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DISSERTATION

The Ecological Context of Substance Abuse Treatment Outcomes

Implications for NIMBY Disputes and Client Placement Decisions

JERRY O. JACOBSON

This document was submitted as a dissertation in June 2004 in partial fulfillment of the requirements of the doctoral degree in policy analysis at the Pardee RAND Graduate School. The faculty committee that supervised and approved the dissertation consisted of Richard Hillestad (Chair), Ricky Bluthenthal, and Jonathan P. Caulkins.

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Abstract

Treatment is an important part of the war on illicit drugs. However, most of the more than 1.1 million annual admissions to treatment end in client dropout. Why treatment often ends this way is largely unknown, though scholars have examined a number of factors related to client characteristics and program components. Absent from research on treatment outcomes to date is location: the physical, social, and economic attributes of neighbourhoods where treatment clients live and receive treatment. The omission is surprising, given that drug use is often viewed as a societal pathology with its roots in a number of factors that depend on local conditions. I develop hypotheses of the influence of "treatment ecology" on retention, characterize the residential and treatment environments of the population of treatment clients in Los Angeles County in the period 1998-2000, construct multi-level Bayesian models to test for an association between neighborhood-level factors and client retention using individual-level episode data for publicly-funded programs, and derive bounds on the expected impact of location-oriented policies on individual and countywide retention. Four contextual factors are examined: drug availability, social stressors, proximity to jobs, and proximity to retail establishments. Small-area proxy measures of each are developed using Census data and administrative data from a number of state and county agencies. I find that clients' residential environments are significantly worse than those of the non-client household population, particularly with respect to social stressors and drug availability, that the neighborhoods of treatment centers are worse still, and that homeless, African American, and other minority clients face the worst environments overall. Failure to complete in both outpatient and residential settings is associated with neighborhood-level social stressors. Provided these associations are causal, which remains to be shown, a policy that matched all clients with the most appropriate neighborhood would increase the county-wide rate of retention by up to 30%, resulting in 1670 additional completions in the first year of such a policy. Neighborhood-level variation in L.A. is such that, for each additional completion in residential care, one would need to invest 6.25 times more treatment capacity in the worst neighborhood compared to the best neighborhood (2 to 1 in outpatient). I review the literature on Locally Unwanted Land Uses and determine that while these analyses would be useful for selecting where to expand treatment, they are not likely to persuade opposed residents to host an unwanted treatment facility.

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Chapter 1

Introducing the Notion of a Treatment Ecology

I Introduction

Substance abuse treatment programs in the U.S. that receive public funds logged more than 1.1 million admissions involving an illicit drug problem annually from 1992 to 2000.[1] However, completion rates and engagement on the part of the participant in these programs are generally low. For example, statewide estimates from the California Alcohol and Drug Data System suggest that nearly 70% of drug treatment clients admitted to residential programs stay less than 90 days (Maglione, Chao and Anglin 2000) and the growing body of retention studies typically reports rates of attrition from about 25% to 75%, depending on the treatment modality and definition of dropout (e.g., Stahler, Cohen and Shipley 1993, Joe, Simpson and Broome 1998, Hiller, Knight and Simpson 1999, De Leon, Hawke, Jainchill and Melnick 2000, Lang and Belenko 2000, Veach, Remley and Kippers 2000). Client attrition is considered by some to be "one of the greatest problems interfering with treatment effectiveness in substance abuse programs" generally (Stahler, et al. 1993) and "one of the main challenges" facing clients coerced into treatment by the criminal justice system (Sia, Dansereau and Czuchry 2000). Indeed, previous investigations into the causes of attrition have been motivated not only by high dropout rates, but by the finding that a longer stay in treatment is among the few consistent predictors of better post-treatment outcomes (see Anglin and Hser 1990 for a review, De Leon 1990, Hubbard, Craddock, Flynn, Anderson and Ethridge 1997). Although prior studies have examined a broad range of factors related to client background, severity and nature of abuse, and components of the treatment program, as a rule they have come up short in explaining much of the observed variation in retention among treatment clients.

One aspect that remains unexamined by the literature on retention to date is the role of geographic and neighborhood context. Later sections of this chapter will argue that there is good reason to believe that many of the contextual factors that define the treatment client's living and treatment environments and their interactions may play a causal role in both voluntary and involuntary dropout decisions. Despite passing mention of the importance of neighborhood context and treatment facility location in the literature on treatment outcomes more generally (Davis and Tunks 1990-1991, Tucker, Vuchinich and Gladsjo 1990-1991,

1 This figure is based on the Treatment Episode Data Set (TEDS) series, made publicly available on-line by the Substance Abuse and Mental health Data Archive (SAMHDA) and the Inter-University Consortium for Political and Social Research (ICPSR); accessed December 1, 2003. Admissions involving a primary, secondary, or tertiary substance other than alcohol or an over-the-counter drug were assumed to involve an illicit drug problem.

Iguchi and Stitzer 1991, Rosenhow, Niaura, Childress, Abrams and Monti 1991, Joe, Simpson and Sells 1994), thoughtful consideration of the causal mechanisms to guide hypothesis generation and testing in the area of retention has been lacking. Attention to causal mechanisms is particularly important in neighborhood effects studies, however, where self-selection into neighborhoods make endogeneity and confounding a real concern and where potential, systematic differences in variation across subjects within neighborhoods raise problems for traditional regression models. In other fields, examination of features of the physical environment on outcomes has been characterized by hasty interpretation of empirical results prior to careful attention to theory, prompting criticism and calls for a change of course (Macintyre, Ellaway and Cummins 2000, Oakes and Kleinman 2001). The purpose of this chapter is to establish a range of hypotheses motivated by previous empirical findings to guide future identification and measurement of the effects of geographic context on attrition from treatment.

To fix ideas, I use the term "treatment ecology" to refer to the characteristics of the principle geographic contexts experienced by the client over the course of treatment and the interrelationships among them. These contexts could include the residential neighborhood or living environment, the workplace, and the treatment location (i.e., where the therapeutic components of treatment and ancillary services are delivered). The physical setting and surroundings (e.g., drug availability) of each is of interest, but an ecological perspective is also concerned with the position of the individual within each context (e.g., relative socio-economic status) and interrelationships among contexts. For example, travel burden to and from outpatient care is determined in part by the geography and transit infrastructure linking two contexts: home (or workplace) and the treatment location; and has just begun to receive attention (De Leon 1990, Umbricht-Schneiter, Ginn, Pabst and Bigelow 1994, Friedmann, D'Aunno, Jin and Alexander 2000, Marsh, D'Aunno and Smith 2000, Friedmann, Lemon, Stein, Etheridge and D'Aunno 2001).

Treatment ecology, in terms of Davis and Tunk's (1990-1991) taxonomy of environmental effects on drug use and relapse, focuses on the "physical" environment, which comprises setting, living place, neighborhood, and geography. The "interpersonal", "cultural", or "organizational" environments that comprise the remainder of this taxonomy are also relevant to the extent that they depend on locale, as in the case of media cues delivered to

specific geographic areas, which Davis and Tunk (1990-1991) would situate within the "organizational" environment. In Gifford and Hine's (1990-1991) typology of micro, meso, and macro scales of contextual analysis, treatment ecology takes the meso, or neighborhood level, as its starting point, but accommodates interplay between meso-environments. In the broadest sense, then, the notion of a treatment ecology subsumes the physical, social, and psychological relationships that depend in any way on locale—the relationship between the client and his or her neighborhood, neighborhood and treatment location, neighborhood and society, and so forth—that bear directly or indirectly on progress during treatment, progress following treatment, or on baseline measures recorded at admission that influence treatment outcomes. This characterization is in keeping with traditional use of the term "ecology" in biology (i.e., the environment as it relates to living organisms), with recent use of the term in the field of health ecology (e.g., Stevens 2001), and provides an explicit conceptual framework that can be used to examine these relationships.

An understanding of the relationship between treatment ecology and attrition also has important policy implications for the placement of treatment facilities, client placement when there is a choice among facilities dispersed geographically, and neighborhood improvement efforts aimed at harm-reduction or promoting recovery. The issue of facility siting alone can be approached via a broad range of measures, including relocation and construction of treatment centers, facility improvement, arranging for treatment providers to travel to the client instead of vice versa ("house calls"), providing or improving transportation for clients to the facility, and reducing the number of visits in the treatment regimen. Additionally, when an agency oversees the placement of clients among multiple facilities, as is often the case with drug offenders sentenced to community-based treatment, knowledge of the impact of locale on attrition can help match clients with facilities in order to maximize regional outcomes, but only to the extent that trade-offs between contextual and other factors are well understood. When neighborhoods resist the placement or expansion of a treatment facility, a methodology for evaluating the desirability of each candidate location can facilitate negotiation and compromise.

In Section II conceptual models of attrition are reviewed with attention to their consideration of locale. Section III draws on findings from the broader literature on drug treatment, drug use, neighborhood effects, and environmental psychology to generate specific hypotheses of the influence of treatment ecology on retention. Section IV concludes with a brief discussion.

II The Role of Context in Current Conceptual Models of Attrition

Lack of interest in "external factors" (Stahler, et al. 1993, De Leon, et al. 2000) in retention studies probably stems from the absence of any conceptual framework to generate hypotheses. Moos (1996) motivated attention to larger contexts in substance abuse treatment studies by pointing to a variety of work in other fields, but provided no explanation of the mechanisms that might link anything in the external environment to progress in treatment. *The International Journal of the Addictions*' special issue on environmental factors provides several discussions relating licit and illicit substance abuse and relapse (Davis, et al. 1990-1991, Tucker, et al. 1990-1991, Dielman, Butchart, Shope and Miller 1991, Rosenhow, et al. 1991), but not attrition *per se*. Considering the burden involved in identifying contextual data and geographic computation, particularly when no theories exist to guide the choice of contextual information, it is not surprising that the area remains unstudied and that the few studies that include some contextual exploration have generally done so as an aside to the main analysis (e.g., see the final pages of Iguchi, et al. 1991).

This section reviews the predominant models of treatment process and attrition and their applications, and highlights the extent to which they have addressed geographic setting. In most cases, current theories provide a residual category for external factors or life context, but with little explanation.

II.A Treatment Process Models

A team at Texas Christian University has made a large contribution to developing conceptual models of the determinants of treatment outcomes, beginning with a general framework that seeks to specify the "specific operations, procedures, and conditions" that define the treatment process (Joe, et al. 1994). The authors emphasize events that occur during treatment and the evolution of the treatment regimen over time, but their process model is essentially a typology of variables considered worthy of attention:

(1) Client characteristics at entry that influence the type of treatment provided, including presenting problems, demographics, and "other" client background attributes);
(2) Treatment program characteristics that are relatively stable, including size, location and physical plant, philosophy and goals, operational structure, staff, client composition, and other contributors to ambiance; and
(3) Treatment events: procedures and actions by staff that may vary over the course of treatment, including dosage, take-home privileges, services provided, and drug testing.

Simpson, Joe, Dansereau, and Chatham (1997a) and Simpson, Joe, Rowan-Szal, and Greener (1997b) develop a narrower model that focuses on cognitive explanations of attrition in reaction to what they see as an artificial restriction of attention to behavioral factors, such as drug use, criminal activity, and employment. They theorize and find that client motivation and readiness at admission are important determinants of engagement (i.e., attendance) and drug use during treatment, which are in turn related to program completion. The treatment components considered in these cognitive models are the strength of the therapeutic relationship (or "therapeutic alliance") between client and staff and specific counseling procedures.

Although Joe et al.'s (1994) conceptual framework mentions facility location, no hypotheses are offered. Otherwise, the treatment process models provide no space for consideration of treatment ecology.

II.B Behavioral and Cognitive Change Models

Other models relate attrition and relapse to a user's progress through various stages of cognitive and behavioral readiness for treatment. For example, Prochaska, DiClemente, and Norcross's (1992) and De Leon's (1996) staged models provide the theoretical base for Lang and Belenko's (2000) study of retention among drug offenders diverted to treatment and Joe et al.'s (1998) study of motivation, engagement, and retention. Prochaska et al.'s Transtheoretical Model of Change describes stages of pre-contemplation of seeking help, contemplation, preparation, action, and lastly, maintenance. In De Leon's model, the client moves from denial to ambivalent recognition of a problem, to a desire for change motivated by "extrinsic" factors

(e.g., the criminal justice system, family, or friends), to a state of intrinsic motivation or self-attribution. Next, the individual becomes willing to change and finally, willing to seek change in a formal setting.

Tucker et al.'s (1990-1991) review of relapse models, also considered by Lang and Belenko, finds that "environmental triggers" are a component common in these models. While a natural entry point for attention to contextual factors, this notion of environmental triggers relates to acute events, rather than more lasting circumstances. In any case, Lang and Belenko do not examine environmental factors and in fact adopt a typology that deviates from the conceptual models that motivate their work: (1) "life choice" indicators (substance abuse, sexual behavior, criminal activity), (2) static background factors (demographics, family and social support, history of psychological problems), and (3) dynamic situational influences (employment, problems with a significant other, recent psychiatric problems, past violent victimization—e.g., shooting or stabbing).

