forth.

4.3 Criminological Theory: Routine Activity Theory of Hotspots

In §1 above, we outlined the routine activity theory of hotspots. Ordinarily attributed to Sherman, Gartin, and Buerger (1980; Cohen and Felson, 1979; 1980), this widely used – and widely accepted – criminological theory explains why predatory offenders are attracted to SOB sites. Dr. Linz argues that the routine activity theory of hotspots does not apply to SOB sites:

While routine activities theory can be very useful in explaining how crime can be distributed spatially or geographically in a given area, McCleary's application of the theory to sexually oriented businesses is unwarranted for several reasons: 1) The hotspot routine activities theory as applied to adult businesses *assumes* that the sexually-oriented businesses are, indeed, a [sic] crime hotspots without any empirical evidence that this true; 2) the theory focuses only on concentric circles in the area immediately surrounding the adult business and fails to take into account the myriad characteristics of the larger neighborhood in which the sexually-oriented business is situated that may account for crime; and, 3) the theory, as stated, cannot be falsified.⁵²

We disagree with each of these points. In fact, 1) the earliest research on hotspots includes SOB sites among the most serious hotspots. 2) Virtually all spatial criminologists, including those cited by Dr. Linz, use concentric circular zones to model ambient crime risk. And 3) the routine activity theory of hotspots is eminently testable (or in Dr. Linz's words, *falsifiable*).

⁵² Report of Daniel Linz, Ph.D., p. 6

4.3.1 Evidence that SOBs are Hotspots

In Dr. Linz's opinion the prevailing routine activity theory of hotspots is predicated on the "not empirically sound" assumption that SOBs are crime hotspots. Dr. Linz's opinion stands on three distinct (but related) claims:

- **Legal** - Dr. Linz argues that recent U.S. Circuit Court of Appeals decisions have held that there is no reason to assume that SOBs are hotspots.
- **Empirical** - Dr. Linz argues that the Minneapolis study by Sherman *et al.* (1989) did not identify SOBs as hotspots.
- **Methodological** - Dr. Linz argues that primary methodological authorities require that exploratory methods must identify SOBs as hotspots prior to conducting hypothesis tests.

We disagree not only with these three specific claims but, also, with the relevance of the claims to any question about the prevailing routine activity theory of hotspots.

Legal - Under the rubric "The Courts do not assume adult businesses are hotspots of crime," Dr. Linz argues that two recent Seventh Circuit Court of Appeals decisions support his opinion. We have read both decisions and disagree with Dr. Linz's interpretation. Although we claim no legal expertise, in our reading, the Seventh Circuit panel decided both cases on legal grounds, not methodological or criminological grounds.

Empirical - It is *not* difficult to find published research that identifies SOBs as crime hotspots. Dr. Linz's contrary opinion is incorrect. In fairness, Dr. Linz's opinion relies on the opinion of Dr. Jeffrey M. Cancino whose opinion rests on two published articles:

[R]esearch conducted by Sherman *et al.* (1989) revealed that three and a half percent of the intersections and some addresses were overwhelming responsible for most crime. These addresses included department stores, 24-hour convenient [sic] stores, liquor stores, public housing projects, apartment buildings, and homeless shelters in Minneapolis. Weisburd *et al.* (2004) found that between four and five percent of street segments in the city accounted for 50 percent of crime incidents for each year over 14 years. As Cancino (2009) has pointed out, these studies did *not* identify adult businesses as hotspots. Nevertheless, McCleary assumes confirmation that adult businesses are hot-spots.⁵³

Since Dr. Linz does not claim that he personally read these articles, we assume that he relied on Dr. Cancino's reading. If Dr. Cancino read Sherman *et al.* (1989), however, he could not have read it thoroughly. As Dr. Linz claims, the list of Minneapolis hotspots identified by Sherman *et*

⁵³ Report of Daniel Linz, Ph.D., p. 6

al. included “department stores, 24-hour convenient [sic] stores, liquor stores, public housing projects, apartment buildings, and homeless shelters.” But the list also included SOBs. *Three of the 25 hottest spots in Minneapolis were SOBs.*⁵⁴

Although Dr. Linz’s mistaken impression of the Minneapolis research conducted by Sherman *et al.* might be attributed to a misreading (though perhaps by Dr. Cancino), his mistaken impression of the research conducted by Weisburd *et al.* (2004) has no obvious explanation. Weisburd *et al.* had *no* interest in identifying hotspots and did *not* report a list of hotspots. Dr. Linz’s claim (though perhaps due to Dr. Cancino) that Weisburd *et al.* “did *not* identify adult businesses as hotspots,” he is not *literally* incorrect. But it is misleading. Weisburd *et al.* neither *looked for* nor *found* any hotspots.

Methodological - Informed by his vicarious misreadings of Sherman *et al.* (1989) and Weisburd *et al.* (2004), Dr. Linz argues that, as a methodological convention, the routine activity theory of hotspots cannot be applied to SOBs until after an exploratory spatial analysis has identified a crime hotspot centered on an SOB. Dr. Linz’s authority for this opinion is a single sentence on the first page of Anselin *et al.* (2000):

Special attention is given to some practical and accessible methods of exploratory data analysis that arguably should be the starting place of any empirical analyses of the relationship of place to crime.⁵⁵

Dr. Linz interprets this sentence to mean that a *confirmatory* (*i.e.*, hypothesis testing) analysis must necessarily be preceded by an *exploratory* (*i.e.*, descriptive) analysis. Anselin *et al.* (2000) do not make this argument in this sentence, however, or anywhere else. Ironically, Dr. George E. Tita is a co-author of the Anselin *et al.* (2000) chapter. An affidavit by Dr. Tita is attached to this report.

4.3.2 Concentric Circles

Dr. Linz’s second argument against the routine activity theory of hotspots concerns the use of concentric circular zones centered on the SOB. Some part of Dr. Linz’s argument stems from his conviction that secondary effects estimates must be adjusted for the effects of social disorganization variables; we address this part of Dr. Linz’s argument in §4.4 below. Another part of Dr. Linz’s argument is based on geometrical properties. Specifically,

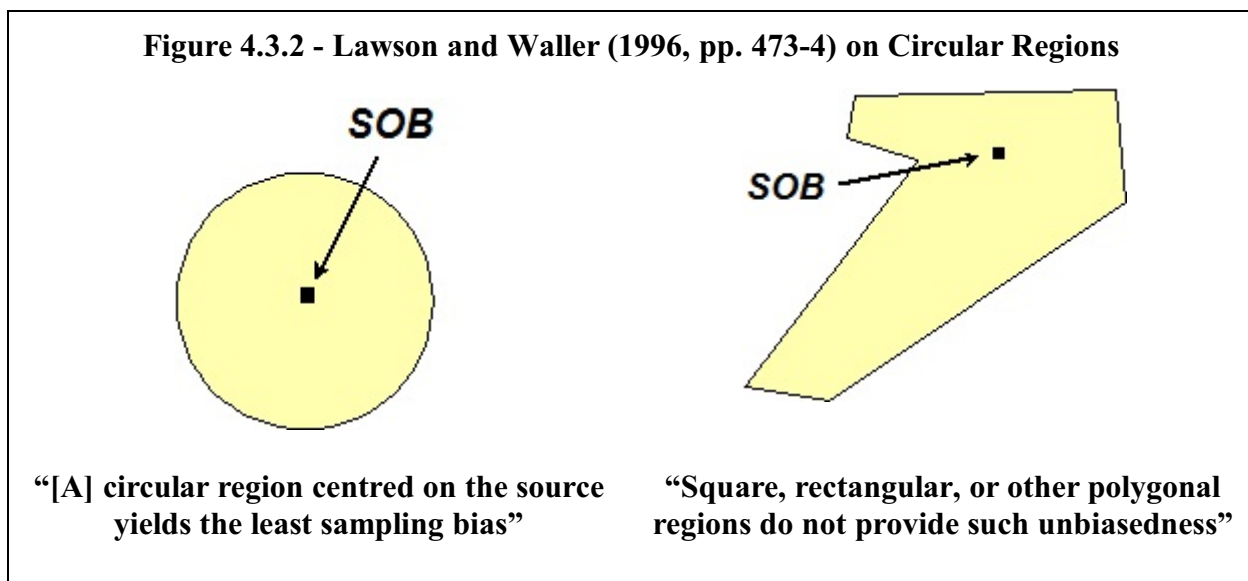
The epidemiology literature concerning sources of pollution and physical health is instructive in illustrating the weakness of the small area analysis (*e.g.*, concentric zones) ... According to Lawson and Waller (1996: 473), when examining risk (in

⁵⁴ See Sherman *et al.* (1989), Table 5, p. 43.

⁵⁵ P. 213, Anselin *et al.* (2000).

this case crime) to a single source of pollution (in this case an adult business), and the assumption is that distance is a surrogate for risk, then a circular region (e.g., concentric zones) centered on the source provides the least sampling bias. However, if the source of pollution is not central to the area (Diggle, 1990) or population structure and other factors dictate the area (Lawson, 1995), then a circular zone has no advantage over more traditional square, rectangular, or polygon shapes (e.g., census tracts, block-groups, blocks). Use of polygons and tabular data are better able to account for many of the truncated factors that small area studies (e.g., concentric zones) tend to exclude (Lawson and Waller, 1996: 473).⁵⁶

Dr. Linz argues that, *under some circumstances*, concentric circular zones offer no statistical advantage over polygonal zones. We agree. But *under most circumstances* – and especially for testing hotspot null hypotheses – concentric circular zones yield *unbiased* estimates of ambient crime risk that “other polygonal regions do not.”⁵⁷ If circumstances dictate the use of polygonal regions, “one should make some effort to provide ‘similar’ sampling detail in all directions.”⁵⁸ But if the regions are census tracts, this is practically impossible.



If Dr. Linz has read the authority that he cites for his idiosyncratic opinions on circular sampling regions – and this is not clear – he has misunderstood the authorities, particularly “Lawson and Waller (1996: 473).” Figure 4.3.2 uses hypothetical map regions to illustrate the

⁵⁶ Report of Daniel Linz, Ph.D., p. 8

⁵⁷ Lawson and Waller (1996), p. 473

⁵⁸ Lawson and Waller (1996), p. 474

only point made by Lawson and Waller: *Circular sampling regions yield unbiased estimates of crime risk; regular and irregular polygonal regions do not.* In light of the well-known statistical properties of circular regions, criminologists have *always* used concentric circles to analyze spatial distributions of crime. This tradition extends back to the early “delinquency area” studies of Shaw and McKay (1942[1929]) – which Dr. Linz cites as the authority for his research – as well as the replications by Hayner (1933) and Schmid (1960). Ironically, studies that Dr. Linz has submitted in *J.L. Spoons*, particularly Paul and Linz (2002) and Linz *et al.* (2004), also use circular regions.

4.3.3 The Routine Activity Theory of Hotspots Is Testable

In our description of the routine activity theory of hotspots, we noted that, due to potential embarrassment, SOB patrons are reluctant to report victimizations to the police. This makes the patrons attractive targets to predatory criminals. In Dr. Linz’s opinion,

This makes [the] theory impossible to falsify. That is, if one were to attempt to empirically test the theory and find that there was substantial crime risk associated with the adult business, we would conclude that we found evidence for the theory. However, even if one were to attempt to empirically test the theory and found no evidence that the adult business was associated with crime, one could explain this finding by asserting that the only reason an effect was not found was because of the propensity of the victims to not report their victimization to the police. Therefore, the theory has no way of being proven false--any contrary evidence can always be “explained away” by the “reluctance to report” corollary to the theory.⁵⁹

We disagree. Criminologists have known for at least fifty years that *some* witnesses and *some* victims are reluctant to report *some* crimes to the police. In the 2004 Charlotte study reviewed at §4.5.2 below, *e.g.*, Dr. Linz and colleagues note the possibility of under-reporting:

Perhaps victims of crime in areas surrounding adult clubs are not motivated to report crime incidents to the police. If this were the case, there may not be stable crime reporting across study and control sites. It could be that, compared to the control sites, more of the crime that occurs in the adult dance club zone goes unreported. It seems plausible that many of the victims of crime in these areas might not want to draw attention to themselves.⁶⁰

The threat of under-reporting is not uniquely criminological. The threat of under-reporting arises whenever social scientists study sensitive issues. People are reluctant to discuss their purchases of “embarrassing” merchandise (Goodwin, 1992), *e.g.*; their sexual histories, especially STDs (Fenton *et al.*, 2001); their tax returns, especially “cheating” (Grasmick, Bursik, and Cochrane,

⁵⁹ Report of Daniel Linz, Ph.D., p. 8

⁶⁰ Report of Daniel Linz, Ph.D., p. 8

1991), and their smoking habits (Ebert and Fahy, 2007). In each of these fields, social scientists recognize the threat of under-reporting and take steps to control it. Criminologists control the threat either by design or by incorporating the errors into the statistical models.

Since most studies *find* secondary effects, Dr. Linz's complaint is moot. Under-reporting *might* be relevant when a study reports a null findings, of course, but we know of no study where a null finding was ruled out on under-reporting grounds. Since Dr. Linz cites no such study, we assume that his complaint is hypothetical.