II.C Pushes and Pulls

Frustrated with what Stahler et al. (1993) see as prior studies' lack of attention to the question of "why" clients drop out, Stahler et al. conduct an ethnographic study of homeless male crack users in a large northeastern city and conclude that in most cases, clients are either pushed away by a "dominant, precipitating force" within the program or "pulled" away by an outside force. They develop a typology of explanations that includes the following: (1) the treatment/shelter milieu (e.g., lack of privacy, a drug-ridden environment, or an atmosphere of violence); (2) the client's personality, propensity for treatment, and "fit" with the treatment program; and (3) "external" forces, such as a job, school, a significant other, business with a criminal justice problem elsewhere, or other affairs to which the client feels compelled to attend.

Stahler et al.'s findings regarding the treatment or shelter environment are useful from the perspective of treatment ecology, since they relate to conditions of specific geographic contexts. Their notion of "external" forces could be expanded to include factors arising from geography or relationships between settings, though this was not their original intent.

II.D Other Typologies

Unlike the studies outlined above, the typologies used by the three studies below to identify factors related to attrition are not based on an explicit theoretical framework.

De Leon et al. (2000) considers "external" factors, as in Stahler et al. (1993), but as one part of a three-way classification that is only briefly introduced. In their study of an intervention to improve baseline motivation and retention by exposing residential treatment clients to senior staff early on, retention is viewed as a function of external influences, client characteristics, and the treatment regimen. However, the external influences category is left undefined, making it unclear what factors might belong there.

Rooney and Hanson's (2001) review is noteworthy for its silence on the issue of context, despite inclusion of a broad range of studies that examine attrition in the treatment of substance abuse, domestic abuse, and other problems. Their review does identify two correlates of attrition potentially related to geographic factors: life-style instability, characterized by frequent moves, unreliable employment, and excessive leisure activities; and the waiting time to first treatment session.

Finally, Moos et al. (1990) present a categorization that emphasizes life context more than the models above. However, their focus is treatment for alcohol, rather than illicit substance abuse. Their view of life context includes "life stressors" and social support. Retention is also thought to be influenced by the treatment environment, although geographic context is not discussed. Moos et al.'s typology could accommodate this by elaborating on the notion of life stressors and by broadening the conceptualization of the treatment environment.

In summary, a number of catchall categories—"other client background factors" (Joe, et al. 1994); "environmental triggers" (Tucker, et al. 1990-1991); "external factors" (Tucker, et al. 1990-1991, De Leon, et al. 2000); and "life context" (Moos 1990) have been identified which could serve as a base for consideration of contextual factors within the framework of existing models. However, these models almost never discuss the issue explicitly. Exceptions are Joe et al.'s (1994) passing mention of facility location and Iguchi and Stitzer's (1991) more serious attempt to assess the relationship between urinalysis results and contextual factors, including drug arrests, though neither examines retention. Still, many of the factors highlighted in this review might function as mediators between ecological attributes and retention. For example,

clients living in areas where employment opportunities are scarce might be more likely to exhibit Rooney and Hanson's (2001) "lifestyle instability"; in regions with less treatment capacity, but with a demand for services comparable to that of other areas, Rooney and Hanson's (2001) waiting time to first treatment could be longer; and when clients or treatment centers are situated where poverty and police harassment are severe, Moos et al.'s (1990) life stressors on treatment clients might be more severe as well.

III Theories of Treatment Ecology's Influence on Attrition

Many of the conceptual models described above view motivation, readiness, and engagement as central to retention and other treatment outcomes (Prochaska, et al. 1992, Stahler, et al. 1993, De Leon 1996, Simpson, et al. 1997b, Joe, et al. 1998), and some empirical results have been offered in support (Simpson, et al. 1997b, Joe, et al. 1998). Below, mechanisms by which residential and treatment neighborhood could influence motivation, readiness, and engagement are developed. Focusing on these contexts and their inter-relationships narrows the discussion, but allows a more detailed elaboration of causal mechanisms. Further, the discussion deals with outpatient treatment, the most general case from a treatment ecology perspective: In outpatient care, the residence and treatment contexts are distinct, so that notions of travel burden and the interaction between contexts during episodes of treatment can be explored.

III.A Residence Context

Studies of neighborhoods and substance abuse, the effects of segregation on individual outcomes, environmental psychology, and the literature on treatment outcomes provide guidance on constructing the theories outlined below. Work in these areas suggests that retention in treatment could be influenced by four aspects of the residence context: drug availability, neighborhood disadvantage, availability of community resources useful during treatment, and "restorative" qualities of the locale.

III.A.1 Drug Availability

Perhaps the most obvious among these is local availability of drugs and visibility of drug use. Because auditory or visual stimuli associated with the expectation of consumption often precede withdrawal stress and relapse (Rosenhow, et al. 1991), the visibility of drug

market transactions, users, or other reminders of use may serve as environmental cues that influence retention through the client's urge to use or actual use, which Simpson et al. (1997b) found to be inversely related to treatment completion.

Clients who face such environmental triggers more regularly may also resort to special techniques to subdue the urge to use. One technique often employed to help assure follow-through with intended behavior is an "implementation intention", in which a person explicitly lays out how he or she will attain their goal by responding to a given situation. For example, a client aiming to avoid relapse might decide, "If I ever come across my old dealer by accident, the first thing I'll do will be to turn around and walk away", or "At 5pm every night, the time I used to start thinking about getting high, I'll go to the gym instead." Unfortunately the literature on implementation intentions indicates that for drug addicts, using them may impede completion of other everyday tasks, like making it to lunch on time (for an introduction to this literature, see Heckhausen and Beckmann 1990, Gollwitzer and Brandstaetter 1997, Gollwitzer 1999, Sheeran and Orbell 2000, Brandstaetter, Lengfelder and Gollwitzer 2001, Koestner, Lekes, Powers and Chicoine 2002, Einstein, McDaniel, Williford, Pagan and Dismukes 2003, Armitage 2004). Thus, one could hypothesize that clients in high drug availability neighborhoods may have to resort to such psychological techniques more often, and if so this would come at some cost to daily functioning, and perhaps lead to a higher likelihood of dropout.

Local availability can be characterized in terms by local prices and search times. In a rational model of addiction (e.g., Becker and Murphy 1988, Becker, Grossman and Murphy 1994), increased availability reduces the cost of and increases the likelihood of use. Caulkins (1995) demonstrated regional differences in illicit drug prices, rising with distance from the source of distribution; and search times also vary (Rocheleau and Boyum 1994). Moreover, drug users or potential users do react to changes in price: Caulkins, Rydell, Schwabe, and Chiesa (1997, p. 84) summarized findings suggesting that the price elasticity of demand for cocaine is substantial, probably on the order of –1.0, so that clients in neighborhoods where prices are lower should be expected to use more. On the other hand, since elasticity estimates include the consumption decisions of new initiates, current users not in treatment, and treatment clients, the figure may not accurately reflect the purchasing behavior of treatment clients alone.

The nature of the local drug market, particularly the relative residential locations of dealers and customers, has been hypothesized to affect market-related violence. If true, the type of local market could also affect attrition indirectly via violence and victimization, discussed below (Reuter and MacCoun 1992).

Third, neighborhood differences could affect *involuntary* attrition through differences in visibility of markets, drug control tactics, or police pressure if these lead to neighborhood variation in the likelihood of arrest given an episode of purchase or use. In this case, a treatment client who lives and uses in a neighborhood with a higher likelihood of drug arrest will be more likely to be arrested and involuntarily removed from treatment than a client who uses in a "safer" home neighborhood.

III.A.2 Neighborhood Disadvantage

Geographically concentrated poverty and disadvantage may also impact attrition. A small number of recent studies have shown that neighborhood disadvantage is related to individual drug use (Lillie-Blanton, Anthony and Schuster 1993, Boardman, Finch, Ellison, Williams and Jackson 2001) and there is some evidence that this relationship has often been masked by confounding with race and ethnicity (Iguchi, et al. 1991, Lillie-Blanton, et al. 1993). A review by Davis and Tunks (1990-1991, p. 812) concluded that for relapse, "cultural differences per se are less decisive than microenvironmental factors, availability, peer use, and other interpersonal factors. Differences seen between cultures may in fact be mediated by variation in the environment." It is Boardman et al. (2001) and Ennett, Flewelling, Lindrooth, and Norton (1997), however, who offer the most detailed hypotheses of how disadvantage might encourage drug use and many of these suggest that similar mechanisms operating at the neighborhood level could exert an influence on motivation and readiness for treatment as well as engagement.

Briefly, Boardman et al. and Ennett et al. together present a four-part argument. Boardman et al. argues first that conditions in poor areas produce a higher frequency of acute stressful events in the lives of residents due to less reliable employment, higher mortality, and violent and traumatizing incidents, which increase individual "life stress." Similarly, more burdensome living conditions and on-going social stigmatization based on neighborhood membership contribute to chronic general strain; and these factors as well as ambient risks such as crime and violence place residents under a higher level of psychological stress. Drug

use becomes more attractive as an alleviator of stress and strain and as a means of escape from a harsh reality. Second, Boardman et al. suggest that because family members and social networks are more stressed, individuals may receive less social support. Third, Boardman et al. cite evidence that individuals in disadvantaged areas express lower levels of self-efficacy. Finally, Ennett et al. describe a lack of community resources in poverty-stricken neighborhoods that depletes social organization and reduces their ability to provide local sanctions for drug use. Lack of local sanctions and other "cultural and social" differences in community tolerance for drug use yield cross-neighborhood differences because they influence an individual resident's willingness to engage in use.

More acute life stressors and general strain in disadvantaged neighborhoods may also draw from a treatment client's ability or willingness to engage treatment regularly. Several studies have linked psychological stress and depression with worse retention (Anglin, et al. 1990, Brown, et al. 1998, Hiller, et al. 1999). If stress contributes to propensity to use, it may also contribute to propensity for relapse, which Simpson et al. (1997b) link to attrition. Taylor (1986) cites evidence that clients who experience a major stressful event within the first 90 days after alcohol treatment were more likely to resume use. Many have pointed to O'Brian, Nace, Mintz, Meyers, and Ream's (1980) finding of low rates of relapse among United States soldiers returning home from the Vietnam War as further evidence that removal of a major stressor can improve the odds of recovery (Davis, et al. 1990-1991, Tucker, et al. 1990-1991).

Second, lower life efficacy among clients who live in or are from disadvantaged areas may reduce their expectation of success in treatment and thus readiness and motivation, especially according to the cognitive models of Prochaska et al. (1992) and De Leon (1996). In their study of probationers in residential treatment, Hiller et al. (1999) found that lower self-efficacy predicted early dropout. Similarly, because residents of poor, racially and ethnically segregated areas are more likely to experience worse life outcomes (e.g., earnings, education) generally (Massey 1990, Borjas 1995, Cutler and Glaeser 1997), clients from these areas may have less hope for the future, incentive to recover, and thus less motivation.

Lack of nearby employment opportunities in disadvantaged areas, in particular, should be expected to reduce retention since stable employment is well established as a predictor of dropout (Anglin, et al. 1990, Lang, et al. 2000, Veach, et al. 2000). Clients who lose their jobs during treatment or unemployed clients in such neighborhoods will have more difficulty

finding work. If local jobs are unreliable, then the result may be higher "life instability" for treatment clients, which Rooney and Hanson (2001) found to be related to attrition as well.

Third, Boardman et al.'s (2001) hypothesis that residents of disadvantaged neighborhoods have less social support because their family and friends also experience high levels of life stress implies lower retention, since social support is another well established predictor of retention (Anglin, et al. 1990, Lang, et al. 2000).

Finally, lack of sanctioning, tolerance of drug use, and geographically varied differences in beliefs about the cost or harms of drug use may influence the client's valuation of drug treatment and thus readiness and motivation. Such differences may also influence a client's belief about "how much" treatment is necessary, which Stahler et al. (1993) found to be a motivator of dropout in some of the crack cocaine users they interviewed.

III.A.3 Community Resources

Proximity to self-help and mutual support groups such as Alcoholics Anonymous and Narcotics Anonymous, not unexpectedly, has been shown to be an important predictor of enrollment in them (Mankowski, Humphreys and Moos 2001). Because participation in these groups has also been linked to improved outcomes in other formal treatment programs (Hawkens, Catalano and Miller 1992), treatment clients in neighborhoods where they are less available may be more at risk of dropout.

Additionally, the availability of the more typical economic, social, and institutional resources that ease the burden of daily life may influence retention in four ways. A dearth of grocery stores and other retail establishments, financial institutions, and health care facilities, for example, that marks many destitute areas may increase the time cost of accessing these services and conducting daily affairs, which increases the opportunity cost of diverting time and energy to treatment. A paucity of establishments that facilitate everyday tasks could also increase what Tucker et al. (1990-1991) refer to as "daily hassles", citing two studies that showed that daily hassles are related to psychological symptoms associated with relapse, more so than acute life events. Third, a lack of recreational establishments and social venues—of the magnitude that inspired the Magic Johnson theatres in Los Angeles—deprives clients of stress-coping mechanisms, and fourthly, reduces the availability of alternative activities to fill the often considerable amount of time spent on consumption and

consumption-related activities prior to treatment (Tucker, et al. 1990-1991); clearly, employment opportunities also present a similar substitute.

III.A.4 Restorative Qualities

Finally, work in the field of environmental psychology suggests that a particular location or setting can influence an individual's mental state and behavior in three ways that may influence treatment motivation or engagement. First, Ulrich, Simons, Losito, Fiorito, Miles, and Zelson (1991) present findings that less urban settings have "restorative qualities" that facilitate recovery from stressful events. Second, according to Apter's (1982) "reversal theory", features of a location can influence an individual's positive affect and anxiety, and produce a shift in mood from a "paratelic" or spontaneous state, concerned primarily with leisure, to a "telic" or more "goal-oriented" state (Apter 1982, Kerr and Tacon 1999). Third, in an investigation of the perceived meaning of place, Gustafson (2001) found that subjects typically considered a given locale as a source of self-identity and a way of distinguishing themselves from others, a sense of life path or a symbolic environment where important or traumatic events had happened, or as offering opportunities and constraints. Gustafson's findings lead naturally to Canter's (1977) "self-fulfilling prophecy" model, in which individual behavior is said to result merely from a desire to conform with expectations of behavior associated with a particular environment or place. Thus, living in an area where substance misuse is expected or perceived as typical could increase the odds of relapse and attrition.

III.B Treatment Context

The treatment context is experienced only temporarily and in the outpatient setting only in passing. Whereas several of the mechanisms outlined above are only plausible when the client has had a long-term, continued exposure to an area, any hypotheses linking the treatment context with attrition must have more viability in the short-term. For example, simply traveling to a new locale cannot be expected to instill in the client new beliefs about drug use or health behavior shared by local residents.

III.B.1 Exposure to the Treatment Neighborhood

On the other hand, to the extent that clients find themselves in the neighborhood of the facility, they are more exposed to that neighborhood's stressors and environmental cues.

Gifford and Hine (1991) relate the extreme case shared by Suedfeld (1979) of an alcohol treatment center with a high dropout rate situated just upstairs from a cocktail lounge that often diverted clients on their way in. But, the treatment site's surroundings can just as well support retention. For example, frequent trips to the treatment location improve proximity to that area's support groups and other social, economic, and retail establishments, potentially augmenting opportunities found in the client's own workplace, residence, or other life contexts.

Further, any "restorative" or "telic" qualities of the physical setting are perhaps even more relevant at the treatment site than the client's neighborhood. Apter (1982) would envision a neighborhood and treatment facility architecture specifically designed to inspire a goal-orientation, but one can just as easily imagine the difference in motivation between traveling to any kind of desirable, as opposed to undesirable area on a regular basis; although there may be considerable variation in what constitutes desirability across clients.

III.B.2 Travel Burden and the Opportunity Cost of Attendance

More straightforward is the issue of travel burden, which is a function of the relationship between the treatment and residence context, not only in terms of distance, but the transit infrastructure linking the two. Accessibility by bus, rail, or other public transit is relevant or instead road traffic, for clients who drive. Because drug treatment clients are often thought to be more disorganized, to have more difficulty planning, and often without reliable access to private transportation (Friedmann, et al. 2000), recent studies have examined how utilization varies when travel burden is reduced. These studies have found that treatment providers that offer transportation assistance generally have higher utilization (Umbricht-Schneiter, et al. 1994, Friedmann, et al. 2000, Friedmann, et al. 2001) and that utilization of ancillary services improves when they are offered onsite (Umbricht-Schneiter, et al. 1994, Friedmann, et al. 2000, Marsh, et al. 2000, Friedmann, et al. 2001). In a recent study examining outpatient treatment of alcohol and illicit drug users at publicly-funded programs in Baltimore City, Beardsley et al. (2003) found an inverse association between distance-to-facility and treatment completion, as well as length-of-stay. Mankowski et al.'s (2001) findings that proximity to AA, NA, and similar groups increases a client's likelihood of participation after completing a treatment program confirms that proximity to programs can be important. Geographic separation of clients' residences and treatment centers can also be viewed from the perspective and with the methods of the "spatial mismatch hypothesis" literature, which examines proximity of jobs to

job-seekers (Holzer 1987, Jencks and Mayer 1990, Wheeler 1990, Moss and Tilly 1991, Ihlanfeldt 1992, Kain 1992). Interestingly, simply possessing a valid driver's license is among the strongest single predictors of use during treatment among all measures contained in the Addiction Severity Index (ASI), according to an examination by Morral, Iguchi, Belding, and Lamb (1997). Although, this relationship is probably influenced by more than travel burden because a valid driver's license may reflect stability and general life management skills as well as legal status and ability to drive.

From the treatment ecology perspective, it is useful to consider how the full opportunity cost of travel depends on context. While time and money resources used in transit are a function of distance, mode of travel, and transit infrastructure, opportunity cost also depends on hours of work foregone (or rescheduled) and associated wages, leisure time, and the like. These costs may be offset by contextual factors at the treatment location if a trip there is worthwhile for other reasons such as employment, shopping, or community resources that allow one to perform other life tasks.

III.B.3 Face-Saving Excuses

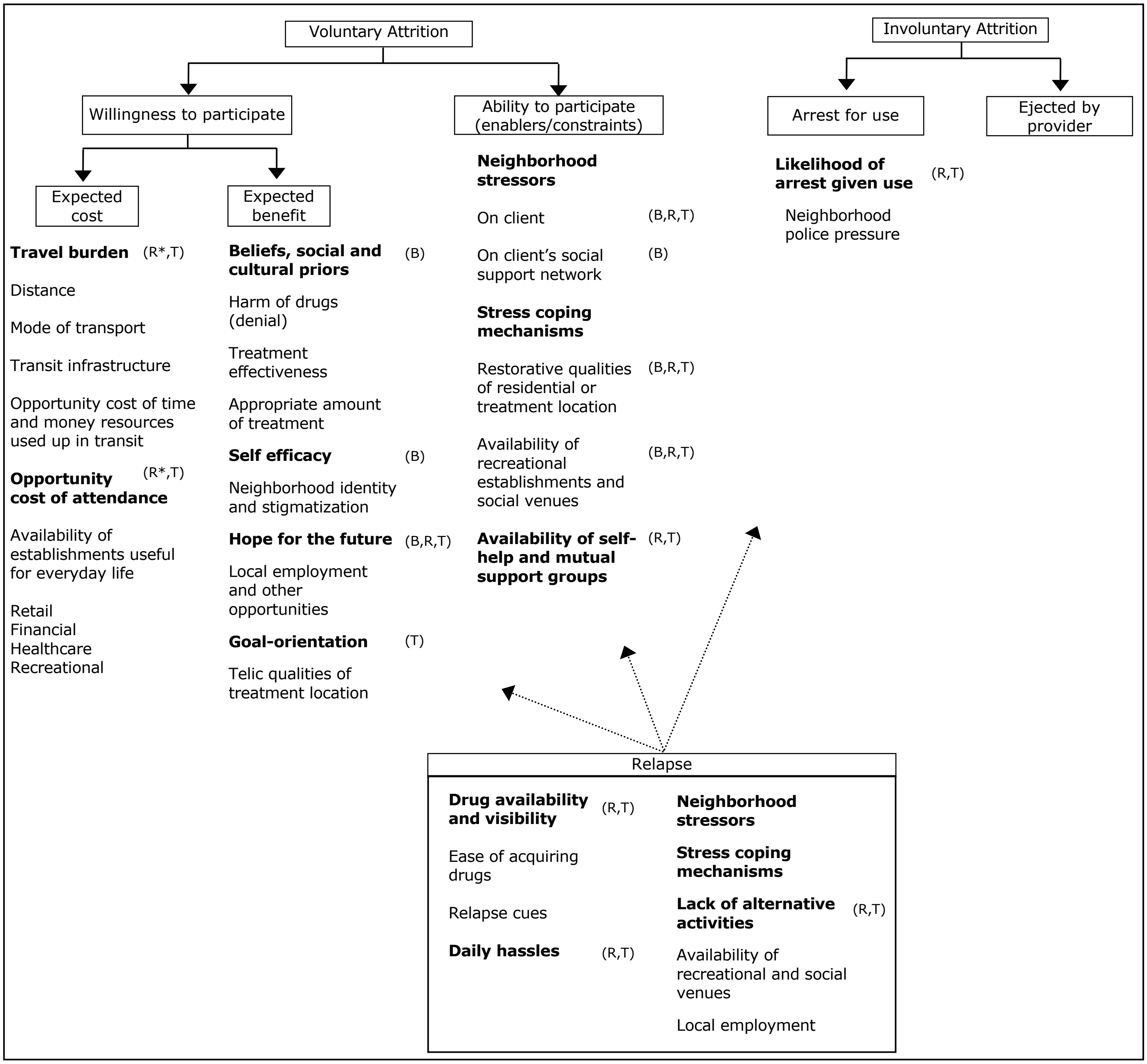
Finally, travel burden, stressors, and any other negative aspects of the treatment neighborhood may also provide an easy excuse for clients who for other reasons find themselves inclined to skip a treatment session or quit treatment entirely. A client can simply say to friends and loved ones, for example, "it was just too much of a hassle with all the traffic" or "it was such a bad neighborhood that I just didn't feel comfortable going there three times a week for counseling", and with that justify a decision to drop out that may in truth have had more to do with a more general lack of interest or motivation. Social psychologists have studied excuse-giving behavior extensively (see Weiner, Amirkhan, Folkes and Verette 1987, de Jong, Koomen and Mellenbergh 1988, Snyder and Higgins 1988, Shafir, Simonson and Tversky 1993, Shaw, Wild and Colquitt 2003).

III.C A Framework for Classifying Treatment Ecology Effects on Attrition

A broad range of theories has been presented in this discussion, from the effect of neighborhood residence on self-efficacy and motivation to the effect of neighborhood police pressure on arrest and involuntary dropout for clients who use during treatment.

Figure 1-1 summarizes the range of neighborhood-level effects on attrition. The framework is based loosely on the behavioral choice model preferred by Tucker et al. (1990-1991) to explain relapse, as opposed to stress-coping and motivational-conditioning models. Tucker et al.'s choice model is concerned with the choice of consumption activities versus the set of alternative activities available; it provides guidance in "identifying dimensions of the environment that bear functional relations with substance use" and is consistent with findings that drug use varies inversely with accessibility to alternative activities. The focus of the present model is attrition, however, which is a separate issue from relapse and requires its own consideration of the choice mechanisms at work. In Figure 1-1, attrition is either voluntary or involuntary (i.e., imposed by the treatment program, criminal justice system, or some other external agent). The model views a voluntary decision to limit engagement or cease treatment as a function of (1) willingness to participate, which subsumes motivation (Prochaska, et al. 1992, De Leon 1996, Joe, et al. 1998), and (2) factors that do not enter into a client's decision-making directly, but instead mediate the decision by enabling or constraining the client's ability to participate or maintain a path to recovery. Willingness to participate in turn depends on the client's assessment of expected benefit of treatment and cost of attendance. Cost is conceived of broadly to include the direct time and money costs of participation as well as the opportunity cost of activities and purchases foregone. Not shown in Figure 1-1 are determinants of attrition unrelated to treatment ecology.

Figure 1-1 Theories of the effect of residence and treatment contexts on attrition from outpatient treatment



* Assuming client travels from residential to treatment location (e.g., rather than from workplace)

B = Presumed to influence baseline motivation at admission

R = Presumed to influence retention immediately upon a change in residential location

T = Presumed to influence retention immediately upon a change in treatment location

Note that the framework not equate relapse with attrition. Instead, relapse is expected to influence attrition indirectly through changes in the client's assessment of success in treatment, self-efficacy, and beliefs about the effectiveness of treatment; by increasing the hardship of faithful adherence to the treatment program; and by raising the likelihood of detection by law enforcement, arrest, and involuntary removal from the program.

Lag time of the effects should be expected to vary, as discussed at the beginning of Section 3. A residential move away from a disadvantaged neighborhood, for instance, may not immediately, nor ever, remove the old neighborhood's impact on self-efficacy or the client's belief in his or her ability to benefit from treatment. On the other hand, effects that depend more on proximity to physical resources (e.g., employment opportunities, retail establishments, improved routes to the treatment facility) should be more sensitive to a change in location and their time course may be more a matter of the time required to learn to take advantage of what the new setting has to offer. As a first step toward characterizing the likely time course of each ecological effect, Figure 1-1 denotes those effects hypothesized to influence baseline motivation at the time of admission (B); and effects for which a change in residential location (R) or treatment location (T) is hypothesized to influence retention in the short-term. For example, while travel burden's influence on retention might change immediately upon a change in either residential (R) or treatment (T) location, travel burden should not be expected to affect motivation prior to enrollment in a treatment program (B), unless the user had a particular facility in mind beforehand. Neighborhood stressors in the client's residential neighborhood could impact baseline motivation (B), and with a residential or treatment location move, the change in neighborhood stressors could impact engagement and dropout quickly (R, T).