4.4 Social Disorganization Theory Cannot Explain Secondary Effects

In Dr. Linz's opinion, the criminological theory of social disorganization is more relevant to secondary effects phenomena than the prevailing theory described in §1 above.⁶¹ But in fact, social disorganization theory is wholly *irrelevant* to secondary effects phenomena. An authority cited by Dr. Linz describes the two related questions addressed by the theory:

The theory of social disorganization speaks not only to the ability of a community to achieve common values (*e.g.*, to defend itself against predatory victimization), but also to community processes that produce offenders.⁶²

This is the consensus view. Criminologists use the theory of social disorganization to explain why well organized communities produce relatively few delinquents: The institutions that define a well organized community exert a higher degree of control on youth while focusing resources on predatory criminals who live outside the community. *Since SOBs are located in commercial and/or industrial zones, social disorganization has nothing to say about secondary effects.*

4.4.1 Social Disorganization Theory

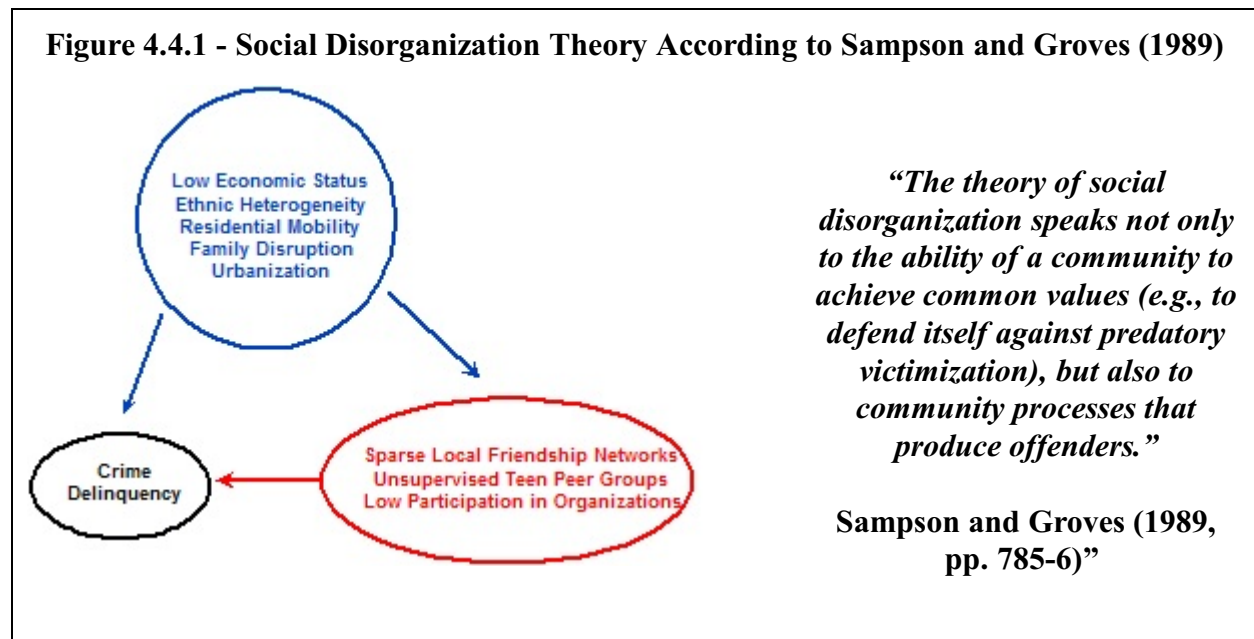
Social disorganization theory was developed in the 1920s by sociologists of the "Chicago School" particularly Clifford R. Shaw and Henry D. McKay. Shaw and McKay (1942 [1929]) posited that residential instability, ethnic homogeneity, and persistent poverty would produce socially disorganized communities which would, in turn, produce higher rates of delinquency. Subsequent elaborations of the theory focused on intervening structures (Bursik and Grasmick, 1993; Kornhauser, 1978; Simcha-Fagan and Schwartz, 1986; Taylor, Gottfredson, and Brower, 1984; Warner, 2007) and on the ability of community residents to organize and to use their

⁶¹ For his opinions on social disorganization theory, Dr. Linz cites *Testing Theories of Crime and Deviance* (Charis E. Kubrin, Thomas D. Stucky, and Marvin D. Krohn, Oxford University Press). We were unable to locate a book with this title. We assume that the correct citation is *Researching Theories of Crime and Deviance* by the same authors and publisher. The website for Oxford University Press describes this book as an undergraduate text.

⁶² Sampson and Groves (1989), pp. 785-6.

“collective efficacy” to control crime and delinquency (Sampson and Groves, 1989; Sampson, Raudenbush, and Earls, 1997).

Figure 4.4.1 shows a causal model of the social disorganization theory from Sampson and Groves (1989).⁶³ According to Sampson and Groves (1989) five neighborhood characteristics (Low Economic Status, Ethnic Heterogeneity, Residential Mobility, Family Disruption, and Urbanization) *cause* three social disorganization factors (Sparse Local Friendship Networks, Unsupervised Teen Peer Groups, and Low Participation in Organizations) which, in turn, *cause* crime and delinquency. In addition to their *indirect* causal effects operating through the social disorganization factors, the five neighborhood characteristics have *direct* causal effects on crime and delinquency.



The core mechanism of social disorganization theory is the breakdown of neighborhood social institutions. Socially disorganized neighborhoods have disproportionately more short-term (transient) residents. Because poorer residents are less able to move, poverty becomes endemic; families break down. Informal institutions, including friendship networks and social organizations (churches, PTAs, *etc.*), wither. The weakened neighborhood’s institutions no longer exert sufficient control on youths; rates of delinquency and crime rise.

We seldom see this core mechanism in SOB neighborhoods. Located in commercial and

⁶³ Figure 4.4.1 is based on Figure 1 in Sampson and Groves (1989, p. 783), an authority cited by Dr. Linz.

industrial zones, SOB neighborhoods have few if any residences, churches, schools, or families.⁶⁴ Neither Shaw and McKay (1942 [1929]) nor Sampson and Groves (1989) would recognize the area around a typical SOB as a “neighborhood.” Dr. Linz’s description of social disorganization theory ignores this point:

Social disorganization theory posits that the level of crime in a particular neighborhood is a function of the “social disorganization” in that neighborhood. A community is considered “disorganized” if there is a lack of social solidarity, social cohesion, and integration among the residents of that community (Kubrin, Stucky, & Krohn, 2009). The lack of these characteristics in a community leads to crime because informal social control is unable to be established without them, and it is this informal social control that helps to deter crime in a community (Kubrin et al, 2009). Perhaps most important to social disorganization’s application to the study of negative secondary effects is the insistence that the characteristics of the community in which the crime is occurring are important.⁶⁵

Although Dr. Linz’s description of social disorganization theory correctly focuses on the actions of neighborhood’s families, residents, and informal institutions, he does not explain how the theory might work in neighborhood’s that have few residents, families, and institutions.

Dr. Linz’s weak understanding of social disorganization theory is further evident in his treatment of alcohol-serving establishments as a social disorganization variable:

I should mention, one of the elements that social disorganization theory points to as a possible mediator of crime, or I should say as a possible predictor of crime in a particular area, is the presence or absence of an alcohol-serving establishment.⁶⁶

No primary authority on criminological theory, including those cited by Dr. Linz, would include “presence or absence of an alcohol-serving establishment” as a social disorganization variable for two reasons. First, the hypothetical residential neighborhoods at the theory’s core tend not have bars or taverns. Second, although the spatial correlation between alcohol-serving businesses and crime is widely known and well accepted (see, *e.g.*, Roncek and Pravatiner, 1989; Roncek and Maier, 1991), the correlation is predicted by the routine activities theory of hotspots, not social

⁶⁴ In Cleveland, Dayton, and Toledo, *e.g.*, there were only 306 residential property sales within one-quarter mile of an SOB.

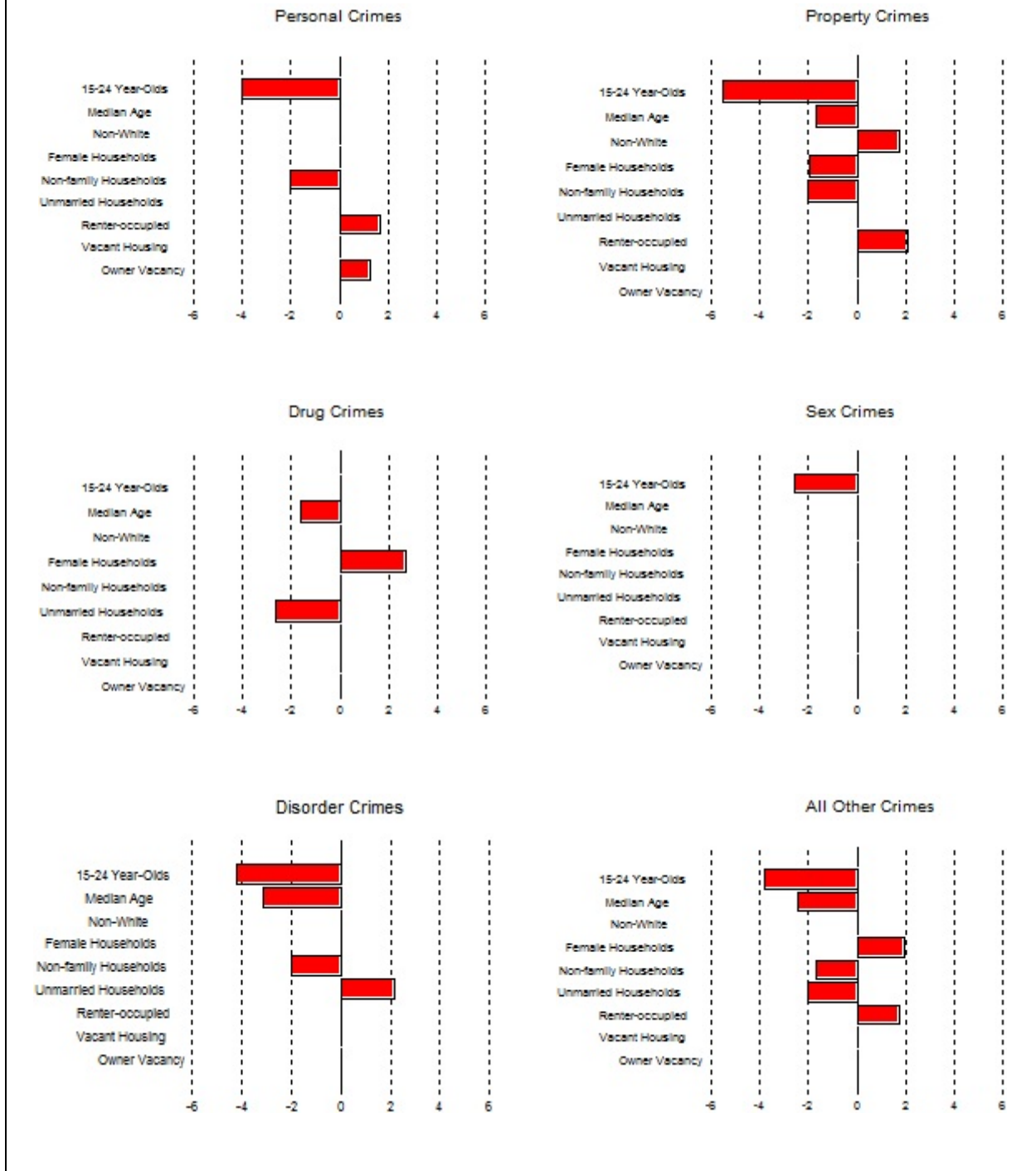
⁶⁵ Report of Daniel Linz, Ph.D., p. 11.

⁶⁶ Testimony of Daniel Linz, Ph.D., March 12, 2004, p. 111

disorganization theory.⁶⁷

⁶⁷ Five of the top ten hotspots in Minneapolis were bars or taverns (Sherman *et al.*, 1989, Table 5, p. 43).

Figure 4.4.2 - Social Disorganization Effects for Six Crimes



4.4.2 Evidence from Dr. Linz's Studies

To test a scientific theory, we (1) model the theory; (2) collect reliable, appropriate data; (3) estimate model parameters from the data; and (4) use the parameter estimates to test crucial hypotheses. Assuming that the model accurately represents the theory, and that the data are reliable and appropriate, the theory will predict specific results for the hypothesis tests. If the observed and predicted results are similar, the data *corroborate* the theory. If the observed and predicted outcomes are discrepant, on the other hand, the data *reject* the theory. Most rejected theories can be modified to account for the discrepancy; but in rare instances, the discrepancy may demand that the theory be categorically rejected.

Figure 4.4.2 plots the standardized parameter estimates for the effects of nine social disorganization variables on six categories of crime in Greensboro.⁶⁸ A few of the 54 observed effects *corroborate* Dr. Linz's social disorganization theory to some extent. Neighborhoods with *higher* median ages are expected to have *lower* levels of crime, *e.g.* As predicted, Dr. Linz find a negative correlation for Property, Drug, Disorder, and All Other crimes (though not for Personal and Sex crimes). *Most* of the 54 observed effects do *not* corroborate the theory, however, and several actually *contradict* the theory.

- **15-24 year-olds.** Neighborhoods with *younger* populations should have *higher* crime rates. Dr. Linz finds *negative* effects on Personal, Property, Sex, Disorder, and All Other crimes, however, but curiously, *no effect* on Drug crimes.⁶⁹
- **Family disruption.** Neighborhoods with *high* numbers of female-headed, non-family, and unmarried households should have *high* crime. Dr. Linz finds *no effect* in eight cases, however, *negative* effects in four cases, and the expected positive correlation in only six of eighteen cases.
- **Residential instability.** Neighborhoods with *high* rates of vacant, renter-occupied, and vacant owner-occupied housing should have *high* crime rates. Dr. Linz finds that residential instability has *no effect* on crime in fourteen of eighteen cases.

⁶⁸ See §3.1 above. Figure 4.4.2 is based on Tables 14-19 in *Evaluating Potential Secondary Effects of Adult Cabarets and Video/Bookstores in Greensboro: A Study of Calls for Service to the Police*. Daniel Linz and Mike Yao, November 30th, 2003.

⁶⁹ Dr. Linz's results for the 15-24 year-old population lead to an *absurd* conclusion: *More* juveniles, *less* juvenile delinquency. We suspect that this result is an artifact of his misspecified regression model. Our suspicion is reinforced by a San Antonio study reviewed at §4.5.5 below. Using the same regression model in San Antonio, Enriquez, Cancino, and Varano (2007) also find less juvenile delinquency in neighborhoods with more juveniles.

Inconsistent effects – *positive* for one crime, *negative* for another – pose a more serious challenge to the theory. To illustrate, female-headed households has a *positive* effect on two crimes (Drug and All Other), a *null* effect on three other crimes (Personal, Sex, and Disorder), and a *positive* effect on Property crime. Female-headed households appears to be *uncorrelated* with crime.

Except for median age, none of Dr. Linz’s effects *corroborates* social disorganization theory. Most of the effects *reject* the theory. Used appropriately, social disorganization is a powerful criminological theory. Used inappropriately, on the other hand, it supports incorrect inferences. Judging from the perverse pattern of effects in Figure 4.4.2, critical readers should ask why Dr. Linz would want to rely on social disorganization theory to estimate the secondary effects of SOBs. The answer, put simply, is that by incorporating a large number of extraneous explanatory variables in his model, Dr. Linz biases his results in favor the null finding. We will return to this topic at §4.5.1 below when we discuss multiple regression.

4.5 Studies That Find No Secondary Effects

In addition to his opinion that the State relied on secondary effects studies that were “not reliable,” Dr. Linz faults the State for ignoring a set of “scientifically sound” studies, conducted by Dr. Linz and his colleagues, that find no secondary effects:

ABSENT FROM THE STATE OF OHIO’S JUSTIFICATION FOR RULE 52
ARE A LARGE NUMBER OF SCIENTIFICALLY SOUND STUDIES THAT
SHOW NO ADVERSE EFFECTS OF ADULT BUSINESSES.⁷⁰

We disagree. Although we do not know whether the State *ignored* these studies as Dr. Linz claims, a fair review of those studies would not have shaken the State’s reliance on the broader secondary effects literature and its own local evidence. On the contrary, studies that find no secondary effects, especially those conducted by Dr. Linz and his colleagues, are consistent with the consensus finding of the broader literature and the State’s local evidence.

Dr. Linz’s claim that his studies are “scientifically sound,” compared to other studies, rests on four design elements that distinguish them, in Dr. Linz’s opinion, from other studies:

- Dr. Linz’s studies use exploratory spatial analyses: “Rather than assuming that [SOBs] are crime hotspots without any empirical evidence ... we have tested this assumption.”⁷¹
- Dr. Linz’s studies analyze crime rates in polygonal regions (*e.g.*, census

⁷⁰ Report of Daniel Linz, Ph.D., p. 1.

⁷¹ Report of Daniel Linz, Ph.D., p. 11.

tracts): “Further, we focus not only on concentric circles ... but we also take into account the myriad characteristics of the larger neighborhood in which the [SOB] is situated that may account for crime.”⁷²

- Dr. Linz’s studies compare crime rates in SOB areas to analogous crime rates in control areas: “[I]t is necessary ... to engage in a comparative analysis in order to ascertain if crime is higher or lower at [SOBs] compared to other venues such as taverns and bars.”⁷³
- Dr. Linz’s studies use multiple regression to analyze SOB-control differences: “[I]t is necessary to employ as [*sic*] specific analytic technique, multivariate regression when studying secondary effects.”⁷⁴

According to Dr. Linz, each of these four design elements enhances the validity of a secondary effects study; and each is found in his studies but are not found in the studies relied on by the State. We disagree with both claims. None of the four elements is a necessary-sufficient condition for validity. The four elements are found in many of the studies relied on by the State, moreover, and are not found in many of Dr. Linz’s studies.

The first two design elements stem from Dr. Linz’s rejection of the criminological theory of hotspots and from a misreading of two primary authorities.

- Dr. Linz’s insistence that spatial null hypotheses⁷⁵ cannot be tested until an exploratory analysis identifies SOBs as hotspots misinterprets Anselin *et al.* (2004), a primary authority on spatial criminology.⁷⁶ Exploratory and confirmatory (*i.e.*, hypothesis testing) methods are “apples and oranges,” appropriately used for very different purposes.
- Dr. Linz’s insistence that existing polygonal map regions (*e.g.*, census tracts) have better statistical properties than purposefully constructed circular regions is a vicarious misreading of Lawson and Waller (1996: 473), a primary authority on spatial sampling.

⁷² Report of Daniel Linz, Ph.D., p. 11.

⁷³ Report of Daniel Linz, Ph.D., p. 12.

⁷⁴ Report of Daniel Linz, Ph.D., p. 12.

⁷⁵ The spatial null hypothesis holds that “The SOB site is *not* a crime hotspot.” To reject the spatial null hypothesis is to conclude that “The SOB site *is* a crime hotspot.”

⁷⁶ See the affidavit from Dr. George Tita, a co-author of Anselin *et al.* (2004) which Dr. Linz cites as the authority for his opinion.

Neither of these design elements makes a study more “scientifically sound.” On the contrary, according to the primary methodological authorities cited by Dr. Linz, either design element can threaten the validity of a study.

Dr. Linz infers his second two distinguishing design elements – which he characterizes as “methodological dictums” – from the Seventh Circuit Court of Appeals decision in *Annex Books v. Indianapolis*. Regarding SOB-control contrasts, *e.g.*, the Court said:

Nor can we tell whether 41 arrests at one business over the course of 365 days is a large or a small number. How does it compare with arrests for drunkenness or public urination in or near taverns, which in Indianapolis can be open on Sunday and well after midnight? If there is more misconduct at a bar than at an adult emporium, how would that justify greater legal restrictions on the bookstore — much of whose stock in trade is constitutionally protected in a way that beer and liquor are not.⁷⁷

No methodologist would quarrel with this *dictum*. Without an appropriate control, secondary effects estimates are *uninterpretable*. But the control must be *appropriate*. The costs in validity of an inappropriate control – a “crack house,” on one hand, or a church on the other – are dear. We return to this point in §4.5.2 below when we discuss Dr. Linz’s Charlotte study.

Although we might agree with Dr. Linz’s opinions on SOB-control contrasts – at least in principle – Dr. Linz’s opinion on the methodological necessity of a “specific analytic technique, multivariate regression,” presumably derived from the Seventh Circuit’s *Annex Books* decision, is surely incorrect. The Court’s only reference to “multiple regression” is:

One may doubt that Linz’s work is the last word; a multivariate regression would provide a better foundation than either a time series or a geographic cross-section. See Daniel L. Rubinfeld, *Reference Guide on Multiple Regression*, Reference Manual on Scientific Evidence (2nd ed.) (Federal Judicial Center 2000).⁷⁸

We do not read this passage as a categorical *dictum* in favor of multiple regression. Since Dr. Linz’s Indianapolis study used “time series” and “geographic cross-section” designs, the Court’s “important methodological dictum” might have this more plausible interpretation: The design of Dr. Linz’s Indianapolis study compares unfavorably to alternative designs, including a multiple regression. The methodological authority cited by the Court reinforces this interpretation. According to Rubinfeld (2000),

⁷⁷ *Annex Books, Inc., et al. v. City of Indianapolis, Indiana*

⁷⁸ *Annex Books, Inc., et al. v. City of Indianapolis, Indiana*

Multiple regression analysis can be a source of valuable scientific testimony in litigation. However, when inappropriately used, regression analysis can confuse important issues while having little, if any, probative value.⁷⁹

Would the Court issue a categorical *dictum* in one sentence and cite a contradictory authority in the next? That is implausible.

Nevertheless, if Dr. Linz's interpretation *were* correct – *i.e.*, if the Court *did* endorse “a specific analytic technique, multivariate regression when studying secondary effects” – then the validity of Dr. Linz's multiple regressions must be judged against Rubinfeld (2000), the multiple regression authority cited by the Court. By that standard, Dr. Linz's use of multiple regression is inappropriate, leading to inferences that are wholly invalid.

4.5.1 Rubinfeld (2000) on Multiple Regression

Quantitatively-oriented social scientists of our generation are familiar with Rubinfeld's *Econometric Models and Economic Forecasts*, 2nd Ed. (1981, written with Robert S. Pindyck) and with his methodological reviews, including Rubinfeld (2000). In sum, Daniel L. Rubinfeld is an acknowledged authority on multiple regression. Contrary to Dr. Linz's opinion, Rubinfeld (2000) does *not* recommend multiple regression in all situations but, rather, only in situations where the following assumptions are warranted:

- ***Strong Causal Theory.*** Multiple regression assumes a strong causal theory connecting a limited number of explanatory variables (*causes*) to a single response variable (*effect*).
- ***No “Feedback.”*** Multiple regression assumes *unidirectional* causality. An explanatory variable is assumed to cause the response variable, in other words, but *not vice versa*.
- ***No Omitted Explanatory Variables.*** Multiple regression assumes that no relevant, significant explanatory variable has been omitted from the model.
- ***No Extraneous Explanatory Variables.*** Multiple regression assumes that no *irrelevant, insignificant* explanatory variable has been included in the model.
- ***No “Fishing.”*** Multiple regression assumes that the theoretically specified model is unique.

After discussing these assumptions, we will compare Dr. Linz's use of multiple regression

⁷⁹ Rubinfeld (2000), p. 183

analyses against the standards laid out in Pindyck and Rubinfeld.

(1) Strong Causal Theory. Prior to any other design consideration, multiple regression requires a *causal* theory that connects a response (or dependent) variable to a limited number of explanatory (or independent) variables:

Causality cannot be inferred by data analysis alone; rather, one must infer that a causal relationship exists on the basis of an underlying causal theory that explains the relationship between the two variables.⁸⁰

As a practical matter, the theory must be strong enough to also specify the proper unit of analysis for cause and effect (*e.g.*, individuals), the “best” response variable (*i.e.*, the measured effect), and a limited number of explanatory variables (*i.e.*, the measured causes). The prevailing theories in most fields are not strong enough to warrant multiple regression modeling. Rubinfeld (2000) cites the field of pay discrimination law as an exception. In a typical lawsuit, the *i*th employee’s “expected” salary is given by the multiple regression model:

$$\text{Salary}_i = \alpha + \beta [\text{Market Variables}]_i + \gamma [\text{Race, Sex, etc.}]_i + \varepsilon_i$$

Strong economic and legal theories hold that the *i*th employee’s salary is determined by a small number of legitimate labor market variables (employee’s education, experience, tenure, *etc.*) – but not by illegitimate variables (employee’s race, sex, *etc.*). Employees with identical scores on the labor market variables may not have identical salaries in the short run. The model’s random error term, ε_i , accounts for these small, unexplained salary differences. In the long run, or in expectation, the random errors vanish, resulting in

$$\text{Expected Salary} = \alpha^* + \beta^* + \gamma^*$$

where α^* , β^* , and γ^* are least-squares estimates of the effects of the legitimate and illegitimate variables on employee salaries. Small values of γ^* support the defendant’s case; large values support the plaintiff’s case.⁸¹

Multiple regression is well suited to pay discrimination suits because strong economic and legal theories specify the response variable (employee’s salary in dollars), the appropriate unit of analysis (employee), a limited number of legitimate explanatory variables (education, experience, tenure, *etc.*), and a limited number of illegitimate explanatory variables (race, sex, *etc.*). Because the underlying theory is so strong, defendant, plaintiff, and court can agree on the correct interpretation of the regression model and results. Where the prevailing theory is not so strong,

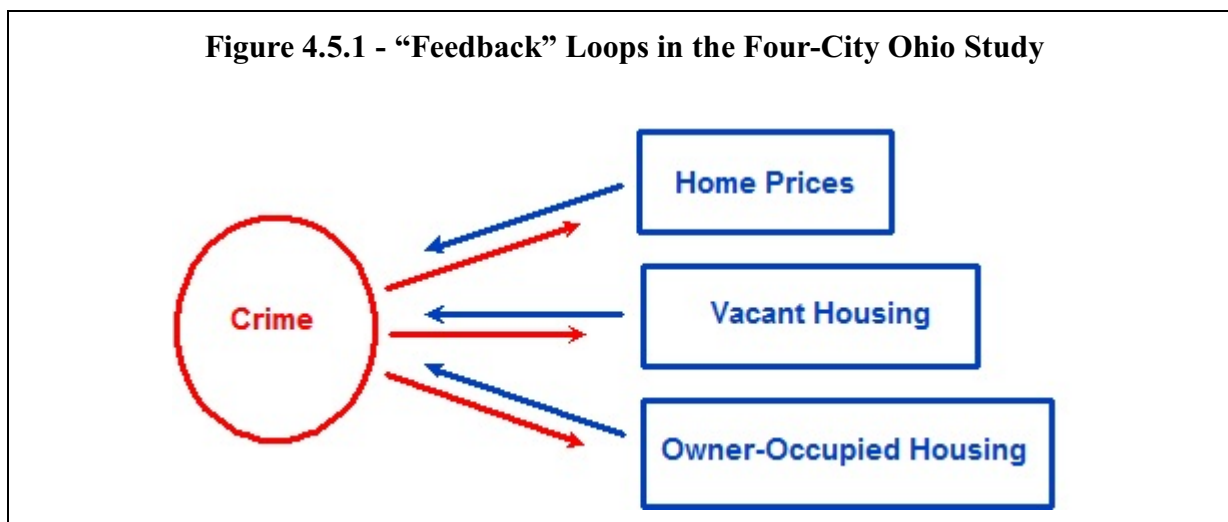
⁸⁰ Rubinfeld (2000), p. 184

⁸¹ In this context, “small” means “not different than zero” or “not statistically significant at the conventional 95 percent confidence level.”

on the other hand, multiple regression may be inappropriate:

Failure to develop the proper theory, failure to use variables that are appropriate for the multiple regression study, and failure to use alternative procedures when they are more appropriate can substantially bias the multiple regression results.⁸²

Rubinfeld (2000) might have been describing secondary effects research. In *J.L. Spoons*, e.g., a typical secondary effects suit, the Defendant and Plaintiff cannot agree on a response variable (911 calls, total crimes, sex crimes, etc.), the unit of analysis (500-foot circle, census tract, face-blocks, etc.), and most important of all, the explanatory variables. This reflects the fact that the Defendant and the Plaintiff have different secondary effects theories.



(2) No “Feedback.” The fact that SOB areas have more crime or lower property values than non-SOB areas has two interpretations. Either SOBs *cause* high crime and low property values; or else, high crime and low property values *cause* SOBs to move into an area. Courts have emphasized this interpretive dilemma:

[T]he studies [on which the City relied] are simple cross-sectional analyses that leave causation up in the air. (In other words, they may show no more than that adult businesses prefer high-crime districts where rents are lower.)⁸³

In some substantive fields, the interpretive dilemma can be resolved on theoretical grounds. In a salary discrimination suit, e.g., the defendant could not argue that the plaintiff’s salary *caused* his or her race or sex. In secondary effects research, the underlying theory is not strong enough to

⁸² Rubinfeld (1986), p. 1072 as cited in Rubinfeld (2000).

⁸³ *Annex Books, Inc., et al. v. City of Indianapolis, Indiana*

resolve the dilemma.

Figure 4.5.1 uses three explanatory variables from Dr. Linz’s Four-City Ohio study, reviewed at §4.5.4 below, to illustrate the interpretive dilemma.⁸⁴ To estimate the effects of SOBs on crime in Cleveland, Columbus, Dayton, and Toledo, Dr. Linz regresses the number of crimes in a neighborhood on the neighborhood’s median home price, vacancy rate, and owner-occupancy rate. To interpret the regression, Dr. Linz must assume that a change in any or all of the explanatory variables will *cause* a change in crime – but *not* vice versa.

In the multiple regression framework, the expert often assumes that changes in explanatory variables affect the dependent variable, but changes in the dependent variable do not affect the explanatory variables—that is, there is no feedback. In making this assumption, the expert draws the conclusion that a correlation between an explanatory variable and the dependent variable is due to the effect of the former on the latter and not vice versa. Were the assumption not valid, spurious correlation might cause the expert and the trier of fact to reach the wrong conclusion.⁸⁵

But in fact, falling home prices, falling owner-occupancy rates, and rising vacancies are among the likely causal effects of rising crime rates. “Feedback” from the response variable (crime) to the explanatory variables violates the unidirectional causality assumption. Dr. Linz’s regression results are biased as a consequence and inferences based on the results are invalid.

(3) *No Omitted Explanatory Variables.* In addition to specifying the “best” response variable and unit of analysis, the strong causal theory must specify the explanatory variables to be used in the multiple regression model. If the theory is inadequate, relevant explanatory variables may be omitted from the model with serious consequences.

Failure to include a major explanatory variable that is correlated with the variable of interest in a regression model may cause an included variable to be credited with an effect that actually is caused by the excluded variable. In general, omitted variables that are correlated with the dependent variable reduce the probative value of the regression analysis. This may lead to inferences made from regression analyses that do not assist the trier of fact.⁸⁶

The consequences of omitting a relevant explanatory variable depend on the relative importance of the variable in the causal system and magnitude and sign of its correlation with the variables

⁸⁴ Our Figure 4.5.1 is based on Rubinfeld’s (2000, p. 196) Figure 1.

⁸⁵ Rubinfeld (2000, p. 195)

⁸⁶ Rubinfeld (1986), p. 1072.

included in the model. Although the magnitude and nature of the bias cannot be predicted, the *inferential* consequences of an omitted variable are always serious in secondary effects research.

To discover the variables that might have been omitted from Dr. Linz's regression models, return to Figure 4.4.1. Sampson and Groves (1989) included measures of eight variables in their social disorganization model: socio-economic status, local friendship networks, participation in local organizations, supervision of local youth, residential stability, ethnic heterogeneity, family disruption, and urbanization. Dr. Linz's regression models include measures of socio-economic status, residential stability, ethnic heterogeneity, and family disruption; but omit measures of local friendship networks, participation in local organizations, supervision of local youth, and urbanization. The inferential consequences of omitting these four variables is unknown. But it is unlikely that the consequences are benign.

(4) *No Extraneous Explanatory Variables.* In specifying the exact set of explanatory variables to be included in the multiple regression model, the strong causal theory excludes all other variables. Incorporating extraneous, irrelevant variables in a multiple regression model biases significance tests.