IV Concluding Remarks

The purpose of this review has been to identify the range of causal mechanisms suggested by the current literature that would link context and geography with attrition in order to guide future empirical work. The hypotheses outlined here have several location-oriented policy implications. Evidence of a link between neighborhood disadvantage and attrition, for example, would build on findings that link neighborhood conditions with drug use and would imply that targeted investments in such neighborhoods could both reduce rates of initiation and improve rates of recovery. Significant findings for travel burden would point to

improvements in transportation assistance for treatment clients and relocation of treatment facilities with attention to the geographic distribution of clients. If availability of community resources and employment opportunities improve retention, then providing incentives to establishments that offer those resources to locate or relocate will become more attractive policies as well. Upon verification, all contextual effects are relevant to informing not in my backyard (NIMBY) disputes over the location of treatment centers.

However, empirical backing is required to test these hypotheses and future analysis appears justified in light of previous findings. Informing any such policy or debate requires an assessment of neighborhood conditions faced by actual clients to determine whether any of the hypotheses are relevant, and estimation of the relative contribution of contextual effects outlined here compared with other determinants of attrition, including program components and approach, whose variation is considerable. Furthermore, trade-offs between the various contextual influences should be expected: for example, identifying the optimal treatment location for clients living in disadvantaged neighborhoods where availability of various establishments is low could involve a trade-off between the cost of travel and exposure to a more desirable, but distant treatment location where resources are more available.

More generally, the notion of a "treatment ecology", forwarded here within a behavioral framework that distinguishes voluntary from involuntary discharge decisions, provides an analogy to motivate consideration of multiple contexts and their interactions, but is a working definition that will require refinement as findings accumulate.

Knowledge of involuntary dropout, even apart from contextual considerations, remains poor and data systems such as the California Alcohol and Drug Data System could do much to improve this state of affairs by expanding their classifications of reason for discharge.

Finally, the relationship between distance-to-facility and dropout is likely to be nonlinear; coupled with trade-offs like the one alluded to above, this suggests exploration of non-linear estimation models and attention to distributional extremes as well as averages. Related to estimation is the question of measurement of environmental conditions: Tucker et al. (1990-1991) argue convincingly that client self reports of their physical surroundings are suspect for a variety of reasons and objective assessment is preferable. Thus, for example, combining an area survey on drug use with area arrest data would provide a more desirable estimate of the likelihood of arrest given use than a treatment client's perception of the same.

Chapter 2

The Physical Environments Where Addicts in Recovery Live and Receive Treatment, Los Angeles 1998-2000

I Introduction

Little is known of the physical, social, and economic environments in which drug addicts in public treatment programs live or receive treatment. To date, research efforts to characterize the treatment population and identify factors linked with successful recovery have been limited to the level of the individual and the treatment program. Specifically, previous studies have examined drug choice and addiction severity, motivation and readiness for treatment, living situation, employment, and medical and psychiatric illness (Joe, et al. 1998, Hiller, et al. 1999, Lang, et al. 2000, Maglione, et al. 2000, Sia, et al. 2000, Veach, et al. 2000), treatment setting (e.g., inpatient vs. outpatient), approach, and process components such as counseling and early exposure to senior staff (Simpson, et al. 1997, Simpson, et al. 1997a, Simpson, et al. 1997b, Moos, Moos and Andrassy 1999, De Leon, et al. 2000), and ancillary services, such as job training, medical and psychiatric care, and transportation assistance (McLellan, Arndt, Metzger, Woody and O'Brian 1993, McLellan, et al. 1994, Umbricht-Schneiter, et al. 1994, Friedmann, et al. 2000, Marsh, et al. 2000, Friedmann, et al. 2001). While a number of ethnographic studies and personal accounts describe conditions in purposively chosen neighborhoods, especially in the inner city, or at specific treatment programs (e.g., Johnson, Williams, Dei and Sanabria 1990, Stahler, et al. 1993, Shavelson 2001), population-level estimates providing a more complete view of environments facing the population of addicts in treatment are lacking.

The omission is surprising, given that drug use is often viewed as a societal pathology with its roots in lack of alternative opportunities, dysfunctional or oppressive social environments, peer use, and absence of community-level sanctioning of drug activity, all of which depend in part on local conditions (Agnew and Duncan 1989, Massey 1990, Lillie-Blanton, et al. 1993, Ennett, et al. 1997, Wallace and Wallace 1998, Boardman, et al. 2001, Dreier, Mollenkopf and Swanstrom 2001, Saxe, et al. 2001). It is reasonable to ask whether these conditions do in fact correctly characterize neighborhoods where addicts in treatment are found, and whether these conditions may interfere with recovery. Among treatment practitioners, a broadly held view is that lasting recovery for most addicts requires not only an effort to curb consumption, but a transformation in lifestyle and worldview as well. For example, treatment programs urge addicts to abandon deviant behaviors, rid themselves of harmful associations and peers, become economically productive, and generally enter the

societal mainstream in terms of their behavior, thoughts, and life goals. The widely applied 12-Step method implores users to take a "moral inventory", to be willing to remove defects and shortcomings from their character, and to simultaneously strive toward self-betterment and pursue a useful, positive, and socially acceptable life course (McGee 2000, Humphreys 2003). Particularly in residential programs, the job search is a formal stage and even a requirement of the treatment regimen. Then for many users, recovery involves abandonment of many aspects of the individual's the way of life, contemplation of the future, and an attempt to identify more promising, alternative life prospects. It is important to understand the environments individuals face as they attempt this transition.

Previous research has identified an association between neighborhood-level factors and risky health behaviors (Diez-Roux, et al. 1997, Scribner, Cohen and Farley 1998, Duncan and Raudenbush 1999, Cohen, et al. 2000), including drug use (Lillie-Blanton, et al. 1993, Ennett, et al. 1997, Boardman, et al. 2001). From the perspective of the treatment community, it is important to know whether the treatment population faces similar conditions and whether these conditions influence clients' commitment to recovery and likelihood of relapse. From the perspective of a disease model of addiction, and contagion or epidemic models (Rowe and Rodgers 1991, Acker 1993, Lyvers 1998), which by analogy suggest an epidemiological approach, an important task toward understanding the epidemic is to identify environmental conditions that are disproportionately prevalent among affected individuals in each stage of the disease process, from initiation to recovery.

Recent advances in geographic information systems (GIS) technology, the widespread availability of GIS software, georeferenced contextual data sets, and georeferenced treatment reporting systems have placed the contextual analysis required for this task well within the reach of the research community.

Chapter 1 reviewed the literature on drug use, treatment outcomes, neighborhood effects on health and behavior, and environmental psychology to develop hypotheses relating environmental factors to drug use, relapse, and treatment completion. But this review provided no indication of the degree to which addicts in treatment are exposed the environmental factors it identified. In this paper, we select four environmental factors highlighted in Chapter 1 based on data availability. We then develop estimates of the prevalence of the level of these conditions for all treatment clients discharged from county-

contracted programs in Los Angeles County (LAC) in the period 1998-2000. We focus our attention on the client's residential location and the treatment site.[2] Following a comparison of the conditions at these locations to those in neighborhoods of the LAC household population at large, we contrast clients' residential and treatment locations, in terms of their separation distance and environmental conditions. Our specific research questions and choice of contextual measures are discussed in Sections II and III. However, it is important to note that the aim of this chapter is to produce population-level estimates of clients' environmental—henceforward, *neighborhood*—conditions in order to set an agenda for future research. We do not evaluate treatment outcomes here. In Section IV, we describe the client data, contextual data, and special considerations related to geographic computation. Section V details our methods. Findings are presented in Section VI.

II Selection of Contextual Measures

The four neighborhood factors we examine are drug availability, social stressors, proximity to jobs, and proximity to retail services. Below, we summarize the rationale from Chapter 1 suggesting that these factors are relevant to treatment outcomes. Section IV.B explains how these constructs are operationalized.

Areas with higher *drug availability*, particularly visible drug markets and use, are hypothesized to provide more of the environmental cues and visible and auditory stimuli shown to be associated with relapse (Rosenhow, et al. 1991), which is turn linked to completion of treatment (Simpson, et al. 1997b). From the perspective of an economic model of addiction (e.g., Becker, et al. 1988, Becker, et al. 1994), higher availability reduces the search cost and price of consumption, and thus increases the expected demand by the treatment client. Price and search costs have been shown to vary geographically (Rocheleau, et al. 1994, Caulkins 1995) and Caulkins et al. (1997, p. 84) summarizes findings that indicate that demand does in fact respond to price. Additionally, if police focus their efforts on high availability areas, then clients who live or receive treatment in these areas may have a higher likelihood of arrest given relapse, and thus a higher likelihood of removal from treatment.

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2 For clients who are employed, the area surrounding the location of employment is a third geographic context that ought to be examined, but we do not examine it here.

Social stressors we examine are neighborhood disadvantage and violence and victimization. Disadvantage, generally a composite measure that taps multiple dimensions of poverty, has been associated with drug use (Lillie-Blanton, et al. 1993, Boardman, et al. 2001). Boardman et al. (2001) motivate the relationship as follows. Residents of disadvantaged areas suffer higher levels of psychological stress due to "life stress" (less reliable employment, higher mortality, and violent traumatizing incidents), "general strain" from harsher living conditions and social stigmatization from living in high-poverty areas, and ambient risks such as crime and violence. Residents of disadvantaged areas turn to drugs in part to alleviate psychological stress and as a means of escape. Psychological stress, in turn, is associated with worse retention in treatment (Anglin, et al. 1990, Brown, et al. 1998, Hiller, et al. 1999). A separate relationship is presented in Ennett et al. (1997). They argue from a social disorganization perspective that disadvantaged areas are less able to deter deviant and delinquent behaviors due to their economic deprivation, neighborhood disorganization, higher race of transitions and mobility, and lower levels of neighborhood attachment. They cite empirical evidence that these factors are associated with adolescent substance use. Finally, individuals in disadvantaged areas have worse expected life outcomes (e.g., earnings and education) (Massey 1990, Borjas 1995, Cutler, et al. 1997), which may reduce their self-efficacy and expectation of success in treatment. Hiller et al. (1999) found that lower self-efficacy predicted dropout. Boardman et al.'s theory rests on several underlying stressors that are correlated with, but do not always go hand in hand with disadvantage, such as local crime and violence. In this chapter, we draw on data that allows us to distinguish between disadvantage and local rates of violence and victimization.

We examine *proximity to jobs* because employment is a well-established predictor of treatment completion (Anglin, et al. 1990, Lang, et al. 2000, Veach, et al. 2000) Proximity to jobs may improve a client's chance of finding employment both during the course of treatment and afterward, and may improve employment stability over time, which has been linked with better retention outcomes in treatment programs for substance abuse, domestic abuse, and other problems (Rooney, et al. 2001).

Finally, *proximity to retail services* is used as a neighborhood-level indicator of what Tucker (1990-1991) refers to as "daily hassles", or factors that make the tasks of everyday living, such as shopping and transportation, more burdensome. Tucker suggested daily hassles might influence treatment outcomes, noting previous research that linked client self-reports of hassles

to relapse. At the neighborhood level, the idea is that lower availability of establishments required to fulfill basic life tasks increases daily hassles for individuals in the area. Beyond the link with relapse, Chapter 1 argues that daily hassles are a tax on clients' time budget that may make them less willing or able to continue to attend treatment sessions.

III Research Questions

This initial, exploratory analysis of the neighborhood conditions facing treatment clients is guided by the following research questions:

RQ1. To what extent do clients in recovery programs for illicit substance abuse live in or attend treatment in neighborhoods that might impede, or facilitate success in treatment, based on the hypotheses raised in Chapter 1?

RQ2. Are there client subpopulations that experience these conditions at higher rates? Specifically, do groups of clients typically reported to have the worst outcomes (e.g., unemployed, dual diagnosis, heavy users) also face the worst environmental conditions?

RQ3. Does the treatment site expose clients to better or worse environmental conditions than they live in? How common is it for clients who live in "better" neighborhoods to travel to "worse" neighborhoods for treatment and vice versa?

IV Data

IV.A Sample of Treatment Discharges

We combine individual level data on substance abuse treatment episodes in Los Angeles County with contextual measures of conditions in the treatment client's residential neighborhood and the neighborhood of the treatment facility. The individual level data are from the county Alcohol and Drug Program Administration's (ADPA) Los Angeles County Participant Reporting System (LACPRS) discharge datasets, fiscal years 1998-1999 and 1999-2000, which contain one record for each discharge during the fiscal year from alcohol and drug

treatment programs that contract with ADPA. These data are collected in interviews with the client at discharge, using a standardized instrument similar to the one used by the California Alcohol and Drug Data System. Interviews are conducted by treatment counselors, whom receive annual training from ADPA. Information on the client's residence is limited to homeless status and zip code if not homeless. Each record is accompanied by a treatment facility identifier that can be linked to a facility address list. All data are by self-report. For example, counselors do not administer drug tests to verify claims of abstinence or use.

Only the first observed discharge for each client during the study period is included in the analysis. This allows us to generalize findings to the population of clients rather than discharges. Of the 45144 clients in the data, 11488 (25.4%) logged two or more discharges in the period 1998-2000. Of these, 273 (0.6% of all clients) logged multiple admissions on the same day, most likely the result of a data entry error since the duplicated admissions differed in other respects.[3] Observations for these clients were excluded.

We restrict our analysis to clients in community-based recovery programs for use of an *illicit* drug. This excludes clients who did not mention heroin, cocaine, crack, marijuana, PCP, methamphetamines, ecstacy, LSD, or non-prescription methadone among their primary, secondary, or tertiary drug problems (N=5711, 12.7% of remaining clients).[4] We also exclude incarcerated clients (2.2%), clients in detoxification as opposed to long-term recovery (20.0%), or in residential programs with an intended stay of less than 30 days (0.3%). Cases that can not be included in analysis of residential context include homeless clients with unknown residential zip code (26.0%), other clients whose zip code was not reported (0.3%) or whose zip code is outside of the LA County study area (3.2%)

Additional restrictions are applied to the set of zip codes representing the clients' residential or treatment locations. Three of the contextual measures described in the next section—homicide rate, drug-induced death rate, and drug treatment episode rate—are per capita rates of rare events. To avoid outliers resulting from inadequate precision, we exclude zip codes with a census population of fewer than 5000 residents. Other contextual measures measured as rates per household or family, so zip codes with fewer than 25 households or

[3] For example, two admissions for the same person on the same date to the same treatment facility, for the same modality of care were found with different dates of discharge or a different discharge status.

[4] Ecstacy and LSD are coded as "other" on the LACPRS reporting form.

families are dropped as well.[5] One zip code (91310, Castaic, California) is missing from the zip code shape file required to construct the zip code level measures, and another (90704, Catalina Island) is dropped from the analysis because it is far from the rest of the county. Finally, nine of the 217 facilities in the data, accounting for 2581 (5.8%) clients, could not be geocoded. These deletions leave 21552 cases for analysis of residential location and 28248 for analysis of the treatment location.

Table 2-1 shows sample sizes and client composition in each modality—outpatient methadone maintenance, other outpatient, and residential. The client strata in the first column were selected for their association with treatment outcomes in the literature. Here, methadone maintenance treatment is identified by a prescription of methadone or levo-alpha-acetylmethadol (LAAM) in a non-residential setting. Polydrug use is defined as a drug problem involving two or more substances. Daily use is defined with respect to the primary substance. Employment is defined as "employed" (working either full- or part-time, or with a job available if the client is confined to residential care) or "not employed" (unemployed or not in the labor force). Mental illness indicates whether the client reported having ever been diagnosed with a chronic mental illness. Source of referral distinguishes clients ordered into treatment by the criminal justice system from others.

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5 See Section 0 for a discussion of the precision of these measures and Section Chapter 2IV.D.1 for computation of zip code population and other measures from finer resolution census data.

Table 2-1 Client characteristics and sample sizes, LACPRS 1998-2000

	Outpatient		Residential		Meth. Maint.		All Modalities	
Client stratum	N	%[a]	N	%	N	%	N	%
Sex								
Female	7180	42.96	3932	34.04	878	43.17	11990	39.57
Male	9532	57.04	7620	65.96	1156	56.83	18308	60.43
Age								
Adult	14413	86.24	11264	97.51	2034	100.00	27711	91.46
Juvenile	2299	13.76	288	2.49	0	0.00	2587	8.54
Race, Ethnicity								
Asian / Pac. Isl.	323	1.93	165	1.43	8	0.39	496	1.64
Black	6209	37.15	4569	39.55	453	22.27	11231	37.07
Hispanic	5298	31.70	2421	20.96	911	44.79	8630	28.48
Native American	136	0.81	79	0.68	14	0.69	229	0.76
White	4501	26.93	3970	34.37	617	30.33	9088	30.00
Other	245	1.47	348	3.01	31	1.52	624	2.06
Education								
< 12yrs	8095	48.44	4340	37.57	906	44.54	13341	44.03
≥ 12 yrs	8617	51.56	7212	62.43	1128	55.46	16957	55.97
Drug Problem								
Marijuana only	1234	7.38	103	0.89	1	0.05	1338	4.42
Other drug only	2719	16.27	2204	19.08	1197	58.85	6120	20.20
Polydrug	12759	76.35	9245	80.03	836	41.10	22840	75.38
Severity of use								
Less than daily use	11028	65.99	3738	32.36	212	10.42	14978	49.44
Daily use	5684	34.01	7814	67.64	1822	89.58	15320	50.56
Non-injection user	14717	88.06	9222	79.83	68	3.34	24007	79.24
Injection user	1995	11.94	2330	20.17	1966	96.66	6291	20.76
Prior treatment								
None	8618	51.57	4554	39.42	92	4.52	13264	43.78
1 prior episode	4390	26.27	3072	26.59	257	12.64	7719	25.48
2 or more	3704	22.16	3926	33.99	1685	82.84	9315	30.74
Employment								
Not employed	13450	80.48	11215	97.08	1838	90.36	26503	87.47
Employed	3262	19.52	337	2.92	196	9.64	3795	12.53
Dual diagnosis								
No mental illness	15375	92.00	10600	91.76	1873	92.08	27848	91.91
Mental illness	1337	8.00	952	8.24	161	7.92	2450	8.09
Source of referral								
Not criminal justice	10961	65.59	8344	72.23	2033	99.95	21338	70.43
Criminal justice	5751	34.41	3208	27.77	1	0.05	8960	29.57
Homeless								
Not homeless	14498	86.75	6178	53.48	1988	97.74	22664	74.80
Homeless	2214	13.25	5374	46.52	46	2.26	7634	25.20
All Clients[b]	16712	55.16	11552	38.13	2034	6.71	30298	100.00

[a] columns give percentage of clients in a particular modality

[b] final row gives percentage of all clients

IV.B Contextual Measures

Eight contextual measures operationalize the area-level constructs introduced in Section II: disadvantage, homicide rate, and calls for service to police (social stressors); jobs per capita (proximity to jobs); supermarkets per capita (proximity to retail services); and drug-induced deaths, drug arrests, and drug treatment episodes (drug availability). Data for these measures come from several sources, each with its own georeferencing system. Because the client's zip code is the only geographic identifier in LACPRS, each contextual measure is aggregated from the finest geographic resolution available to the zip code level. As a proxy for neighborhoods, zip codes suffer several shortcomings, although often used in neighborhood analyses (e.g., Bingham and Zhang 1997, Acevedo-Garcia 2001). Section IV.D describes our approach to mitigate these shortcomings. As a general strategy, we construct measures that approximate the *expected value* of each contextual measure for a given zip code resident. To form zip code level population counts required for the denominator of some measures—described in detail below—census counts at the census block, block group, and tract levels were extracted from Census 2000 Summary Files 1 and 3, allocated to zip codes proportional to the share of the census area falling in each zip code. Counts of arrests for drug-related offenses and calls for service from the Los Angeles Police Department (LAPD) Crime Analysis Unit use a system of police reporting districts that also do not align with zip codes and had to be aggregated similarly. Other measures were obtained at the zip code level: homicides and drug-related deaths from Center for Health Statistics (CHS), discharges from chemical dependency recovery inpatient care from the Office of Statewide Health Planning and Development (OSHPD), counts of supermarkets and employment from the U.S. Census Bureau's 1999 Zip Code Business Patterns data set.

As mentioned earlier, three of the measures—homicides, drug-induced deaths, and drug treatment episodes—are rare events for which we compute average annual per capita rates. In addition to restricting zip codes based on population size, we take a longer time history of data to obtain adequate precision in each zip code: five years for homicides and drug-induced deaths and two years for drug-treatment episodes, based on their overall rates in the county of 1.1, 0.7, and 28 per ten thousand, respectively. Section VIII.A in the appendix

discusses the issues involved in taking a longer time history of data and develops a model to select an adequate number of years to include in the average.

IV.B.1 Neighborhood Disadvantage

We adopt Boardman et al.'s (2001) measure of neighborhood disadvantage, an indicator of poverty and other dimensions of socioeconomic status, which they constructed at the census tract level. For each zip code, neighborhood disadvantage consists of four components, standardized and then summed: (1) percent of individuals living below the poverty line; (2) male unemployment rate; (3) percent of occupied households with families headed by a female; and (4) percent of families receiving public assistance. Some variants of the disadvantage measure include high school dropouts (e.g., Saxe, et al. 2001), but our intent is to measure poverty and household resources. The components were obtained from Census 2000 data at the finest resolution available: poverty and unemployment at the block group level and female-headed households and public assistance at the tract level. Counts were allocated to zip codes as described earlier.

IV.B.2 Violence and Victimization

We use the homicide rate and selected calls for service to the LAPD as proxies for violence and victimization. Homicide counts come from CHS and calls of service from the Crime Analysis Unit of the LAPD. Both are rare events and were obtained for a longer time history than other measures to provide adequate precision in each zip code (see Section VIII.A). Counts were obtained for 1996-2000, averaged across years and divided by the zip code population to obtain average annual rates per capita. Calls for service were restricted to those that imply a victim or possible violence: assault with a deadly weapon, arson, attack, battery, bomb, burglary, child abuse, dispute, explosion, kidnapping, murder, a prowler or a neighbor reporting an open window or door,[6] robbery, screaming, shots fired, theft, and vandalism.[7] These reflect a broader class of sources of victimization than homicides, but under-reporting makes the measure problematic—nearly half of all violent crimes in Los Angeles are unreported (Hart and Rennison 2003). Further, there is the possibility that residents of neighborhoods with the highest rates of victimization are the least likely to report crimes, either due to negative views of law enforcement or fear of retribution by the parties

[6] According to the LAPD Crime Analysis Unit, these incidents, coded simply as "open", often result in apprehension of a suspect for breaking and entering or burglary.

involved in the incident. However, we suspect such neighborhoods are still likely to log a larger number of calls than less victimized areas.

IV.B.3 Proximity to Retail Services

Because preferences for retail goods may differ among neighborhoods, we examine one essential establishment, supermarkets, which offer lower prices, higher quality, and a broader selection of goods than Mom-And-Pop operations and convenience stores. In their study of economic geography and poverty in Ohio, Bingham and Zhang (1997) operationalize "chain" supermarkets as groceries (Standard Industrial Classification (SIC) 541) with 50 or more employees. We map their definition to the North American Industry Classification System (NAICS) to extract counts of supermarkets within each zip code from the U.S. Census Bureau's 1999 Zip Code Business Patterns data set.[8] Bingham and Zhang (1997) and others studying local amenities and disamenities often examine counts within a zip code (e.g., Mladenka 1989, Ficker and Hengartner 2001), but we take the sum over the zip code and its neighbors to mitigate boundary effects. Zip codes that border LA county zip codes, but lie outside of the county, are also included in the calculation. The count is then divided by the zip code census population for a final measure of supermarkets per capita.

IV.B.4 Proximity to Jobs

The procedure above is repeated to obtain a measure of number of jobs per person age 16 and over in the labor force in a zip code and its neighbors, again from the 1999 Zip Code Business Patterns data set. These data include employment for all single and multi-establishment companies known to the Census through its Annual Survey of Manufactures and Current Business Surveys, administrative records of the Internal Revenue Service and Social Security Administration, and other Census Bureau programs, excluding self-employed persons, domestic service workers, railroad employees, agricultural production workers, and most government employees.

IV.B.5 Drug availability

Estimates of illicit drug use for small areas are difficult to obtain short of primary data collection. The largest national surveys of drug use, the National Household Survey on Drug

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[7] Calls for service excluded from this analysis are: alarm (According to the Crime Analysis Unit, over 90% are false alarms),

[8] The corresponding NAICS classifications are 45291 (supermarkets and grocery stores with substantial general merchandise) and 44511 (supermarkets and grocery stores with little general merchandise).

Abuse (NHSDA) and Monitoring the Future (MTF), produce representative state-level estimates for some states, but do not have sufficient sample size for precision within individual counties and cities. Even to obtain adequate precision for NHSDA's eight designated "large sample states", Substance Abuse and Mental Health Services Administration (SAMHSA) analysts needed to pool data from two waves of the survey and develop a model to combine the NHSDA sample with small area data sources from states (Wright 2002). The Drug Abuse Warning Network's (DAWN) estimates of emergency department (ED) visits and mortality related to drug use is more promising because DAWN focuses on 21 purposively selected metropolitan areas, but these data are only available at the county level. Further, it is unlikely that even the disaggregated data would provide the geographic coverage necessary for this study because in 2001 the Los Angeles sample included only 33 of the county's more than 80 emergency departments.

Large local probability samples, such as the Los Angeles Families and Neighborhoods Survey (LAFANS) and the Los Angeles Health Survey (LAHS) conducted by RAND and the Los Angeles County Department of Health Services, respectively, are potential alternatives to national level surveys. However, these too suffer poor precision at the zip code level for drug use measures. For example, LAFANS Wave I (2000-2001) asked respondents about drug use, but only in households with children.[9] Even if LAFANS had concentrated its entire sample of N=2000 primary caregivers in, say, 10 of the 318 zip codes in LA, estimates of the rate of use within each zip code would have 95% confidence intervals as large as 14 percentage points, and as large as 20 percentage points if the sample were spread among only 20 zip codes.[10] LAFANS' oversampling of poor census tracts means some zip codes will have better precision than others, but precision for most areas in the county is likely to be poor. Of course, future investigations focusing exclusively on destitute areas may find LAFANS a good source of self-reported drug use, once the restricted area-identified LAFANS datasets become available.