The major effect of the inclusion of irrelevant variables is to reduce the statistical significance of the regression results ... If the variable that is included, however, is inappropriate for theoretical reasons but does have an effect on the dependent variable, then the problem can be more serious. As will measures of goodness-of-fit, t-values may rise, and the trier of fact may reach inappropriate conclusions from the statistical analysis. The courts should be wary of the practice of including a large number of variables solely to overfit the data.⁸⁷

If tests of statistical significance are not an issue, incorporating extraneous variables in the multiple regression model can have benign consequences. When the overfit model is used to test the significance of a secondary effect estimate, however, the consequences will be severe and will *always* favor the null hypothesis.

Dr. Linz's Four Ohio City study, reviewed at §4.5.4 below, illustrates the extent of the problem. Table 4.5.1 reports the parameter estimates from the four models available to us. Nearly 60 percent ($31/52 = .596$) of parameter estimates in the four models are not statistically significant at the 95 percent confidence level. With the statistically insignificant variables in the model, Dr. Linz adds a dummy variable, coded for the present or absence of SOBs in a Census Block Group, to the model. Since the incremental proportion of variance explained by the SOB dummy variable is not statistically significant, Dr. Linz concludes that SOBs in the four Ohio cities have no secondary effects. The statistically insignificant effect estimates in his model pose a threat to the statistical conclusion validity of this inference.

⁸⁷ Rubinfeld (1986), p. 1077.

Table 4.5.1 - Parameter Estimates in the Four Ohio Cities Study

| | <i>Table 4^a</i> | <i>Table 6^b</i> | <i>Table 8^c</i> | <i>Table 10^d</i> |
|--|----------------------------|----------------------------|----------------------------|-----------------------------|
| Area | — | — | — | — |
| Population | 2.485 | — | — | — |
| Median age | — | -2.631 | -2.631 | -2.663 |
| Nonwhite | 2.985 | — | — | — |
| Female-headed households | — | -3.613 | -3.613 | — |
| Married households | -3.463 | — | — | — |
| Less than 9th grade | — | — | — | — |
| College degree | -2.191 | — | — | 2.271 |
| Median household income | -3.096 | — | — | — |
| Median home price | — | — | — | -2.014 |
| Percent below the poverty level | 3.409 | 2.690 | 2.690 | 4.162 |
| Vacant housing units | 3.520 | 6.256 | 6.256 | 3.656 |
| Owner-occupied housing units | 2.326 | — | — | — |
| Significant : Non-significant | 8 : 5 | 4 : 9 | 4 : 9 | 5 : 8 |

^a Columbus sex crimes; ^b Dayton forcible rapes; ^c Dayton prostitution; ^d Dayton sex crimes

(5) *No “Fishing.”* Also known as “data torturing”⁸⁸ and “data dredging,”⁸⁹ “fishing” refers to the all too common practice of searching through a menu of multiple regression models to find one that supports a particular inference.

The large number of possible explanatory or control variables available tempts statisticians to run a large number of regressions. It can be dangerous, however, if the expert uses this approach to search for the best coefficient that he or she can find.⁹⁰

Allowed to choose from even a modest menu of potential multiple regression models, naive analysts can produce a null (or non-null) finding at will.

In experimental research, design structures, including random assignment and placebo

⁸⁸ Mills (1993, p. 1196): “If you torture your data long enough, they will tell you whatever you want to hear.”

⁸⁹ Federal Judicial Center (2000, p. 358).

⁹⁰ Rubinfeld (1986), p. 1073

blinding, minimize the opportunity for “fishing.” In quasi-experimental research, where these design structures are not available, rigidly enforced conventions minimize the potential for “fishing.” In multiple regression modeling, where neither experimental design structures nor quasi-experimental conventions are available, the opportunity for “fishing” can only be minimized by strict adherence to a strong causal theory. Departures from the theoretical specification must be explained and if the explanation is unconvincing, critics must conclude that the results are an artifact of “fishing.” The unusually wide range of designs found in Dr. Linz’s studies suggest that “fishing” is a potent, plausible threat to the statistical conclusion validity of his results.

4.5.2 Adult Cabarets in Charlotte

To estimate the secondary effects of SOBs in Charlotte, Dr. Linz and his colleagues compared ambient UCR Part I crime at the sites of twenty adult cabarets and three control businesses: a McDonald’s fast food restaurant, a Kentucky Fried Chicken take-out store, and an Exxon mini-mart gas station. Comparing ambient crime at the SOB and control sites, Dr. Linz and his colleagues found that

[The] presence of an adult nightclub does not increase the number of crime incidents reported in localized areas surrounding the club (defined by circular areas with 500 and 1,000 feet radii) as compared to the number of crime incidents reported in comparable localized areas that do not contain such an adult business.⁹¹

Before commenting on the most controversial aspect of the Charlotte study’s design, note that it did *not* include an exploratory spatial analysis; that it measured crime in *circular* (vs. polygonal) regions centered on the SOB and control sites; and that it did *not* control for extraneous variables with multiple regression. In short, it had *none* of the design elements that, in Dr. Linz’s opinion, make a study “scientifically sound.”

But the most memorable design element of the Charlotte study must be Dr. Linz’s choice of control sites.⁹² In their report to Dr. Linz, the North Carolina co-authors explicitly note the potential threat to validity posed by the choice of these particular control businesses.

[I]t may be more appropriate to compare adult club sites with non-adult club sites so that one can determine whether the type of club activity affects the level of crime. This comparison may be implicit (if not explicit) in the minds of citizens

⁹¹ Report of Daniel Linz, Ph.D., p. 13

⁹² The Charlotte study was a collaboration between Drs. Linz and Paul in California and Drs. Land, Ezell, and Williams in North Carolina. After designing the study, Dr. Linz retained Drs. Land, Ezell, and Williams to collect and analyze the data and write a report. Drs. Linz and Paul added “introduction” and “methods” sections to the report and submitted it for publication. Descriptions of data collection and analysis written Drs. Linz and Paul are inaccurate.

and justices when considering whether an adult club should be allowed to locate in a particular area. Methodologically, using basic service type businesses such as fast food restaurants as control sites may confound the comparisons being made in the research, even if they are located in areas equivalent to those in which adult dance clubs are located.⁹³

The Seventh Circuit *Annex Books* panel would surely agree with Dr. Linz’s co-authors. Control is a fundamental principle of design. Implicit in this principle, however, is the idea that analog controls must be theoretically *appropriate*. Given the obvious *inappropriateness* of Dr. Linz’s controls, the Charlotte study says very little – if anything – about the secondary effects of adult cabarets.

Had Dr. Linz chosen more appropriate control businesses, the Charlotte design would still have had another, more serious shortcoming. Calling attention to the shortcoming, Dr. Linz’s co-authors attribute it to a combination of not enough control sites and too little crime data:

[D]ue to the limited number of clubs/controls and the relatively large number of parameters in comparison (especially the club-specific fixed-effects that were included to account for unobserved heterogeneity at the club/neighborhood level), we ... were not able to reliably and robustly estimate these models ... If we had access to either more years of crime data or more clubs/controls, these models would have been more feasible and more appropriate.

Dr. Linz’s co-authors are describing the *statistical power* of the study design. Statistical power is a property of secondary effects null hypothesis tests. The SOB-control difference in Charlotte was too small to reject the null hypothesis. This does not imply that the null hypothesis is true, however. While the null hypothesis might be true, it is also possible that the study design lacked the statistical power required to detect a secondary effect. If the adult cabarets had had relatively large secondary effects, as Dr. Linz’s co-authors note, the effect would have passed unnoticed in the background “noise” of the study design.

Statistical power is an endemic shortcoming of the secondary effects studies conducted by Dr. Linz and his colleagues. We discuss this relatively esoteric topic in detail in §8 below. For present purposes, we note that when a study is designed without an adequate level of statistical power, the failure to find a secondary effect says nothing about the hypothetical secondary effects of SOBs. The study is *inconclusive*. Most of Dr. Linz’s “scientifically sound” studies belong in this category.

⁹³ Linz *et al.* (2004), p. 87.

4.5.3 Adult Cabarets in Fort Wayne

The 2001 Fort Wayne study by Drs. Linz and Paul compares a subset of crime incidents recorded within 1,000 feet of eight adult cabarets to the analogous incidents recorded in eight “matched” 1,000-foot control circles. Since Drs. Linz and Paul do not report their secondary effect estimates and standard errors, critics cannot calculate the statistical power of their null finding.⁹⁴ Since the number of SOB and control sites in the Fort Wayne study is smaller than the number of sites in the Charlotte study, however, and since the number of crimes is smaller, we can assume that the Fort Wayne null finding is *inconclusive*. Given the weak statistical power of their design, the null finding is expected.

One contribution to the low statistical power of the Fort Wayne design warrants special notice. Drs. Linz and Paul used Part I UCR crimes “cleared by arrest” to measure ambient crime risk. Since *most* Part I crimes are *not* “cleared by arrest,” their design excludes most of the crime in Fort Wayne. Critics can only wonder what their findings might have been if Drs. Linz and Paul had used a conventional ambient crime risk measure (*e.g.*, all Part I crimes).⁹⁵

| | 500-foot circles | 1,000-foot circles | Census block groups |
|-----------------------|-------------------------|---------------------------|----------------------------|
| Cleared crimes | | <i>Fort Wayne</i> | |
| Serious crimes | <i>Charlotte</i> | | |
| 911 calls | | | <i>Greensboro</i> |

The use of this unconventional measure of ambient crime risk raises the specter of “fishing,” a methodological threat that Cook and Campbell discuss in the context of statistical conclusion validity. To illustrate this methodological threat, consider the designs used in Dr. Linz’s Fort Wayne, Charlotte, and Greensboro studies. Although the three studies were all designed and implemented by the same actors, the studies use three different measures of ambient crime risk (cleared crimes, serious crimes, and 911 calls) and three different areal units of analysis

⁹⁴ Paul, Linz, and Shafer (2001, 355) fault the government-sponsored studies relied on by the State for not reporting these statistics: “[T]he most frequently cited studies ... fail to meet the basic assumptions necessary to calculate an error rate — a test of the reliability of findings.”

⁹⁵ An area that has fewer “crimes cleared by arrest” may have fewer crimes; alternatively, the area’s crimes may be more difficult to “clear.” Criminologists use “cleared” crimes to study investigative processes but not to measure ambient crime risk.

(1000-foot circles, 500-foot circles, and Census block groups). Crossing the design variations in these three studies yields nine potential quasi-experimental designs.

Finally, we are surprised to learn that the Fort Wayne study “received a top award from the U.S. Department of Justice” and that it was “vetted by peer review for its methodological soundness.” An internet search reveals that the Fort Wayne study did indeed win a student paper award. But all (three) of the student papers submitted to the 5th Annual International Crime Mapping Research Conference won “Top Student Paper” awards. None of the student papers submitted to the 5th Annual International Crime Mapping Research Conference was peer reviewed.

4.5.4 Sex Crimes in Four Ohio Cities

The Four Ohio Cities study was completed in 2005.⁹⁶ An abridged version (omitting the statistical details reported in our Table 4.5.1 above) appeared in 2007.⁹⁷ To estimate the effects of proximity to an SOB on sex crimes in Cleveland, Columbus, Dayton, and Toledo, Dr. Linz and his colleagues regress the number sexually-related 911 calls in a Census Block Group on demographic characteristics suggested by the routine activities and social disorganization theories. With these variables in the model, a dummy variable, coded for the presence of an SOB in the Census Block Group, is added to the model. The difference in R² statistics between the two hierarchical models is used to test the significance of the secondary effect estimate. Since the R² differences are small, Dr. Linz and his colleagues conclude that SOBs in the four Ohio cities have no secondary effects.

In our opinion, however, this conclusion does not follow from the statistical results reported by Dr. Linz and his colleagues. First, the underlying regression model violates all five of Rubinfeld’s (2000) assumptions. Second, more important, the model’s response (dependent) variable is sex-related 911 calls. In a recent decision, a panel of the Eleventh Circuit Court ruled that statistical analyses of 911 calls cannot be used to show the absence of secondary effects

Such crimes are often “victimless,” in the sense that all of those involved are willing participants, and, therefore, they rarely result in calls to 911 ... [A]n encounter between a prostitute and a “john” rarely leads to a 911 call.⁹⁸

⁹⁶ Linz, D. and M. Yao. *Evaluating Potential Secondary Effects of Alcohol Serving Adult Cabarets: A Four City Study of Police Activity*. September 28, 2005.

⁹⁷ Linz, D., M. Yao, and S. Byrne. Testing supreme court assumptions in *California v. la Rue*: Is there justification for prohibiting sexually explicit messages in establishments that sell liquor? *Communication Law Review*, 2007, 7:23-53.

⁹⁸ *Flanigan’s Enterprises v. Fulton County*, Eleventh Circuit Court of Appeals, No. 08-17035, February 16th, 2010. The decision is quotation from the earlier Daytona Grand decision

Although criminologists use 911 calls for specific purposes, their use as a measure of ambient crime risk is limited. In light of Dr. Linz's contrary opinions on 911 calls, we discuss the details of the disagreement at §6 below.

4.5.5 Crime in San Antonio

A published study by Enriquez, Cancino, and Varano (2007) replicates the design used by Dr. Linz and his colleagues in secondary effects studies of SOBs in Greensboro (reviewed at §3.1 above), Daytona Beach (reviewed at §3.2 above) and the four Ohio cities (reviewed at §4.5.4 above). The results of the San Antonio replication raise the same questions. First, none of the regression models reported by Enriquez, Cancino, and Varano (2007) fit the data.⁹⁹ Second, more important, the results are inconsistent with the theory of social disorganization.

Nearly half of the variables that should affect crime, according to social disorganization theory, are statistically *null*; they have no effects on crime. Of the 32 statistically significant effects, 17 *contradict* the theory. Most notably, the estimated effects of the 15-29 year-old male population on crime are *negative* for five of seven crimes; neighborhoods with *high* populations of 15-29 year-old males, in other words, have *low* crime rates. Proportions of renter-occupied housing, divorced couples, Latinos, and Blacks also have *negative* effects on crime, contradicting the theory. Median household income is the only social disorganization variable whose effects are consistent with the theory. Oblivious to the theoretical perversity of their results, Enriquez, Cancino, and Varano (2007) conclude that the consensus view of the secondary effects literature is unwarranted:

Instead, the results point to weak institutions, namely alcohol outlets and community characteristics associated with social disorganization theory as causes and correlates of crime.¹⁰⁰

The reported results do not support this conclusion, however, but rather, strongly suggest that the theory of social disorganization is inappropriate for secondary effects phenomena.

4.6 Dr. Linz Does not Refute Evidence Presented in the State's Hearing

In addition to relying on the broader secondary effects literature, the State relied on

which also ruled out the use of 911 calls to prove the absence of secondary effects.

⁹⁹ The reported R² statistics (interpreted as the proportions of variance explained by the models) range from 3.1 to 10 percent. R² statistics as low as these preclude publication in a peer-reviewed journal. The journal that published this article is edited by law students. It is not a peer-reviewed journal.

¹⁰⁰ Enriquez, Cancino, and Varano (2007), p. 34.

evidence submitted in a public hearing. Dr. Linz does not refute this evidence.

Testimony of David Raber. Mr. Raber encouraged the Liquor Control Commission to consider the broader circumstances in which violations occur so as not to overly penalize permit holders for the actions of rogue employees.

Testimony of Bruce Taylor. Mr. Taylor described his experiences as a Cleveland attorney and how they prosecuted nudity at juice bars using indecent exposure statutes. When confronted with the argument that paying customers would not be offended by the nudity, they began prosecuting these cases under obscenity statutes instead. He discussed the Rule 52 that has been subject to recent amendments and how it has been challenged on the basis that it violates the First Amendment to the U.S. Constitution. The Cuyahoga County Court of Appeals argued that even greater protection of dancing exists within the Ohio Constitution. A number of studies have been conducted examining the harmful secondary effects of places of adult entertainment, resulting in a common body of knowledge. The Supreme Court held that cities need not conduct their own studies but instead can draw on the experiences of other places. Bars or juice bars with nude dancing experience increased levels of prostitution, drug trafficking, fights and brawls, even compared to other bars. Federal court in Cleveland ruled the 1998 version of the Rule 52 to be overly broad. In 2000, the U.S. Supreme Court, upholding an Erie ordinance, ruled that requiring dancers to wear pasties and G-strings does not violate their First Amendment rights.

Mr. Taylor argued that the new proposed Rule 52 is constitutionally safer because (1) it is more specific and (2) the rule will not be subject to an overbreadth challenge. He also argued that places that sell alcohol give up some of their rights by obtaining a permit to do so, which gives the Commission the power through liquor regulation to deny permits to establishments that have certain types of entertainment.

Mr. Taylor presented an affidavit from an undercover investigator in Monroe, Ohio demonstrating that topless lap dancers in the Bristol Show Club had physical contact with customers during lap dances in back rooms. The club took a cut of this extra money as well.

Mr. Taylor submitted affidavits for search warrants issued by police in El Paso, Texas. Police determined there were strip clubs in the city hiring underage girls as dancers.

Mr. Taylor submitted testimony given to the Ohio legislature before the Ohio Senate's Judiciary Committee by (1) David Miller from Citizens for Community Values of Cincinnati; (2) Carol McKenzie of Memphis, Tennessee who works for an organization to help young girls get out of the strip club business; (3) Dave Sherman, manager of Deja Vu in the midwest region who testified about how girls are recruited for the business and what they are really told to do. He also submitted copies of testimony given in 2000 before the Michigan House Committee from Dave Sherman, Caroline McKenzie, and two former strippers.

Mr. Taylor provided a copy of a study conducted in Adams County, Colorado, in 1991, discussing the high levels of crime associated with their strip club businesses and the effectiveness

of their ordinance in reducing these crimes. He also provided a copy of “Strip Club Testimony” by Kelly Holsopple, which chronicles her experience as a stripper.

Mr. Taylor also discussed the 1993 federal district case of *Bright Lights vs. the City of Newport, Kentucky*, where a federal district judge upheld an ordinance requiring dancers to wear more than pasties, instead requiring that they wear clothing covering most of the lower half of women’s breasts. The judge justified the ordinance on the basis of the secondary effects caused by adult entertainment businesses.

Mr. Taylor’s testimony ended with a description of how the exploitation of young girls produces a slippery slope from dancing at a strip club to prostitution. Finally, he argues that the proposed Rule 52 is constitutional; a First Amendment challenge to the proposed Rule 52 is not likely to be accepted.

During questioning, Mr. Taylor argued that the Supreme Court of Ohio has stated that the Ohio Constitution does not grant greater protection to speech or expression than the First Amendment and most appeal courts in the State have sided with the Ohio Supreme Court (Cleveland being one exception). He further argued that the language in the proposed Rule 52 is not vague.

Mr. Taylor was questioned regarding whether any studies have produced contrary findings to the particularly harmful effects found with the combination of nudity/partial nudity and alcohol. He responded that he was unaware of any studies that show the contrary.

Testimony of Ed Duvall. Duvall, of the Ohio Department of Public Safety, presented two concerns. The first was a request to expand the definition of Section A-2 in the proposed Rule 52 to include not just the nipple but also the areola area. The second was a request that section B-6 be expanded to include language specifically referring to electronic benefit transfer (EBT) cards.

During questioning, Mr. Duvall was asked whether it has been experience over the course of his many years in law enforcement that the combination of nudity/partial nudity and alcohol increases antisocial behavior. He stated that this was indeed his experience and that complaints were common in bars with impaired customers and nudity or lap dancing. He also reported receiving complaints from both local law enforcement and concerned citizens regarding prostitution, drug selling, and violent crime. He further confirmed that harmful activity *outside* of the establishment increases as well. He described how “street individuals” know that customers, out-of-town individuals, and tourists go to these establishments - that overcharge for everything - with money to spend. Cars in the parking lots are subject to breaking and entering. Domestic situations also arise, *e.g.*, where a boyfriend of a dancer will show up with a weapon.

4.7 Concluding Remarks

Dr. Linz's critiques of the secondary effects literature relied on by the State are based on the idiosyncratic methodological rules endorsed by Paul, Linz, and Shafer (2001), not on primary methodological authorities. Some (but not all) of the secondary effects studies conducted by Dr. Linz and his colleagues find no secondary effects. Dr. Linz interprets these null findings to mean that SOBs have no secondary effects. In every case, however, Dr. Linz's null findings fail to achieve the conventional level of statistical power required for that interpretation. Lacking the conventional level of power, a null finding is an inconclusive result. We expand on this technical point in §7 below.

5. Opinions of Lance Freeman, Ph.D.

Dr. Freeman uses “hedonic” pricing methods to estimate the secondary effects of SOBs on residential real estate values in Cleveland, Columbus, Dayton, and Toledo during 2001-2003. In simple terms, hedonic pricing methods assume that the value of a home can be inferred from its sales price; and that the sales price of a home is determined by its features (age, construction, living space, bathrooms, garage, *etc.*) and its by neighborhood (SES, schools, parks, *etc.*). Using a statistical model to control for the effects of these variables, homes near SOBs should sell for more or less the same price as any other home. If not, the price difference is interpreted as a secondary effect of SOBs.

Hedonic methods do not guarantee a unique statistical model but, rather, allows the analyst to pick and choose model structures and assumptions from a menu. As a consequence, independently constructed models will always have small (or sometimes even large) differences. Parameter estimation introduces another menu of potential differences. When a model finally emerges from this process, it must be interpreted conditionally. Different model structures and assumptions and different estimation algorithms may support different interpretations.

Although Dr. Freeman discusses some of the ambiguities in his statistical results, he does not consider whether other model specifications, assumptions, and estimation algorithms might have produced different results. Informed by the same hedonic methods, McCarthy, Renski and Linz (2001) chose very different structures and assumptions for their model and used a different algorithm to estimate its parameters. Dr. Freeman characterizes McCarthy, Renski, and Linz (2001) as “[p]robably the most sophisticated examination of the secondary effects thesis”¹⁰¹ but does not explain why he chose a different set of model structures with different assumptions and estimated the model parameters with a different algorithm.

Ultimately, Dr. Freeman dismisses the ambiguities in his statistical results and finds “little evidence to conclude that the secondary effects thesis is a robust theory that explains how adult oriented businesses impact surrounding neighborhoods.”¹⁰² However, our experience with hedonic methods, especially in secondary effects research, suggests an alternative interpretation: Hedonic methods are poorly suited to modeling the secondary effects of SOBs. The two most problematic aspects of the statistical results support this interpretation. First, Dr. Freeman’s results have *perverse* implications.¹⁰³ Second, more important, his results cannot be reconciled with the results of other presumably valid methods.

¹⁰¹ Report of Lance Freeman, p. 23.

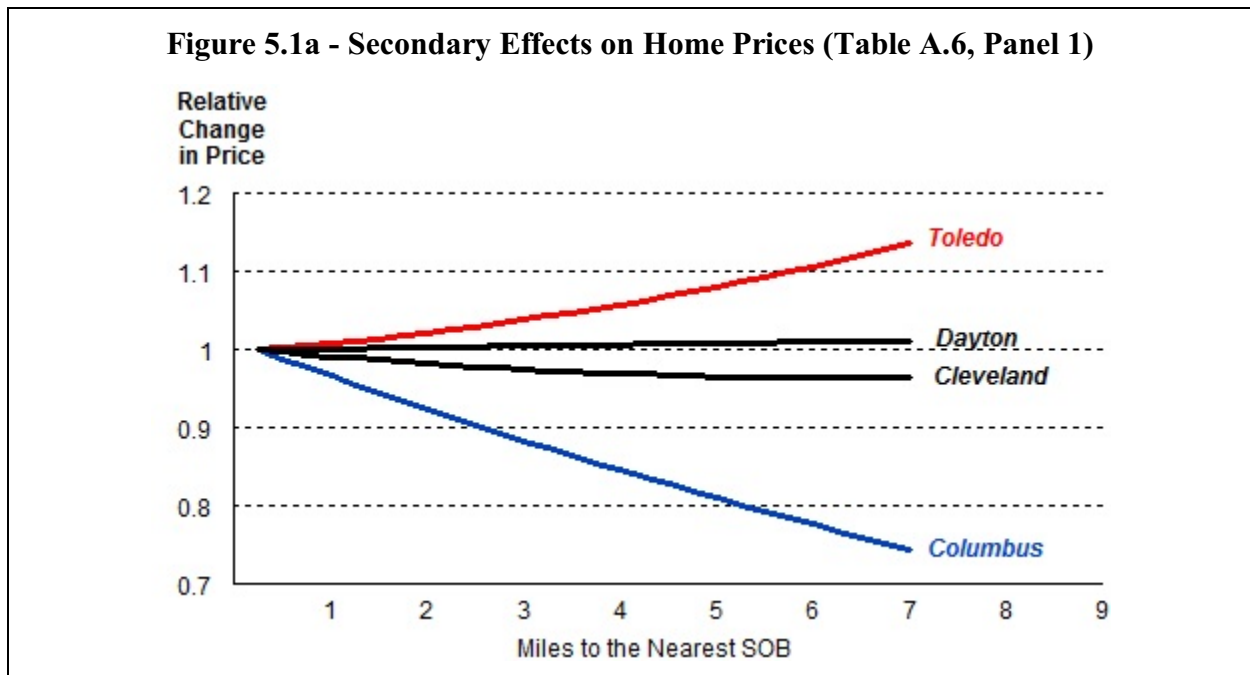
¹⁰² Report of Lance Freeman, p. 23.

¹⁰³ We use “perverse” as economists use the term. See, *e.g.*, Capozza and Van Order (1978). The term “unreasonable” might have a similar meaning for legislators.

Theoretical considerations also question the suitability of hedonic methods. Although hedonic methods have been used successfully to estimate the secondary effects of public amenities (parks, transportation hubs, *etc.*), public disamenities (landfills, power lines, *etc.*) and pollution point-sources, the secondary effects of these other land uses are qualitatively different. Whereas these other land uses might affect residential property, *e.g.*, most SOBs are located in commercial and industrial zones. The secondary effects of SOBs cover relatively small areas and are realized through independent (though correlated) pathways: crime, blight, deterioration, *etc.*

5.1 Dr. Freeman’s Perverse Statistical Results¹⁰⁴

Figure 5.1a illustrates one perverse implication of Dr. Freeman’s statistical model.¹⁰⁵ The horizontal axis gives the distance in miles (measured in quarter-mile increments) to the nearest SOB. The vertical axis gives the relative change in the sales price of an “average” home after the differences in features and neighborhoods have been controlled. Relative prices in Dayton and Cleveland change little with distance to the nearest SOB. Relative prices in Columbus and Toledo, on the other hand, continue to change with distance to the nearest SOB.

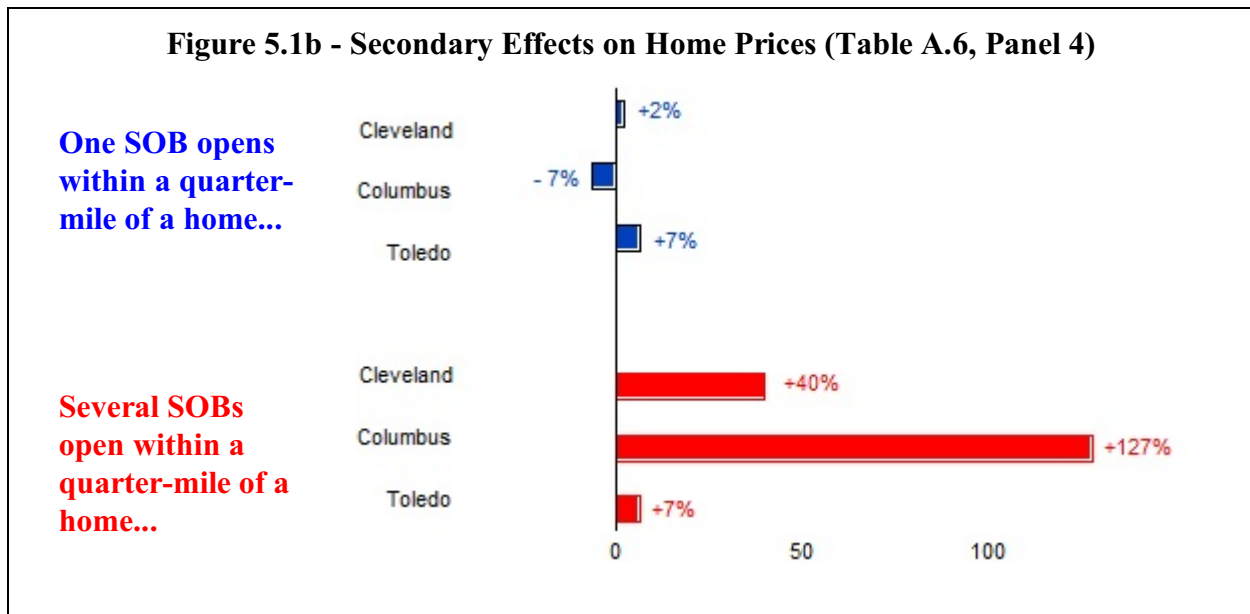


Ignoring other considerations for the moment, the price-distance plots in Figure 5.1a seem

¹⁰⁴ We use “perverse” in the sense that economists use the term. See, *e.g.*, Capozza and Van Order (1978). Many economists cite Merton’s (1936) “law of unintended consequences” as the source.

¹⁰⁵ Report of Lance Freeman, Table A.6, Panel 1, p. 35.

to support Dr. Freeman’s summary conclusion. If SOBs had the secondary effects that claimed, we might expect to see the same *positive* price-distance relationships in Cleveland, Columbus, and Dayton that we see in Toledo. Instead, we see *null* price-distance relationships in Cleveland and Dayton and a *negative* price-distance relationship in Columbus. The Achilles’ heel in this argument, of course, is that we expect to see *no relationship whatsoever* at the scale (quarter-mile increments) and distances (miles away from an SOB) that Dr. Freeman’s statistical model assumes. *Finding these relationships hints at the model’s shortcomings.*



Dr. Freeman also reports the results of statistical models that were not estimated with the assumption of an unbounded price-distance relationship. The blue bars in Figure 5.1b plot the estimated effect on the sales price of an “average” home located within a quarter-mile of an SOB, controlling for differences in features and neighborhoods.¹⁰⁶ One interpretation of these estimates is that, if an SOB opens within a quarter-mile of a home, the sales price of the home will rise by two percent in Cleveland and by seven percent in Toledo; but the sales price will fall by seven percent in Columbus.