We rely instead on administrative data available for all zip codes in the county to construct three proxy measures for drug availability: drug-induced deaths, arrests for drug-related offenses, and drug treatment episodes. Each has limitations, as described below.

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[9] http://www.lasurvey.rand.org/thesurvey.htm, accessed March 10, 2003.

[10] These figures assume a conservative 50% of respondents report use, when the CI half-width, $1.96\sqrt{\frac{\hat{p}(1-\hat{p})}{n-1}}$, is at its maximum:

Because these are rare events,[11] to increase precision the measures are constructed as annual per capita rates over a three year period, 1998-2000.

Drug-induced deaths. The Center for Health Statistics (CHS) of the California Department of Health and Human Services maintains a Vital Statistics Query System that classifies all deaths in the state by underlying cause of death, the deceased's zip code of residence, and other factors. The underlying cause is operationalized as a set of International Classification of Diseases – Ninth Revision (ICD-9) codes and described as the factor that "initiated the chain of events leading directly to death." [12] We extracted annual counts of "drug-induced" deaths, ICD-9 codes 292 (drug withdrawal syndrome) and 304 (drug dependence) for 1998, 1999, and 2000 by zip code, averaged across years, and divided by the zip code population to obtain a mean per capita rate in 1998-2000. In metropolitan areas, drug-induced death most frequently results from polydrug use involving heroin morphine, cocaine, or alcohol (Substance Abuse and Mental Health Services Administration 2003). Although drug-related deaths have been used in previous research as a proxy for area rates of use (Corman and Mocan 2000), a weakness of this approach is that the probability of death given use may depend on other factors that vary by neighborhood, such as drug type, risky drug-using behaviors, baseline health status, and access to medical care. Thus, we view deaths as an indicator of serious abuse, and potentially, as an indicator of inexperienced use of dangerous drugs (e.g., cocaine, crack, heroin, PCP).

Drug arrests. Individual-level arrest records were obtained from the Los Angeles Police Department's (LAPD) Crime Analysis Unit for 1998, 1999, 2000, for drug-related offenses in each of the LAPD's 1008 reporting districts. Reporting districts are small, on average 1.2 sq km, but are limited to the City of Los Angeles, rather than the county, and do not align with zip codes. The share of each reporting district's area contained in each zip code was computed using a GIS and arrest counts were allocated proportionally to each zip code, following the method in Section IV.D.1.[13] Because the data are limited to the City of Los Angeles, the drug arrest measure are used only in analyses of the 89 zip codes in the county with 75% or more of

[11] Annual rates per 10000 persons in the were 0.20, 3.4, and 64 for drug-induced deaths in the county, drug treatment discharges from inpatient care and LACPRS facilities, and drug-related arrests in the City of Los Angeles, respectively.

[12] http://www.applications.dhs.ca.gov/vsq/instruct.htm, accessed March 10, 2003

[13] A small fraction of reporting district area was contained in harbors off the coast of Long Beach, Pacific Palisades, and Marina Del Rey and does not fall within the borders of any zip code. I examined the map and allocated arrests from these reporting districts to the nearest zip code, which was unambiguous in each case.

their area in LAPD reporting districts. A small number (N=988, 1%) of arrests in 1998-2000 could not be attributed to any zip code. Drug-related arrests include arrests for "use" offenses—possession, use, presence during use, paraphernalia, attempting to obtain or use a fraudulent prescription—and "trafficking" offenses—the sale, transport, furnishing of illicit drugs or fraudulent prescriptions and related offenses. We exclude arrests for failure to register, (i.e., previously convicted drug felons who fail to register with local authorities in a new locality), because the offense does not necessarily indicate present drug activity. While it seems reasonable that "use" and "trafficking" offenses would measure different aspects of drug availability, in these data the two are very highly correlated in zip codes both within and between years (0.9 or greater), so we sum them for a single measure to avoid problems with multicolinearity.

Drug arrests are an imperfect measure of drug availability because they depend on allocation of police resources and local law enforcement propensity to arrest. The measure is probably best interpreted as an indicator of the visibility of local drug markets. In a study of 2100 census tracts in various U.S. cities, Saxe (2001) found that residents of more disadvantaged areas were more likely to perceive local drug market activity, whereas self-reported *use* of any illicit drug was more widespread. Thus, the drug arrest measure may underestimate true drug availability in affluent areas.

Drug treatment episodes. Counts of drug treatment discharges by residents of each zip code come from two sources. Inpatient discharges from chemical dependency care are from the Office of Statewide Health Planning and Development (OSHPD) Patient Public Discharge Data for calendar years 1999 and 2000, Version B. These are inpatient stays in licensed chemical dependency recovery hospital beds by the patient's zip code of residence (Office of Statewide Health Planning and Development 2000).[14] Services in the chemical dependency recovery licensing category include patient counseling, group therapy, physical conditioning, family therapy, outpatient services, and dietetic services.[15] In each zip code, the count is summed with the number of non-hospital discharges in LACPRS in fiscal years 1998-99 and 1999-2000, restricted to persons who reported a substance other than alcohol as the primary, secondary, or tertiary illicit drug problem at admission or discharge. The composite measure

[14] The OSHPD data capture discharges from all licensed acute care hospitals, including general acute care hospitals, acute psychiatric hospitals, psychiatric health facilities, and chemical dependency recovery hospitals.

captures hospital-based treatment, which may be confounded with access to inpatient care, and outpatient and residential drug treatment, where the cost is offset with public funds. The measure does not account for treatment episodes at private, non-hospital treatment facilities that receive no public funding.

Table 2-2 shows the mean, standard deviation, and range of the contextual measures. Table 2-3Table 2-3 summarizes geographic coverage of the client population included in LAC and the "L.A. City" study area, which includes zip codes in the county with at least 75% of their area in LAPD reporting districts. From the table, the L.A. City study area includes about a third of the facilities in the county in each modality and a third of client discharges.

Table 2-2 Contextual Measures

Construct	Measure	Observation Period	Units	Mean‡	SD	Range
Social Stressors	Disadvantage	2000	N/A (standardized scale)	0	3.6	-5.6, 14.5
	Violence and victimization:					
	Homicides	1996-2000	Homicides per year per capita x 10000	1.0	1.0	0, 5.6
	Calls for service to law enforcement†	1996-2000	Calls per year per capita x 10000	850	486	274, 3902
Employment opportunities	Proximity to jobs	1999	Jobs per capita*	0.9	0.5	0.2, 3.4
Daily hassles	Proximity to supermarkets	1999	Supermarkets per capita x 10000	0.6	0.3	0, 2.5
Drug availability	Drug-induced deaths	1998-2000	Deaths per year per capita x 10000	0.7	0.6	0, 6.8
	Treatment episodes	1998-2000	Discharges per year per capita x 10000	27	23	0, 305
	Drug-related arrests†	1998-2000	Arrests per year per capita x 10000	99	231	3.8, 2006

‡ Among zip codes

† L.A. City study area (zip codes in LAC with at least 75% of their area within LAPD's jurisdiction)

* The denominator is the census count of individuals in the labor force age 16 or older.

Table 2-3 Coverage of the L.A. County and L.A. City study areas

	Study Area			
	L.A. County		L.A. City	
Valid zip codes	283		80*	
Number (percent) of clients with a				
…valid residential zip code	21552	(100)	6847	(31.8)
…valid treatment zip code	28248	(100)	10330	(36.6)
Number (percent) of facilities				
Outpatient	127	(100)	43	(33.9)

[15] See California Health and Safety Code 1250.3(a)

Meth. Maint.	65	(100)	30	(46.2)
Residential	43	(100)	16	(37.2)

IV.C Distance to Provider

Distance from the client's household to the treatment site cannot be computed directly because only clients' zip codes rather than household locations are reported by LACPRS, but the distance can be approximated. A common approximation is the distance from the zip code's population centroid; this is the *expected location* for residents of the zip code. A problem with this approach is that distance from the expected location is not equivalent to the *expected distance* for a given resident of the zip code because distance functions (e.g., Euclidean, Manhattan, and transit-based distances) are not linear, so that the distance as measured from the average location is not the same as the average over the distance from each resident or household in the zip code (i.e., $E(f(x)) \neq f(E(x))$. Hewko, Smoyer-Tomic, and Hodgson (2002) demonstrate that differences between the true distance and the centroid measure can be large. Unfortunately, with our data we cannot compute the expected distance for the zip code population either because census data as well do not reveal the exact locations of households. The best approximation using census data is obtained by computing the distance[16] from each block or portion of a block that falls entirely within one zip code or another, then summing the results within the zip code, weighting by block population. The method is computationally intensive (22 million distance calculations in our data) and was implemented in C for faster processing.

IV.D Special Considerations for Zip Code Level Data

A wealth of contextual data are available at the zip code level, but zip codes as representations of neighborhoods have been criticized on several grounds:

1. Their boundaries change over time, making comparing results across studies difficult;
2. They vary greatly in area and population;

[16] We use Euclidean distance, rather than driving distance over the transit network, because of the difficulty of determining the street address nearest to a block centroid.

3. They are not directly comparable with census enumeration areas, so that aggregation from measures at the tract, block group, or block level is not straightforward;
4. They are designed by the U.S. Postal Service to facilitate mail delivery, not to approximate homogeneous neighborhoods;

We address the first point by drawing all data and zip code boundaries from a similar timeframe: 1998-2000 for client data and most contextual measures; 1996-2000 for measures rare events. A consequence of the second point is that the precision of contextual measures will vary among zip codes. For example, estimates of the rate of drug-related deaths will be more precise in zip codes with a larger population. We average data over a timeframe selected to provide adequate precision for smaller zip codes (see Section VIII.A).

IV.D.1 Aggregation

A problem that arises as a consequence of the third point is illustrated in Figure 2-1, which shows a typical relationship between zip code and census tract boundaries in Los Angeles. The fact that census areas overlap with zip codes means that the true number of residents in a zip code cannot be computed simply by summing counts from blocks or tracts, because the fraction of the population within the census area who reside in each zip code is unknown. This problem is often ignored (Bingham, et al. 1997, p. 294-295, Acevedo-Garcia 2001, p. 735-736).[17] However, under the assumption that populations are uniformly distributed within blocks or tracts, the fraction of individuals in each zip code can be computed using a GIS. Taking for the moment the example of tract-level data, the GIS is first used to obtain the set of all pairs of zip codes and tracts that overlap, along with the share of the surface area in each area of intersection. Then census counts at the tract level are weighted up to the zip code level, where the weighting function accounts for the area share and population of the tract. Equation 2-1 formalizes the procedure for the measure percent of persons in poverty, for which the finest level of resolution available is the block group.

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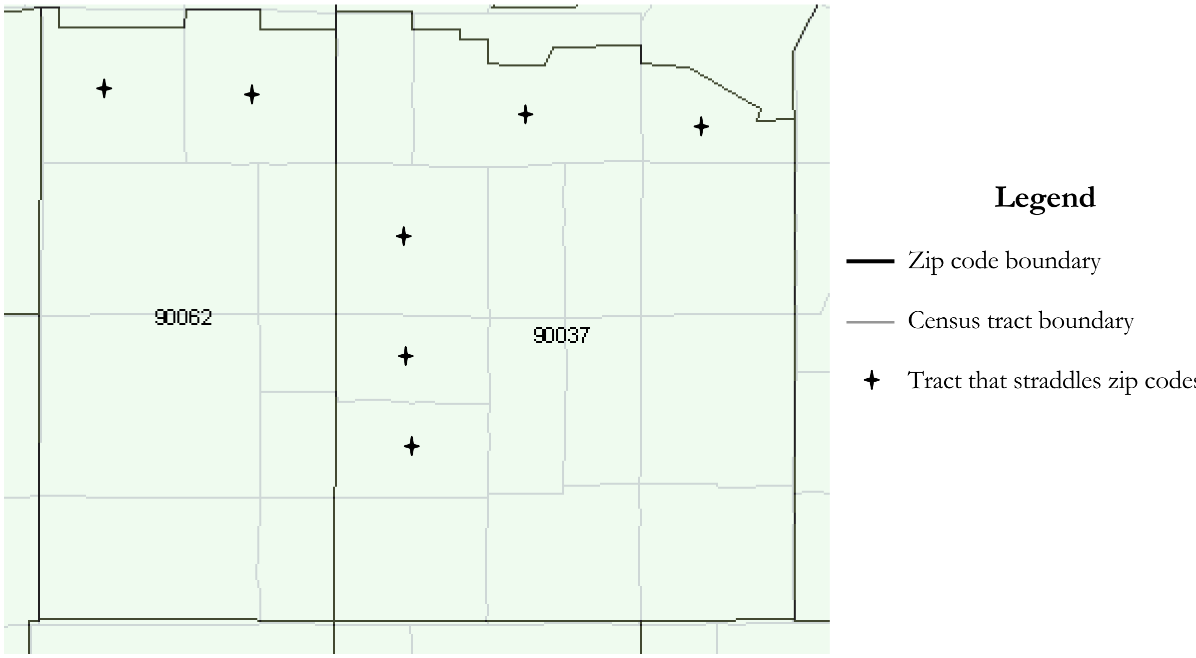
[17] In estimating the mean exposure to poverty, overcrowding, and dilapidated housing for residents in a zip code, computed from block group level data, Acavedo-Garcia's expressions implicitly assume that block groups are nested entirely within one zip code or another. Bingham and Zhang's calculation of poverty rates within zip codes assume the same of census tracts.