The red bars in Figure 5.1b plot the analogous effects for several SOBs. If more than one – *i.e.*, several – SOBs open within a quarter-mile, the sales price of an “average” home will rise by 40 percent in Cleveland, 127 percent in Columbus, and seven percent in Toledo. These effect estimates have *perverse* implications for the economic behavior of buyers, sellers, and real estate agents. Those who are not well versed in economic theory might discount these estimates on the grounds that they are *unreasonable*. From either perspective, the statistical results raise serious questions about the suitability of hedonic methods for modeling secondary effects.

¹⁰⁶ Report of Lance Freeman, Table A.6, Panel 4, p. 35.

5.2 Surveys of Real Estate Appraisers

A related problem is that Dr. Freeman's statistical results are not easily reconciled with the results of other presumably valid methods. Secondary effects studies in Los Angeles (1977), Indianapolis (1984), Austin (1986), and Garden Grove (1991) used the opinions of real estate professionals to document the effects of SOBs on nearby property. Allowing for substantive and methodological variations, these surveys support the conventional wisdom that SOBs adversely affect the value of nearby property.

In their methodological critique of the secondary effects literature, Paul, Linz, and Shafer (2001) argue that, compared to the opinions of real estate professionals, analyses of actual home sales prices are preferred:

Survey evidence is not comparable to, nor can it replace, the evidence supplied by objective comparisons of ... property values ... within areas containing adult entertainment businesses, with property values ... within areas containing no such businesses. Such a comparative analysis is the preferable social scientific means by which to establish a relationship between the presence of adult entertainment businesses and ... decreases in property values.¹⁰⁷

We disagree. Paul, Linz, and Shafer (2001) offer no methodological authority for their opinion. "Survey evidence" is widely used across the social sciences because survey data are reasonably valid, relatively inexpensive to collect, and optimally flexible. This explains why surveys of real estate professionals are the preferred method for demonstrating secondary effects.

We do not argue that an hedonic analysis (such as Dr. Freeman's) and a survey of real estate professionals (such as those relied on by the State) must yield *identical* results. The results of hedonic and survey methods should be *reconcilable*, however, and that is not the case here.¹⁰⁸

A recent survey by Cooper and Kelly (2008) has typical results and, furthermore, seems particularly relevant to *J.L. Spoons*. Cooper and Kelly asked a sample of 195 Texas appraisers certified as Members of the Appraisal Institute (MAIs) or Senior Residential Appraisers (SRAs) two questions:

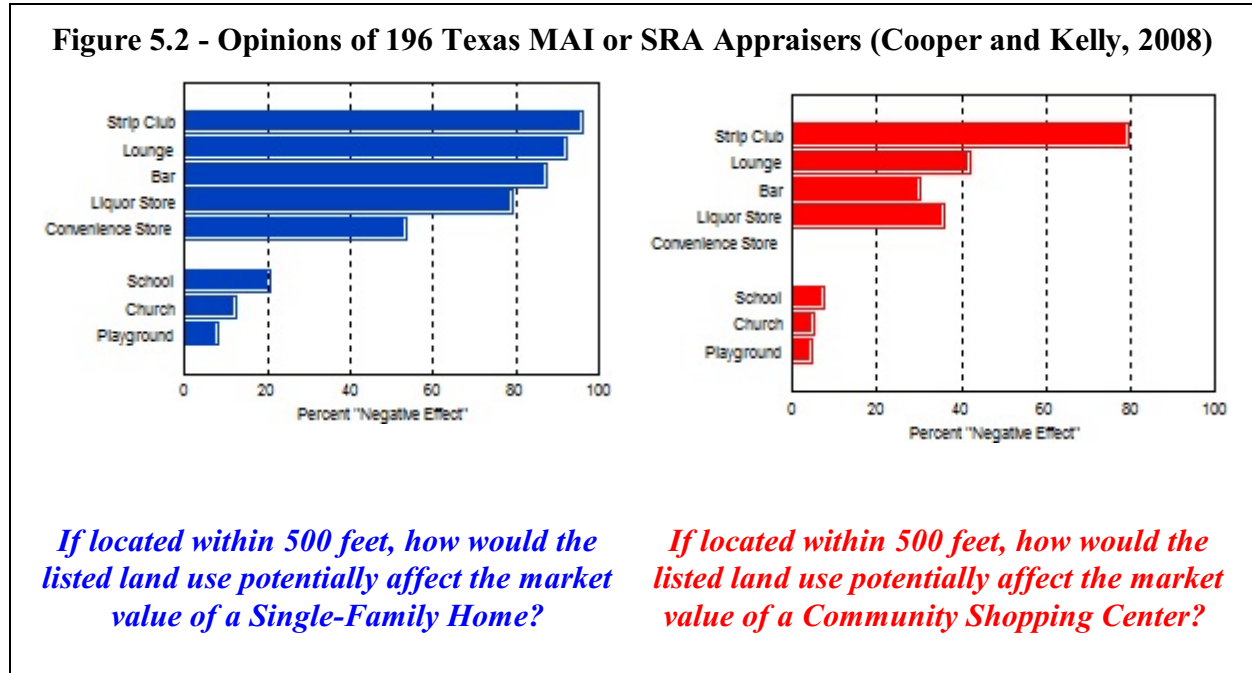
- If located within 500 feet, how would [a Gentleman's Club/Strip Club] potentially affect the market value of a single-family home?
- If located within 500 feet, how would [a Gentleman's Club/Strip Club]

¹⁰⁷ Paul, Linz, and Shafer (2001), pp. 374-5

¹⁰⁸ Brookshire *et al.* (1982) demonstrate that a properly designed hedonic study and an analogous sample survey will yield findings that are at least consistent.

potentially affect the market value of a community shopping center?

These questions were nested in a list of other land uses that included other SOB subclasses, businesses that serve alcohol along with non-adult entertainment, businesses that serve alcohol without live entertainment, businesses that sell (but do not serve) alcohol, typical NIMBY sites, and public amenities. Figure 5.2 reports several relevant contrasts.



Practically all of the appraisers (96.2 percent) believed that opening an SOB within 500 feet of a home would adversely affect the home's value. Roughly the same proportion (92.4 percent) believed that opening a non-adult cabaret (*i.e.*, a lounge that serves alcohol and offers non-adult live entertainment) within 500 feet of a home would have the same adverse effect. For residential property then, the appraisers saw only minor differences between adult and non-adult cabarets. For commercial property, in contrast, appraisers saw major differences. Whereas an overwhelming majority of the appraisers (79.6) believed that opening an SOB within 500 feet a shopping center would adversely affect the shopping center's value, only a minority (41.9 percent) – albeit a large one – believed that opening a non-adult cabaret within 500 feet of a shopping center would have the same adverse effect. *None of the appraisers believed that an SOB operating within 500 feet would affect the value of a single-family home or a community shopping center positively.*

Aside from their reasonableness, the results depicted in Figure 5.2 illustrate the salient advantages of survey methods. Due to practical constraints, Dr. Freeman was forced to estimate the secondary effects of a generic SOB (*vs.* the SOB subclass of interest) on residential property (*vs.* commercial and/or industrial property) within a quarter-mile (*vs.* 500 feet) of the SOB site. Survey methods do not have the same practical constraints. Surveys can focus on the specific

SOB subclass (e.g., “Gentleman’s Club/Strip Club”), real estate market (e.g., commercial), and effect zone (e.g., 500 feet) of interest in *J.L. Spoons*.

5.3 Hedonic Methods Are Unsuitable to Secondary Effects Research

Although Rosen (1974) developed the hedonic theory to describe market phenomena, the application to real estate markets, along with a set of methodological rules, is generally attributed to Freeman (1974, 1979). Subsequently, hedonic methods have been used to estimate the effects of air and water pollution, proximity to NIMBY sites, parks, public transportation and a range of other amenities or disamenities on property values. Other than McCarthy, Renski, and Linz (2001), however, and Dr. Freeman’s work, hedonic methods have not been widely used to model the secondary effects of SOBs. Based on this admittedly small sample, every hedonic analysis finds no secondary effects. It is no surprise that SOB plaintiffs prefer hedonic methods.

In contrast, every survey of real estate professionals finds secondary effects. Given the results of surveys in Los Angeles (1977), Indianapolis (1984), Austin (1986), and Garden Grove (1991), it seems unlikely that any survey would yield plaintiff-friendly results. This result does not explain why government defendants prefer surveys, however. In our opinion, the preference is based on practical considerations of *flexibility*, *cost*, and *understandability*.

Understandability is a major consideration. Few readers have the backgrounds required to critically evaluate the statistical results of an hedonic analysis. In contrast, survey results are easily understood by general audiences. Practical considerations of *cost* are also important. The cost of a survey of real estate professionals will often be less than half the cost of an hedonic analysis. The most important practical advantage of a survey, however, is the relative *flexibility* of the method. Whereas a survey can be designed to focus on the variables that are relevant to, say, *J.L. Spoons*, the design of an hedonic model is constrained by circumstances. Accordingly, the hedonic model can be more relevant than circumstances allow.

5.3.1 SOBs vs. the Relevant SOB Subclass

The survey by Cooper and Kelly (2008) was designed to compare the secondary effects of an SOB subclass (“Gentleman’s Club/Strip Club”) to the secondary effects of a control business (“Lounge with Live Entertainment”). The design feature optimizes the relevance of the survey results to *J.L. Spoons*. Generalizing Dr. Freeman’s results to the relevant SOB subclass would require unwarranted assumptions. Dr. Freeman does not explain why he chose to estimate the secondary effect aggregated across SOB subclasses. On purely statistical grounds, however, it is likely that estimating a disaggregated effect would have demanded a larger sample of home sales than was available.

5.3.2 Residential Properties v. Commercial-Industrial Properties

The survey by Cooper and Kelly (2008) was designed to estimate the secondary effects of SOBs on the values of both residential (“*Single-Family Home*”) and commercial (“*Community*”

Shopping Center”) properties. Since most SOBs are located in commercial-industrial zones, the secondary effects on commercial and industrial property may be more relevant than the effects on residential property. Dr. Freeman does not explain why commercial and industrial properties were excluded from the design. Considerations of statistical power (*i.e.*, sample size) certainly played a role in this decision. The inherent difficulty of comparing commercial-industrial properties must have played a larger role, however.

Hedonic methods make sense only when the price difference between two properties can be attributed to a small set of objectively measurable variables. Residential properties meet this requirement in many instances.¹⁰⁹ If two homes have similar features (age, construction, living space, *etc.*) and are located similar neighborhoods (SES, schools, parks, *etc.*), any difference in their sales price is expected to fall in the range of “sampling error.” Commercial and industrial properties are not obviously comparable, however, at least not on a few objectively measurable variables. Because used car lots, coffee shops, and medical offices have few common features, hedonic models of price differences make little sense.

Hedonic models of rent differences among commercial-industrial properties constitute a small proportion of the total literature, usually focusing on specialized market niches. If a model can be limited to small homogeneous subclass – say, offices – rent differences can be attributed to a few objectively measurable features (size, parking, security, *etc.*) and neighborhood variables (accessibility, theme, density, *etc.*). Without this focus, however, hedonic pricing methods are not well suited to the problem.

5.3.3 Omitted Variables Bias

One of the most crucial assumptions of any regression model concerns the set of variables included in the model. If a variable has been omitted from the model, and if the omitted variable is correlated with an included variable, ordinary least-squares parameter estimates are *biased* and *inconsistent*.¹¹⁰ Lacking these essential estimator properties, Dr. Freeman’s statistical results are uninterpretable. The most obvious omitted variables in Dr. Freeman’s models are neighborhood characteristics, particularly:

- ***School Quality*** - Other things equal, homes in “good” school districts will

¹⁰⁹ Dr. Freeman’s models include seven distinct residential housing markets: apartments, condominiums, duplexes, mobile homes, town homes, triplex homes and single family homes. Homes in each of these markets have distinctive features, neighborhoods, sellers and buyers. Combining the distinct markets seems to violate the conventional assumption, due to Rosen (1974), of a homogeneous market. Comparing homes in distinct markets as if they were bought and sold in same market is misleading.

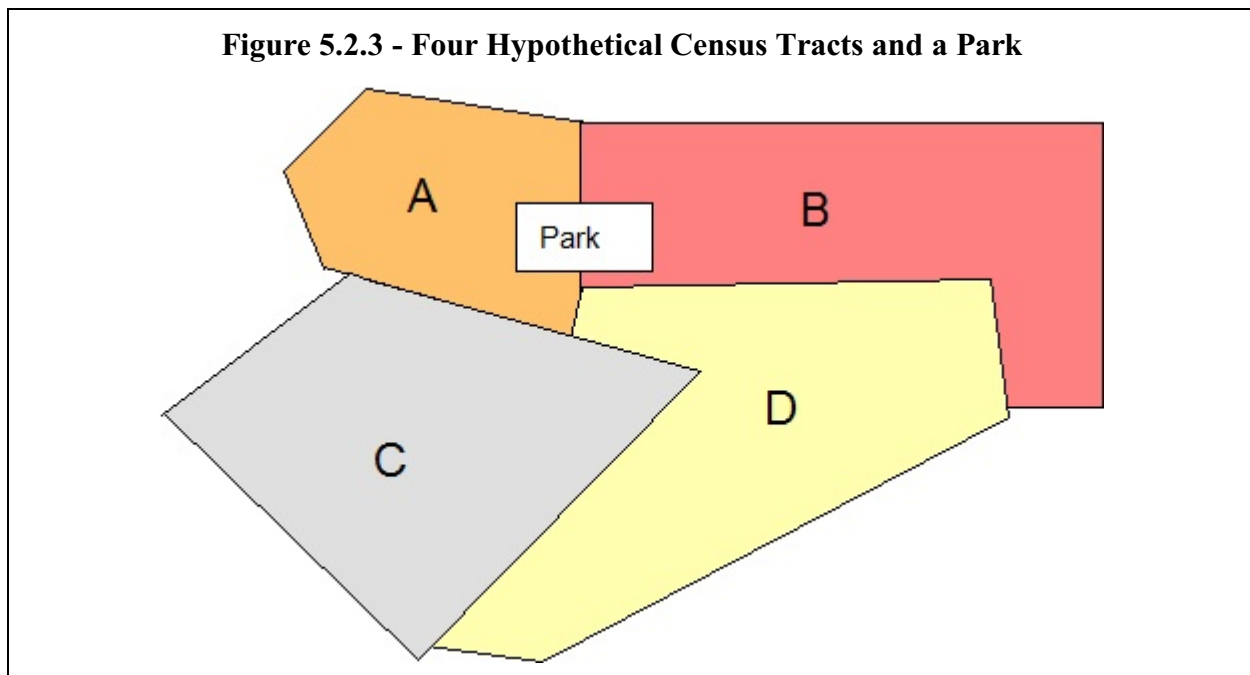
¹¹⁰ For proof, see any graduate econometrics text, *e.g.*, Judge *et al.* (1985, p. 857).

command higher sales prices.

- **Crime Rate** - Other things equal, homes in “safe” neighborhoods will command higher prices.
- **Access to Public Amenities** - Other things equal, homes within walking distance of parks and playgrounds will command higher prices.

And so on and so forth. Dr. Freeman relies on fixed-effects estimators to account for these (and all other) omitted neighborhood characteristics.¹¹¹

In principle, fixed-effects estimators can account for omitted neighborhood characteristics if the characteristics vary discretely across neighborhoods. Dr. Freeman’s neighborhoods are *not* neighborhoods in the ordinary sense, however. Lacking data on “organic neighborhoods,” Dr. Freeman uses Census Tracts as a proxy. Unfortunately, the neighborhood characteristics that affect home prices vary within and “spill out” across Census Tracts.



Hypothetical Census Tracts **A**, **B**, **C**, and **D** in Figure 5.2.3 illustrate the inadequacy of fixed-effects estimators. Tracts **A** and **B** have a (shared) Park while Tracts **C** and **D** do not. Fixed-effects estimators assume that each of the homes in Tracts **A** and **B** benefit equally from proximity to the Park but that none of the homes in Tracts **C** and **D** benefit at all. In fact, some of

¹¹¹ Report of Lance Freeman, p. 14: “To control for the effect of neighborhood characteristics we used a census tract fixed effects approach.”

the homes in Tracts **C** and **D** are closer to the Park – and thus, derive a greater benefit from the Park – than some of the homes in Tracts **A** and **B**.

In this hypothetical (albeit realistic) example, the fixed-effects estimator does not *solve* the statistical problems raised by the omitted variable, but rather, aggravates the problem. In a curious footnote, Dr. Freeman alludes to replicating his models without fixed-effects estimators:

Although the results are not included in this report we also estimated models that did not include neighborhood fixed effects. One may argue that inclusion of neighborhood controls with distance from adult business measures could lead to an overidentified model which would bias our results towards not finding a significant relationship between distance from an adult business and property value. However, the results from the models that exclude the neighborhood controls do not substantively change the general conclusions of this report.¹¹²

The fact that fixed-effect and ordinary least-squares estimators yield indistinguishable results would suggest to many economists that the basic model is improperly specified. We concur in that opinion. In this particular instance and in general, the straightforward solution to the omitted variables problem – measuring and incorporating all of the relevant variables in the model – is impractical.

5.3.4 Spatial Dependence

Spatial dependence poses another obstacle to the hedonic method. The sales price of a home is ordinarily affected by the condition of nearby properties and by the sales price of similar homes in the neighborhood. This implies *spatial dependence* or *spatial autocorrelation* among home sales. Dr. Freeman notes this problem:

[W]e argue that real property may indeed be affected by surrounding neighborhood attributes including the value of adjacent properties, which is a violation of [model error terms] being independently distributed. So to correct for this autocorrelation ... as well as any violation of the assumption that [model error terms are] identically distributed, we estimate our models using the Huber/White/Sandwich corrected standard errors.¹¹³

Huber-White-Sandwich estimators adjust *standard errors* of estimation for the effects of spatial dependence, particularly for violations of the model's homoskedasticity assumption. Huber-White-Sandwich estimators do *not* correct for *estimator bias*, however. Freedman's (2006, p. 1) statement on this point is typical.

¹¹² Report of Lance Freeman, footnote #1, p. 16.

¹¹³ Report of Lance Freeman, Ph.D., p. 16.

The “Huber Sandwich Estimator” can be used to estimate the variance of the [maximum likelihood estimator] when the underlying model is incorrect. If the model is nearly correct, so are the usual standard errors, and robustification is unlikely to help much. On the other hand, if the model is seriously in error, the sandwich may help on the variance side, but the parameters being estimated by the [maximum likelihood estimator] are likely to be meaningless – except perhaps as descriptive statistics.

If Dr. Freeman’s Huber-White-Sandwich estimators are *biased* – and that is a certainty – knowing that the *standard errors* of the biased estimators are *consistent* is no consolation.

The hedonic pricing literature has used three basic modeling approaches to control the estimator bias due to spatial dependence: spatial lag models (Armstrong and Rodriguez, 2006; Kim, Phipps and Anselin, 2003); spatial error models (Boxall, Chan and McMillan, 2005); and spatial Durbin models (Brasington and Hite, 2005; Li and Saphores, 2009). Each approach has unique strengths and weaknesses. McCarthy, Renski, and Linz (2001) use a spatial lag models estimated in a system of “seemingly unrelated regressions.”

5.4 Concluding Remarks

Although hedonic methods have been widely used to estimate the effects of public amenities, public disamenities, and pollution sources on property values, they are a novel method in this field. Ignoring the question of whether Dr. Freeman’s secondary effect estimates are *credible*, they are *irrelevant* to *J.L. Spoons*:

- Dr. Freeman’s estimates apply to the broad class of SOBs. There are no estimates for the SOB subclass of interest in *J.L. Spoons*.
- Dr. Freeman’s estimates apply to residential properties only. There are no estimates for the commercial or industrial properties that are likely to be involved in *J.L. Spoons*.
- Dr. Freeman’s estimates assume that secondary effects can be detected at a distance of one quarter-mile. The secondary effects in *J.L. Spoons* affect a smaller area and are overlooked by Dr. Freeman’s hedonic model.

Ignoring the question of relevance, theoretical considerations suggest that hedonic pricing methods are unsuited to the secondary effects of SOBs. The perverse implication of Dr. Freeman’s statistical results reinforce this view. Forced to choose between Dr. Freeman’s results (Figure 5.1a-b above) and the results of a traditional survey (Figure 5.2 above), reasonable people would reject Dr. Freeman’s results and would accept the survey results.

6. 911 Calls as a Measure of Ambient Crime Risk

Secondary effects studies have used three measures of ambient crime risk: crime incident reports (*e.g.*, UCRs), 911 calls, and to a lesser extent, victimization surveys. Given the “rare event” property of crime, victimization surveys are an impractical means of demonstrating the presence (or absence) of secondary effects. Crime incident reports are a more reliable measure, compared to 911 calls, and preferred by criminologists. For the same reason, plaintiffs’ experts prefer to use 911 calls. Dr. Linz’s Greensboro and Daytona Beach studies, reviewed at §3.1-2 above, and his Four Ohio City study, reviewed at §4.5.4 above, are typical.

All large police agencies record 911 calls for planning and budgeting purposes.¹¹⁴ In a pinch, 911 databases can generate “quick and dirty” snapshots of crime problems. In the long run, however, police agencies use crime incident reports to measure ambient crime risk, not 911 calls. Criminologists share this view. The reasons why plaintiffs’ experts prefer 911 calls would be obvious to most criminologists. Asked to defend their preference, plaintiffs’ experts argue – incorrectly – that criminologists routinely use 911 calls to measure ambient crime risk.

Many criminologists have employed citizens’ telephone calls-for-service (CFSs) to police dispatch centers to measure crime at the address (Sherman, Garten, & Burger, 1989), neighborhood (Bursik, Grasmick & Chamlin, 1990; Warner & Pierce, 1993), and city (Bursik & Grasmick, 1993) levels. According to its proponents, the CFSs measure offers a more valid description of aggregate levels of crime than either police records collated in the FBI’s Uniform Crime Reports (UCR) or victimization data collected in the National Crime Survey.¹¹⁵

This is a half-truth at best. Shortly after the advent of computerized 911 systems, criminologists did indeed experiment with 911 calls, sometimes even using them as surrogate measures of crime risk. The research cited by these experts was conducted during this early period. However, the results of this early research led to the consensus view that 911 calls are not the *best* – or even a *good* – measure of ambient crime risk.

Today, few criminologists study 911 calls for any reason; but no criminologists study 911 calls to learn about ambient crime risk. The published literature review summarized in Table 6 supports this point. During a recent five-year period, four general criminology journals published 705 items. Most of the items were either non-empirical (essays, reviews, *etc.*) or else, analyzed phenomena other than crime (police behavior, sentencing decisions, *etc.*). Of the 254 articles that analyzed a crime statistic, 134 (52.8 percent) analyzed UCRs; 119 (46.8 percent) analyzed victim or offender surveys. Only five items (1.9 percent) analyzed 911 calls. Of these five, *only one*

¹¹⁴ These legitimate uses of 911 calls are discussed in most undergraduate policing texts. See, *e.g.*, Roberg, Crank and Kuykendall (1999).

¹¹⁵ Linz, Paul, and Yao (2006), p. 191.

used 911 calls as a crime risk measure.

| | Total Items | Crime Stats | UCRs | Survey | 911 Calls |
|--------------------------------------|------------------------|------------------------|-------------|---------------|----------------------|
| <i>Criminology</i> | 193 | 52 | 37 | 16 | 0 |
| <i>Justice Quarterly</i> | 152 | 48 | 23 | 23 | 2 |
| <i>J of Quantitative Criminology</i> | 95 | 47 | 30 | 17 | 0 |
| <i>J of Criminal Justice</i> | 265 | 107 | 44 | 63 | 3 |
| | (705) | (254) | (134) | (119) | (5) |

The 1997 Fulton County study and the 2001 replication are unique among government-sponsored secondary effects studies. Otherwise, the use of 911 calls to measure ambient crime risk is as rare as hen’s teeth. More important, of course, recent case law supports the views of criminologists and governments.¹¹⁶ In short, analyses of 911 calls are not sufficient to meet the standards required under *Alameda Books* to cast doubt on the secondary effects evidence relied on by the government to support an ordinance.

6.1 Why Plaintiffs’ Experts Prefer 911 Calls

Plaintiffs’ experts prefer to use 911 calls because, first, 911 calls are relatively “noisy.” This statistical property obscures the difference between high- and low-risk public safety hazards. Second, compared to crime incident reports, 911 calls provide a biased picture of the vice crime incidents that weigh so heavily in the secondary effects of SOBs. Third, compared to crime incident reports again, 911 calls are easily manipulated. We cover these three points in order.

6.1.1 911 Calls Are a “Noisy” Measure of Crime Risk

“Sound” is a useful analog to “ambient crime risk.” Whereas the sound of a party is easily detected by next-door neighbors, neighbors who live two blocks away must listen carefully to separate the voices, music, and other party sounds from the sounds of wind, traffic, and other natural background “noise.” Four blocks away, party sounds are overwhelmed by background

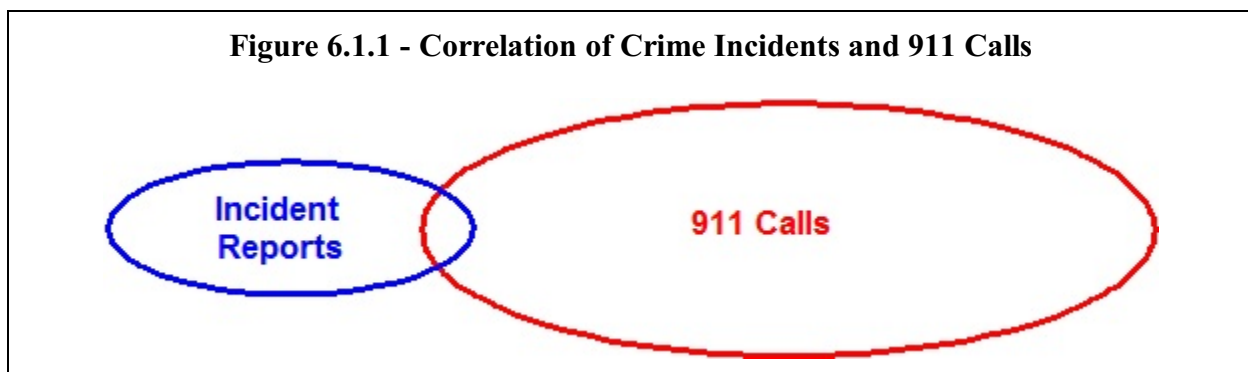
¹¹⁶ The Eleventh Circuit outlined the limitations of 911 calls in *Daytona Grand* and pointed out (fn. 33) that three other Circuits had rejected attempts by plaintiffs to use 911 calls to cast direct doubt on an ordinance: *Gammoh v. City of La Habra*, 395 F.3d 1114, 1126-27 (9th Cir. 2005), *G.M. Enter., Inc.*, 350 F.3d 631, 639 (7th Cir. 2003), and *SOB, Inc.*, 317 F.3d 856, 863 & n.2 (8th Cir. 2003).

“noise.” Neighbors who live four blocks away would need exotic sound detection equipment to separate the meaningful sounds of a party from meaningless background “noise.”

The analogous problem arises when we try to attribute crimes to an SOB point-source. Crime incidents occur sporadically in any large neighborhood with no apparent causal source. In purely statistical terms, these sporadic incidents are background “noise.” When a crime occurs within, say, 500 feet of the SOB, we can be reasonably confident that it is a secondary effect of the SOB, not a sporadic incident. But as one moves away from the SOB, the relative frequency of sporadic crimes rises and confident attribution becomes more difficult.

In either case, of course, the statistical task is more difficult problem when the criterion signal – party sounds or crime incidents – is not easily distinguished from background noise. To illustrate, suppose that the party music were muted and atonal or that the guests spoke in foreign languages. The task of separating the sounds of such a party from typical background noise might be relatively difficult.

The analogous principle applies to ambient crime. Suppose that burglary 911 calls are used to measure the burglary rate. Since *most* of these 911 calls are triggered by malfunctioning alarms, it is difficult to distinguish real burglaries from false alarms.¹¹⁷ Since *most* addresses are *not* protected by burglar alarms, moreover, it is difficult to distinguish high- and low-risk addresses.



As a measure of ambient crime risk, the salient shortcoming of 911 calls is that they are too “noisy.” Figure 6.1.1 depicts this fact. Since 911 calls outnumber crime incidents by a great factor in every jurisdiction, the area accorded to 911 calls (in red) is larger than the area accorded to crime incidents (in blue). The correlation between the two measures is proportional to their overlapping area. The larger the area of overlap, relative to the combined total area, the stronger

¹¹⁷ In their San Diego study, Drs. Linz, Paul, and Yao (2006) counted 147,127 burglary 911 calls. Of these, 74.8 percent were alarm-initiated, 99.1 percent of which turned out to be false.

the correlation. In this case, the correlation is relatively small or weak.

The non-overlapping areas in Figure 6.2.1 fall into two categories. The first consists of 911 calls that have nothing to do with crime. Examples include duplicated or unfounded calls; calls that have no apparent basis; and calls that are precipitated by false alarms.¹¹⁸ The second consists of crimes that circumvent the 911 system and, thus, leave no record. Examples include crimes that the police discover through routine or proactive patrolling and crimes that the police discover through specialized unit activity, especially “victimless” vice crimes, particularly drugs and prostitution. This raises the issue of statistical biases in 911 calls.

6.1.2 911 Calls Are a Biased Measure of Some Crime Risks

Whereas “noise” affects all sub-samples equally, tending to obscure differences between subsamples, “bias” affects subsamples differently. There are two known biases in 911 calls. The first affects the reporting of the vice crimes that weigh heavily in secondary effects. To illustrate the magnitude of this bias, Drs. Linz and Paul analyzed 21,132,503 New York City 911 calls. Of these, 41 were initiated by prostitution incidents.¹¹⁹ These numbers speak for themselves.