Figure 2-1 Misalignment of two zip codes with census tracts in Los Angeles



$$Y_z = \frac{\sum_{b \in B_{bz}} s_{bz} p_b}{\sum_{b \in B_{bz}} s_{bz} i_b} = \frac{\text{Persons in poverty in z}}{\text{Persons for whom poverty status is determined in z}} \tag{2-2}$$

Y_z – percent of persons in poverty in zip code z

B_{bz} – the set of block group "subsections" (e.g., unique subsection of a block group) entirely contained within zip code z

s_{bz} – percent of block group b's area that falls in zip code z, computed by a GIS[18]

p_b – census count of persons in poverty in block group b

i_b – census count of persons for whom poverty status is determined in block group b

IV.D.2 A Test of Homogeneity within Zip Codes

The critique that zip codes do not represent homogeneous areas can be viewed as a measurement error problem. If areas *within* zip codes differ from one another, then zip code level averages would reveal little about the neighborhood conditions facing a given zip code resident: these conditions would be measured with error. In regression models, estimates of coefficients for covariates measured with error are inconsistent and biased with effect sizes

18 ArcGIS 8.0 was used for all computations requiring a GIS.

smaller in magnitude than their true values. Of course, the error is mitigated to the extent that zip codes are internally homogeneous.

One way of examining internal homogeneity is to assess the extent to which smaller areas within zip codes are correlated. Here, we examine internal homogeneity of a single measure, percent of persons in poverty, which was aggregated from block group subsections[19] to zip codes earlier. To assess intra- and inter-zip-code variation, we use the standard random intercept model with notation below from Snijders and Bosker (2002, p. 18-22):

$$Y_{bz} = \mu + U_z + R_{bz} \tag{2-3}$$

Y_{bz}	–	poverty rate in block subsection b in zip code z
μ	–	grand mean poverty rate
U_z	–	effect of zip code z
R_{bz}	–	residual effect for block subsection b in zip code z

This model assumes that the poverty rate in each block group subsection is the sum of a grand mean, a random zip code effect, and a random residual that is independent of the zip code effect. Both effects are assumed to be normally distributed with zero mean and unknown variance, which must be estimated. Under these assumptions, the total variance in the poverty rate is just $\mathrm{Var}(U_z) + \mathrm{Var}(R_{bz})$, the within and between zip code variance components. It is important to note that the within-group variance is not the variance among all block group subsections in the study area—which would say little about homogeneity within zip codes—but rather a weighted sum of the variance among subsections computed separately for each zip code (equation 2-4).[20]

In Census 2000 Los Angeles County data, the estimated within and between zip code variances are 0.009 and 0.011, respectively. The intraclass (intra zip code) correlation coefficient (ICC), which measures the proportion of variance in poverty rates attributable to variation between zip codes—or equivalently, to correlation within zip codes—is 0.535, and the F-statistic testing the null hypothesis that ICC=0 is statistically significant ($F=151.3$, $p<0.001$). These results suggest that zip codes are homogenous with respect to poverty,

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[19] Recall that a block group subsection is a piece of a block group that falls entirely within one zip code or another.

[20] The expression uses n_j rather than n_j-1 as in Snijders, T., and Bosker, R. (2002), *Multilevel Analysis: An Introduction to Basic and Advanced Multilevel Modeling*, London: SAGE Publications. because the set of block group subsections within each zip code is a census rather than a sample.

though this of course says nothing about whether they mirror residents' perceptions of their neighborhoods.

$$s^2_{within} = \frac{1}{M-N}\sum_{z=1}^{n_z} n_z s_z^2 \tag{2-4}$$

s^2_{within}	–	weighted average within zip code variance
s^2_z	–	variance among block group subsections within zip code z
n_z	–	number of block group subsections in zip code z
M	–	total number of block group subsections
N	–	number of zip codes

A simpler alternative to the calculation above would be to compute the observed variance among block groups and among zip codes from standard formulae, without committing ourselves to the assumptions of the random intercept model. However, such standard calculations would overstate the between zip code variance (Snijders, et al. 2002, p. 17) and the ICC.

V Statistical Analyses

We first estimate the complete empirical distribution of each contextual measure, separately at clients' residential and treatment locations, in Figure 2-2 and Figure 2-3. Examining the full distribution is important because as of yet the effect of exposure to any given level of each contextual measure is unknown. The distributions are presented as survival functions, to show the fraction of individuals who reside (or receive treatment in) a zip code with a value at or greater than the value displayed on the x-axis. A similar curve for the general (i.e., non-client) population is superimposed for comparison. The count of the general population in each zip code required to construct this curve is the 2000 decennial census count less the number of clients residing in the zip code. Figure 2-2 and Figure 2-3 are plots of the distributions for non-homeless clients and homeless clients (whose residential location is unknown), respectively. To provide a quantitative summary, we then estimate a ratio of means: Table 2-4 gives the mean of each contextual measure for clients' residential locations relative to the mean for the general population, separately by modality and client stratum. The standard error of the ratio and the results of a one-sample t-test of the null hypothesis that the mean for the client sample is equal to the mean for the general population also appear in the table. We use a one-sample test because the general population mean is a known population parameter

calculated from census data, rather than a sample estimate. Table 2-5 presents similar ratios comparing *treatment* locations to households in the general population.

To identify client subpopulations that are more exposed than others, independent of other client attributes, we then regress each contextual measure on a set of dummy variables representing the client strata (Table 2-6 and Table 2-7). In each case, the dependent variable (the contextual measure) is standardized so that the coefficient estimates are in units of standard deviations.

We then turn to comparisons between clients' residential and treatment locations. To convey a sense of their physical separation, we estimate the expected straight-line separation distance from the client's zip code to the address of the treatment site and plot the survival curve by modality (Figure 2-4). Note that in the graph each modality is represented by a different color. As before, the graphical summaries are followed by regressions to identify client subpopulations who travel farther to treatment (Table 2-8). The logarithm of the expected distance is regressed on dummy variables representing the client strata, where the log transform is used to improve the fit of the model in the presence of skew and outliers in the distance distribution. In this case, separate models are estimated by modality because the dynamics of travel burden differ considerably among methadone maintenance, outpatient, and residential care.

Qualitative differences between the residential and treatment site in terms of the contextual measures are assessed in Figure 2-5 and Table 2-9. For each contextual measure, we first bin all of the zip codes in LAC into quartiles. Since we have eight contextual measures, this produces eight separate rank orderings of the zip codes; in each case the ranking is a function of only the distribution of the contextual measure among zip codes. We then estimate the fraction of clients who travel from a household in one quartile of the measure to treatment in another. The result is a 4x4 matrix for each measure whose cells indicate the percent of clients whose residential zip code falls in the quantile indicated by the row and whose treatment zip code falls in the quartile indicated by the column. For brevity, we present just two of the eight matrices: disadvantage and drug arrests (Figure 2-5). Table 2-9 provides a more concise, but less informative summary. It divides zip codes into halves instead of quartiles for each measure; then, for clients who reside in the "best" half of zip codes and the "worst" half of zip codes, Table 2-9 presents the fraction of clients whose treatment location is (1) "about the same" (within one standard deviation) as their residential zip code; (2) "better" (by at least one

standard deviation) than their residential zip code; and "worse" (by at least one standard deviation).[21] The table is intended to convey a sense of flow between types of neighborhoods.

Due to issues of data availability discussed earlier, all subsequent analyses of drug arrests and calls for service pertain only to clients who reside in *and* attend treatment within the L.A. City (e.g., LAPD) study area. The other contextual measures pertain to clients and treatment locations anywhere in the county.

VI Findings

Research question #1 (RQ1) asks to what extent clients are exposed to each contextual measure. The principal finding from plots (a)-(h) along the left column of Figure 2-2 is that neighborhoods where clients come from are more disadvantaged, have more violence and victimization, and have more signs of drug activity than the neighborhoods of the general household population. For these measures, the curves for both outpatient and residential treatment clients lie to the right of the curve for the general population over the range of values along the x-axis, which indicates that greater exposure to these environmental conditions holds *no matter what critical value* we might choose for the comparison. In the case of homicides, for example, analysts who disagree as to whether a rate of 0.5, 2, or 6 homicides in ten thousand constitutes a significant level of violence would all reach the same conclusion, that treatment clients are more likely than the general population to live in zip codes with the given level of homicides or greater. The relatively smooth descent indicates that this difference is not due to clustering of clients in a small number of worse-off neighborhoods. Note that while it is tempting to compare the area between curves across measures and conclude that the differences are smaller for drug-related arrests than for disadvantage and homicides, such visual comparisons should be avoided because they can be heavily influenced by outliers.

Plots (i)-(p) of Figure 2-2 show that *treatment* locations are also in areas with higher rates of disadvantage, violence, and drug activity than the general population, and the difference appears greater than in the case of clients' residential neighborhoods. Further, outpatient facilities tend to be in worse areas along these measures than residential facilities. Clients' residential neighborhoods also have lower proximity to jobs and supermarkets, again

[21] For disadvantage, homicides, treatment episodes, drug deaths, drug arrests, and calls for service, lower values are considered "better"; for proximity to jobs and supermarkets, lower values are considered "worse".

no matter what the level or critical value examined. In contrast, whether treatment locations have lower proximity to jobs and supermarkets compared to the general population does depend on the critical value examined.

Finally, plots (a)-(h) of Figure 2-3 demonstrate that *homeless* clients attend treatment in locations notably worse than non-homeless clients, which may simply reflect differences in areas where they reside. The worst areas are homeless clients' outpatient locations. For example, while more than half of homeless outpatient clients attend treatment in areas with homicide rates of at least 2.0 per ten thousand residents, only a quarter of non-homeless outpatient clients experience homicide rates at this level. For the general household population the fraction is just 12%. One exception is that proximity to jobs at the treatment location is better for homeless outpatient clients than homeless residential clients and even the general household population, which may indicate that outpatient facilities tend to be placed in commercial, rather than residential areas. Interestingly, this relation does not hold for treatment locations of non-homeless outpatient clients. The steep descent of the curves (o)-(v) suggests that unlike non-homeless clients, a large portion of the homeless population in recovery is centered in a small number of zip codes that perform poorly with respect to these measures.

To address RQ2, Table 2-4 quantifies these relationships for a variety of client strata. Each cell is the ratio of the mean for a client population relative to the mean for the general population. For example, the first row indicates that the average non-homeless outpatient client lives in an area with 3.5 times the level of disadvantage, 1.5 the rate of homicide rate, 1.2 and times the rate of drug-induced deaths than the average county resident. The two rightmost columns have the same interpretation but apply only to L.A. City. We use italics to indicate that the ratio is *not* statistically different from 1.0 at the 5% level. All others are significant.[22] The main finding from Table 2-4 is that regardless of treatment setting, sex, age, education, drug choice, addiction severity, prior treatment history, employment status, presence of chronic mental illness, or criminal justice status, drug treatment clients on average live in neighborhoods with higher levels of disadvantage, violence and victimization, and indicators of drug activity than the general population. We identify white clients as the one subpopulation living in areas of *less* disadvantage, violence and victimization than the general population. Of

[22] Some ratios are significant, but differ only slightly from 1.0. We have rounded nonetheless to make the table legible.

all the client strata examined, black clients reside in areas with the highest levels of these measures on average ($p<0.01$ in each case). Black clients reside in areas with on average 6.6 times the level of disadvantage than the average resident, compared to 1.8 ($p<0.001$) for clients in other race groups, and 2.7 compared to 1.4 times the level of drug arrests ($p<0.001$).

More generally, clients who live in more disadvantaged areas on average are females, adults, racial or ethnic minorities, less educated clients, and clients who are not employed. By comparison, some of the historically most difficult-to-treat populations—poly-drug using clients, injection users, and chronically mentally ill clients—reside in *less* disadvantaged areas than other clients, as do those who enter treatment through the criminal justice system.

With respect to drug activity, levels are higher on average for clients in residential and methadone maintenance treatment, adults, blacks, more educated clients, clients who use multiple drugs or an illicit drug other than marijuana, clients with previous treatment episodes, those not employed, and non-criminal justice clients. In magnitude, many of these differences appear small, on the order of 10%. However, due to the large sample size all are significant at the 1% level. With respect to violence and victimization, we find the largest differences for adults and racial and ethnic minorities. The table provides little evidence that clients live in areas with less access to jobs. However, most clients live in areas with less access to supermarkets on average.