A second bias affects the address-specific (“hotspot”) analyses used in the 1997 Fulton County Police Department reports. Address-specific comparisons assume that the address recorded on a 911 record is the address where the precipitating crime occurred. The address on a 911 record instructs responding patrol units where they go to “see the man,” however, and this is often not the address of the precipitating incident. If X calls 911 to report a disturbance at Y’s house, *e.g.*, the responding patrol unit will be asked to “see the man” at X’s address. Of course, significantly many 911 calls are attributed to places (“Third and Main”), not addresses (“321 East Main”). Business proprietors who are aware of this convention are able to manipulate the 911 system.

6.1.3 911 Calls Are Easily Manipulated

One reason why SOB plaintiffs might prefer 911 calls is that, because relatively few “victimless” crimes (drugs, prostitution, *etc.*) come in through 911 channels, 911 calls understate the incidence of these crimes by a large factor. Another reason is that 911 calls can be used to mask an address-specific public safety hazard. This last problem merits special comment. If a business is familiar with the coding conventions, 911 records can be manipulated to make the

¹¹⁸ More than 80 percent of the 607,903 911 calls analyzed by Linz, Paul, and Yao (2006) were cancelled, duplicated, unfounded, disposed of without report, or had some other non-crime disposition.

¹¹⁹ Linz, D. and B. Paul. *Measuring the Secondary Effects of 60/40 Businesses in New York City: An Analysis of Calls for Service to the Police*. April 14th, 2005.

business look more or less in need of police service. To build a case for more police service, the proprietor can complain to the police about problems that might otherwise be handled informally. Or alternatively, to mask a public safety hazard, the proprietor can handle problems informally, thereby creating fewer 911 records and making the business seem safer than it actually is.

The testimony of a private investigator retained by Sandy Springs, a city in Fulton County, documents the manipulation 911 records by the management of local SOBs. After witnessing the beating of an unruly, inebriated patron, the undercover investigator asked an employee why the management had not called the police:

And she said: Well, the police are never called. The police don't come around here ... It's obvious that the clubs are attempting to hide crime statistics. If they don't call the police or call for service, then there's no report made and there's no evidence that there's been a problem at the clubs ... We did look at the crime statistics for Fulton County and in that area, and there are very few complaints. I witnessed two fights that night, the 25th, that I was in there. While we were in at other times, other fights were observed. No police calls were shown on those particular nights. The club didn't call. Other patrons didn't call. The person who got beat up didn't call. Again, it just seems like they're handling this inside the club for there not to be statistics out there.¹²⁰

With obvious exceptions, manipulations of this sort are legal, strictly speaking. At the extreme, however, manipulating the 911 record-keeping system crosses the line. In a recent Manatee County case, for example, an SOB bribed at least two deputies to illegally circumvent and/or to falsify 911 records.

Another Manatee deputy, Daniel E. Martin, 35, told sheriff's investigators that one of the Cleopatra's door girls had his cell phone and would call him personally to quell customer disturbances ... Former Manatee deputy Joshua R. Fleischer, 25, who resigned this month, told a detective that whenever he was dispatched to Cleopatra's for a disturbance he listed the address as the "3900" block of U.S. 41 – deliberately misidentifying the actual address in the 3800 block. Fleischer, according to the detective, did not want his reports associated with the club.¹²¹

The investigation into this scandal has spread to surrounding counties. The relevant point, for our purposes, is that business proprietors who are familiar the 911 geo-coding conventions can (and in Manatee County, at least, *do*) attempt to manipulate the system.

¹²⁰ Mr. Guy Watkins, Business Consulting and Investigations, Inc., pp. 91-2, December 20th, 2005 transcript of the Sandy Springs City Council Meeting .

¹²¹ *StripClub News*, September 22nd, 2006, "Investigation tied to strip club leads to resignations and charges."

6.2 Concluding Remarks

As a measure of ambient crime risk, 911 calls are less reliable than crime incident reports; are biased against the crimes most closely associated with SOBs; and are easily manipulated to mask address-specific public safety hazards. None of these statistical shortcomings is “fatal.” On the contrary, given adequate time, resources, and data, each of these threats to validity can be controlled – albeit at great expense. This raises the question of why anyone might *want* to use 911 calls to measure ambient crime risk. One answer is that each of the threats favors the null; secondary effects are always “smaller” when 911 are used as the risk measure. Experts retained by SOB plaintiffs offer other answers. First, compared to crime incident reports, 911 calls are a “better” measure of ambient crime risk. Second, 911 calls are a widely used measure of risk, not only in the secondary effects literature relied on by legislatures but, also, in basic criminological research.

Both arguments are incorrect, of course. In purely statistical terms, 911 calls are not an acceptable measure of ambient crime risk. Nor are 911 calls widely used in secondary effects studies or criminological research. The published literature review summarized in Table 6 depicts the use of 911 calls in criminological research accurately. Few criminologists study 911 calls for any reason; but no criminologists study 911 calls to learn about ambient crime risk.

An analogous statement can be made for the secondary effects literature. Relatively few government-sponsored studies have used 911 calls any purpose whatsoever. Other than the 1997 Fulton County study, studies based on 911 calls are as rare as hen’s teeth. Studies commissioned by SOB plaintiffs are another matter. Since 2001, 911 calls have become the preferred measure of ambient risk in plaintiff-sponsored studies.

7. *Negativa Non Sunt Probanda*¹²²

To cast doubt on a government’s secondary effects evidence, plaintiffs’ experts conduct a study that finds no secondary effects – *a null finding*, in other words. Dr. Linz’s study of Four Ohio Cities, reviewed at §4.5.4 above, is typical. Although plaintiffs’ experts interpret their null findings to mean that SOB’s have no secondary effects, in fact, their null findings have a simpler interpretation: the design of their studies lacked the level of statistical power required to detect a secondary effect.

A mundane analogy illustrates the interpretational dilemma by a scientific null finding. If you cannot find your car keys, you might be tempted to conclude that your car keys do not exist. Although that is entirely possible, it is also possible (and more likely) that you did not look hard enough for your car keys or that you were looking in the wrong place. By analogy again, if a “quick and dirty” study finds no statistically significant secondary effects – a null finding – it is entirely possible that no secondary effects exist. But it is also possible (and more likely) that the study was “too quick” or “too dirty.”

Analyzing San Diego 911 calls, Drs. Linz, Paul, and Yao (2006) found that SOB areas had 15.7 percent more calls than control areas. This is a *substantively* large effect. Indeed, the budgetary implications of a 15.7 percent increase in 911 calls boggles the mind. The effect was *statistically* small, however – *i.e.*, not statistically significant. Ignoring the substantive size of the effect, Drs. Linz, Paul, and Yao (2006) argued that the statistically insignificant effect proved that the *real* effect was zero – or in other words, that no real effects existed.

Their null finding has other interpretations, of course. Reanalyzing the San Diego data, McCleary and Meeker (2006) discovered that Drs. Linz, Paul, and Yao (2006) had failed to find the significant secondary effect because they had not looked “hard enough” for it. A reanalysis using a more powerful statistical design found the significant secondary effect.

Figure 7a - Jury Trials and Hypothesis Tests

| | 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 |

¹²² This translates roughly to “Finding nothing proves nothing.”

Figure 7a summarizes the principles of statistical hypothesis testing by analogy to a jury trial. Suppose that an SOB stands accused of posing an ambient crime risk. After hearing the evidence, the jury can convict, acquit, or hang. If the jury convicts, there is a small (but non-zero) probability that the jury convicted an innocent SOB; *i.e.*, a false-positive (or “Type I” or “ α -type”) error. If the jury acquits, on the other hand, there is a small (but non-zero) probability that the jury acquitted a guilty SOB; *i.e.*, a false negative (or “Type II” or “ β -type”) error. Finally, if the jury hangs, there was no decision and, hence, no possibility of error.

In real-world courtrooms, the probabilities of false-positive and false-negative verdicts is unknown. Courts enforce strict procedural rules to minimize these probabilities but we can only guess at their values. In statistical hypothesis testing, on the other hand, the values are set by rigid conventions, to five percent for false-positives and twenty percent for false negatives.¹²³ Adopting these same values, to convict, the jury must be 95 percent *certain* of the SOB’s guilt. To acquit, the jury must be 80 percent *certain* of the SOB’s innocence. To ground the 95 and 80 percent certainty levels, we could try each case in front of a large number of independent juries. To convict, 95 percent of the juries would have to return the same guilty verdict; in the case of an acquittal, 80 percent would return the same not guilty verdict.

Correct decisions are painted blue in Figure 7a. Five percent of all convictions are false-positives and 20 percent of all acquittals are false-negatives. Incorrect decisions are painted red in Figure 7a. When the levels of certainty are too low to support conviction *or* acquittal, of course, the jury hangs. Non-decisions, painted yellow in Figure 7a, depend on factors such as the strength of evidence, credibility of witnesses, and so forth. So as not waste a jury’s time, the prosecutor doesn’t bring obviously weak cases to trial. Likewise, faced with strong evidence of guilt, the defense counsel seeks a plea bargain in order to avoid trial.

The analogy to statistical hypothesis testing is nearly perfect. The researcher considers two complementary hypotheses. The SOB either has secondary effects; or alternatively, the SOB does not have secondary effects. Based on the magnitude of the expected and estimated effects, the researcher then accepts one of the two hypotheses.

- If the false-positive rate for the estimated is smaller than five percent, the hypothetical secondary effect is accepted with 95 percent *confidence*. The SOB

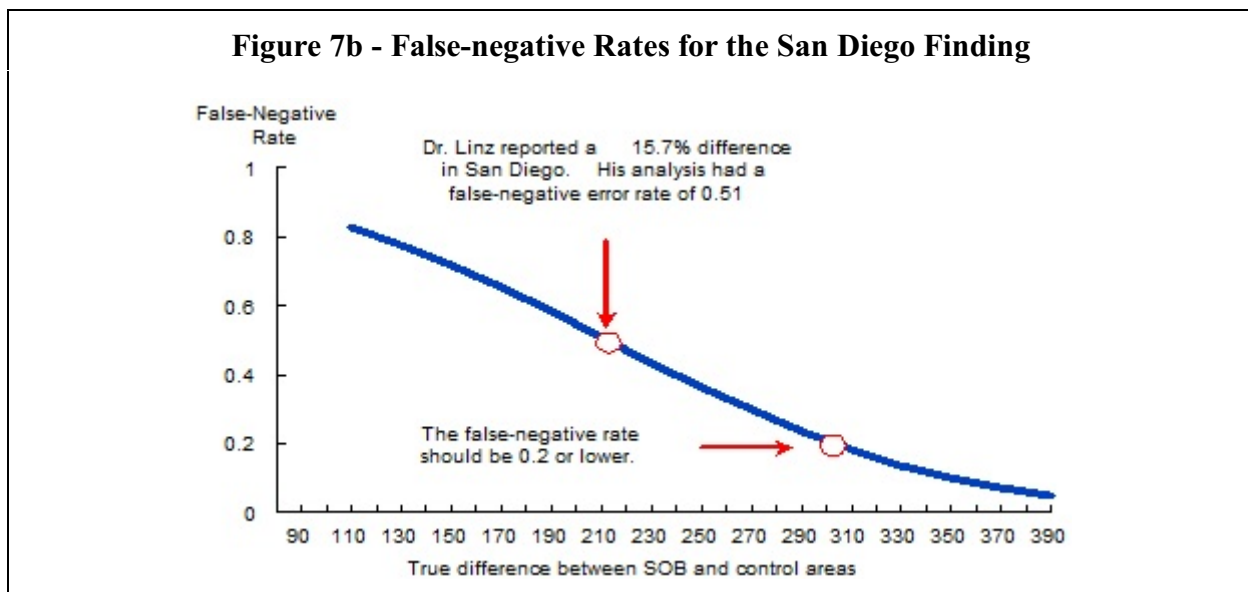
¹²³ The most comprehensive authority on this issue is Chapter 22 of Kendall and Stuart (1979). This authority requires a strong background in mathematics, however. Cohen (1988) and Lipsey (1990) are more accessible. Both Cohen (1988, pp. 3-4) and Lipsey (1990, pp. 38-40) set the conventional false-positive and false-negative rates at $\alpha=.05$ and $\beta=.2$, respectively. These rates can be set lower, of course. The convention also sets the ratio of false-positives to false-negatives at 4:1, implying that false-positives are “four times worse than” false-negatives. The 4:1 convention dates back at least to Neyman and Pearson (1928). It reflects a view that science should be conservative. In this instance, *e.g.*, the 4:1 convention works in favor of the SOB. When actual decision error costs are known, the actual ratio is used.

has a large, significant secondary effect.

If the false-positive rate is larger than five percent, the researcher does not automatically accept the alternative hypothesis but, rather, conducts a second test.

- If the false-negative rate for the expected effect is smaller than twenty percent, the alternative hypothesis is accepted with 80 percent *power*. The SOB does not have a secondary effect.

But lacking *both* 95 percent confidence *and* 80 percent power, neither hypothesis is accepted; *the results are inconclusive*. Since inconclusive results invariably arise from weak research designs, and since the relative strength of a design is known *a priori*, inconclusive results should be rare. But in fact, many of the secondary effects studies sponsored by SOB plaintiffs have inconclusive results. An example illustrate the plaintiffs' rationale.



McCleary and Meeker (2006) calculated the false-negative error rates plotted in Figure 7b from statistics reported by Drs. Linz, Paul, and Yao (2006). As shown, the reported 15.7 percent secondary effect estimate has a false-negative rate of .508. What this means, simply, is that the reported null finding is more likely (51 percent) to be *incorrect* than it is to be correct (49 percent). The effect would have to exceed 22.7 percent (304.5 calls) before it could be detected with the conventional level of 80 percent power.

The mathematics of statistical hypothesis testing is so demanding that few social scientists

understand the concepts or their importance to research.¹²⁴ The conventional 80 percent power level was proposed and adopted in the 1920s when statistical hypothesis testing was in its infancy. The convention has survived for eighty years because it serves two useful, crucial functions.

- Anyone with a modest background in research methods can design a study in a way that favors – or even guarantees – a null finding. The convention minimizes abuses by malicious investigators.
- Haphazardly designed “quick and dirty” studies favor the null finding. The convention minimizes the impact of spurious findings generated by naive (but benign) investigators.

Lay audiences, who must rely on common sense, cannot always distinguish between weak and strong designs or between benign and malicious investigators. Scientific conventions guard against both abuses. In this particular instance, the 80 percent power convention allows the lay audience to trust the validity of a null finding.

Recognizing the conventions, secondary effect studies can be assigned to one of three categories: studies that report secondary effects with 95 percent *confidence*; studies that report null findings with 80 percent *power*; and studies that are *inconclusive*. All of the studies listed in Table 1 above either report large, significant secondary effects or else are *inconclusive*. No studies report null findings with the conventional 80 percent power. This reinforces a statement that we made in the introduction to this report: It is a *scientific fact* that SOBs pose large, significant ambient crime risks.

¹²⁴ *E.g.*, “I attributed this disregard of power to the inaccessibility of a meager and mathematically difficult literature...” (p. 155, Cohen, 1992).

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In formulating the opinions described in §1-7 above, we relied upon research reports written by ourselves and others, and on standard authorities that would be available in any research library.

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