The regression estimates in Table 2-6 mostly confirm the bivariate results, but important differences emerge once the relationship between each contextual measure and client stratum is assessed conditional on the other strata. With regard to disadvantage in the residential neighborhood, differences between men and women, and between the historically most difficult-to-treat clients and others, largely disappear: in each case the estimated difference is less than one tenth of a standard deviation. With respect to violence and victimization, the two measures, homicide and calls for service, yield different results. But, as in the bivariate results adults and racial and ethnic minorities live in areas with significantly higher homicide rates (differences estimated from 0.2 to 1.6 SDs); to this list Table 2-6 adds clients who are less educated, not employed, or not mentally ill—again all statistically significant at the 5% level with effect sizes of 0.10 SD or greater. Finally, compared to white clients, racial and ethnic minorities live in areas with less access to markets (0.2 to 0.5 SDs).

From Table 2-6 we also see that residential clients come from less disadvantaged neighborhoods with lower homicide rates than other clients (among the non-homeless). Note

that the column for the drug deaths measure is not shown in the table because no coefficient was larger than 0.01 SDs.

Table 2-5 is the same as Table 2-4, but pertains to *treatment* locations. As the distributional plots suggested, treatment locations are considerably worse than residential locations in terms of all of the measures, particularly disadvantage and drug arrests. While Table 2-4 showed that white clients are the only group to live in areas less disadvantaged than the county average, we find from Table 2-5 that all client groups, including whites, on average attend treatment in areas more disadvantaged than the general household population, by factors ranging from 1.0 to 6.5. The table also shows that among clients, the homeless attend treatment in the worst areas, along all measures except disadvantage (as a group, only blacks attend treatment in more disadvantaged areas than the homeless).

The regression results for treatment locations (Table 2-7) for the most part mirror the results reported above for residential locations. An important difference is that more severe users (i.e., daily use) and clients who have been in the treatment system longer, attend treatment in significantly more disadvantaged neighborhoods than other clients (estimated differences from 0.1 to 0.2 SDs).

To address RQ3, we turn to a more direct comparison of residential and treatment locations. In terms of geographic distance, residential clients live the farthest from their treatment centers (not surprising since the other modalities involve a frequent commute), followed by methadone maintenance and other outpatient clients (Figure 2-4). From the figure, the vast majority of residential clients (90%) attend treatment within 30 miles of home and nearly all (99%) within 46 miles of home. In contrast, 90% of methadone maintenance and other outpatient clients attend treatment within just 9 miles of home and 99% within 22 miles.23 For comparison, the latter figure of 22 miles is roughly in line the with average commute time to work in Los Angeles, which in 2001 was about 30 minutes (Newman 2003). This is equivalent to 17.5 miles assuming commuters travel at the speed limit on city streets (35mil/hr), and 27.5 miles assuming they travel the speed limit on the freeway (55mil/hr). Thus, 99% of those clients who travel periodically to treatment face a journey to treatment that is within about 20-25% of what the distance the average commuter travels to get to work,

[23] These estimates ignore the small fraction of clients who cross the county line to obtain treatment, who may travel farther than others.

roughly speaking, and the vast majority of clients face a treatment commute that is considerably shorter.

Table 2-8 shows the results of a regression of log distance on dummy variables representing the client strata. Men and children, more educated clients, employed clients, criminal justice clients, and heavy users tend to live farther away from their treatment programs on average. Of the statistically significant differences, however, few represent a difference in magnitude of more than 1%.

Qualitative differences are evaluated in the matrices in Figure 2-5 and Table 2-9. In Figure 2-5, the rows of each matrix sum to one. Higher quartiles along the rows and columns indicate more disadvantage or a higher rate of drug arrests. The cells display the fraction of clients who live in a residential zip code in the quartile indicated by the row who receive treatment in the quartile indicated by the column. For example, among clients who live in the least disadvantaged zip codes (quartile 1), 11% attend treatment in the same quartile and 29% attend treatment in the most disadvantaged zip codes (quartile 4). Along the diagonal are clients who receive treatment in locations in the same quartile as their home neighborhood. The results for both measures indicate a flow from better to worse neighborhoods with respect to disadvantage and drug activity. Even among clients coming from the best areas (quartiles 1 and 2), a large majority attend treatment in considerably worse areas (quartiles 3 and 4). Clients living in the higher quartiles mostly stay there for treatment. For brevity, only matrices for two of the eight contextual measures are shown.

A problem with the matrices is that if many clients are near the quartile boundary, the estimated "flow" between areas can be overstated. Table 2-9 provides a less detailed summary, but one that avoids the boundary problem. Here, the difference between each client's residential and treatment zip code is computed and classified as "better" (client's treatment zip code is better than his/her residential zip code by 1 SD or more), "worse" (by 1 SD or more), or "about the same" (client's treatment and residential zips are within 1 SD). Results are then tallied separately for clients coming from the best or worst half of zip codes, again with respect to each measure. The cells report the fraction of clients in each group who travel to treatment in an area that is better, worse, or about the same than their residential neighborhood. Findings are similar, but less dramatic than would appear from the matrices. A considerable flow of clients from the upper half of neighborhoods to worse neighborhoods (16 to 35%) remains using the stricter definition, but most clients receive treatment in a neighborhood

similar to where they live. A smaller fraction of clients in the worst neighborhoods (5 to 17%) receive treatment in a better one.

Figure 2-2 Distribution of contextual measures by residential and treatment location, non-homeless clients

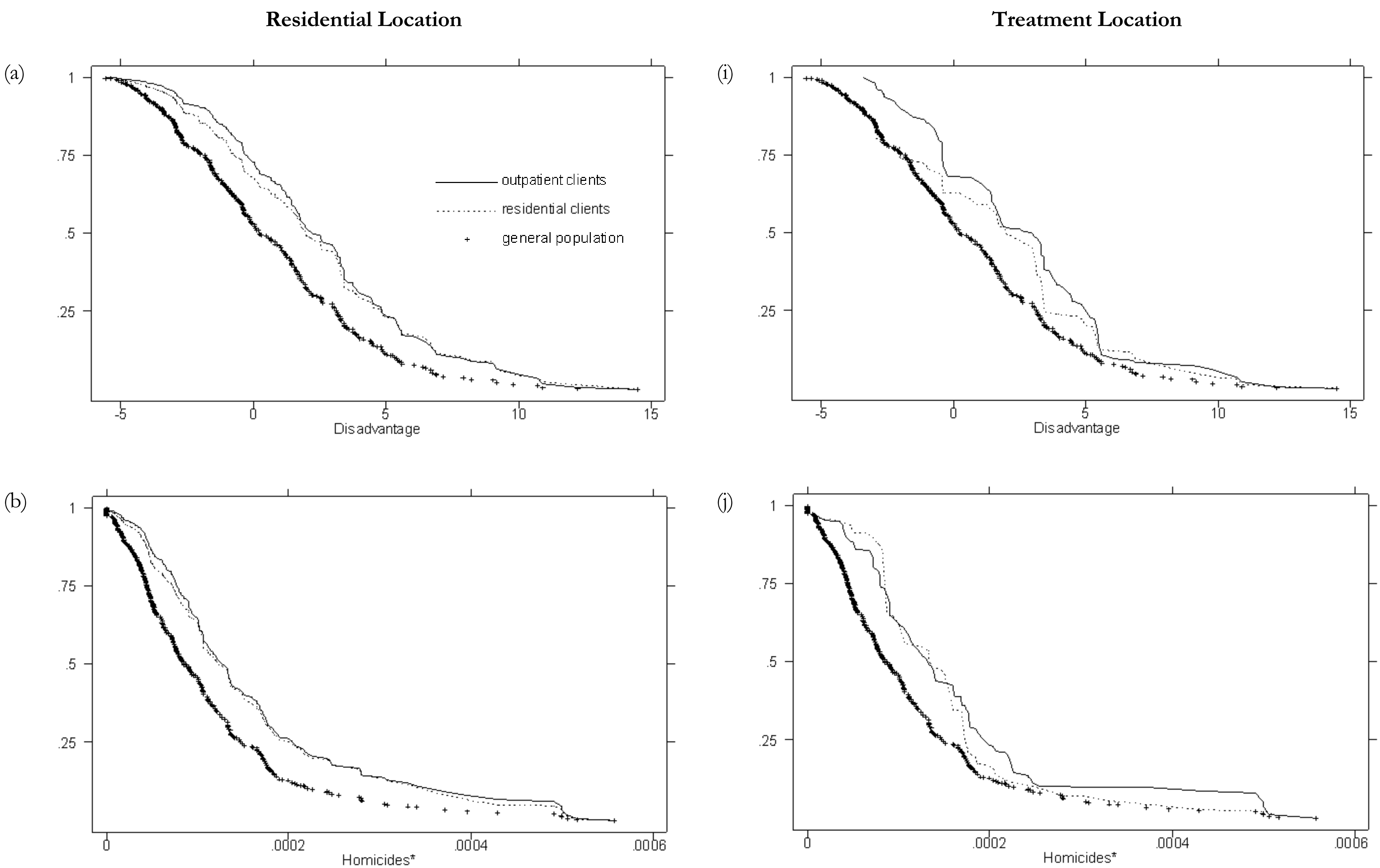

Figure 2-2 Distribution of contextual measures by residential and treatment location, non-homeless clients, continued

Residential Location **Treatment Location**

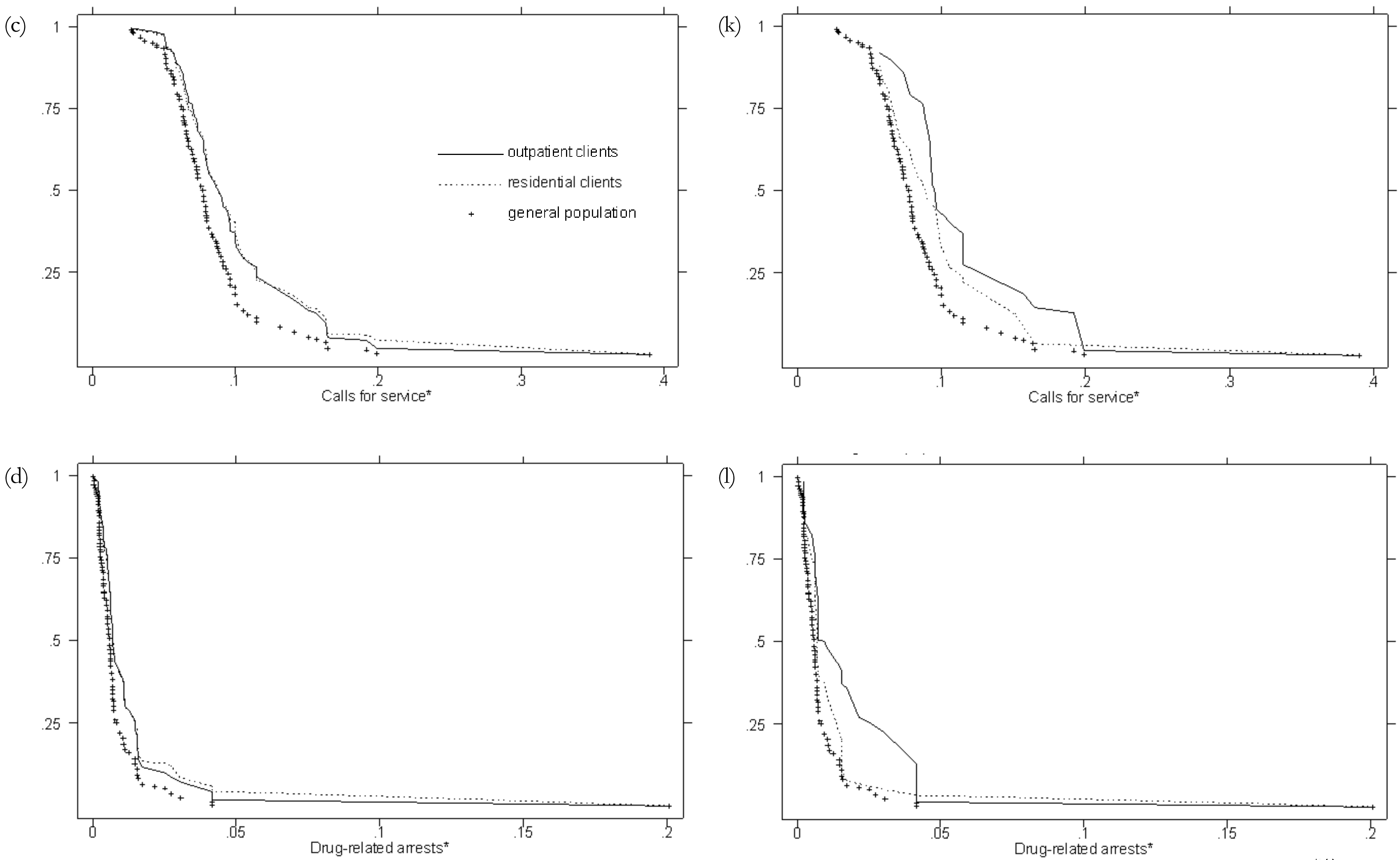

Figure 2-2 Distribution of contextual measures by residential and treatment location, non-homeless clients, continued

Residential Location | **Treatment Location**

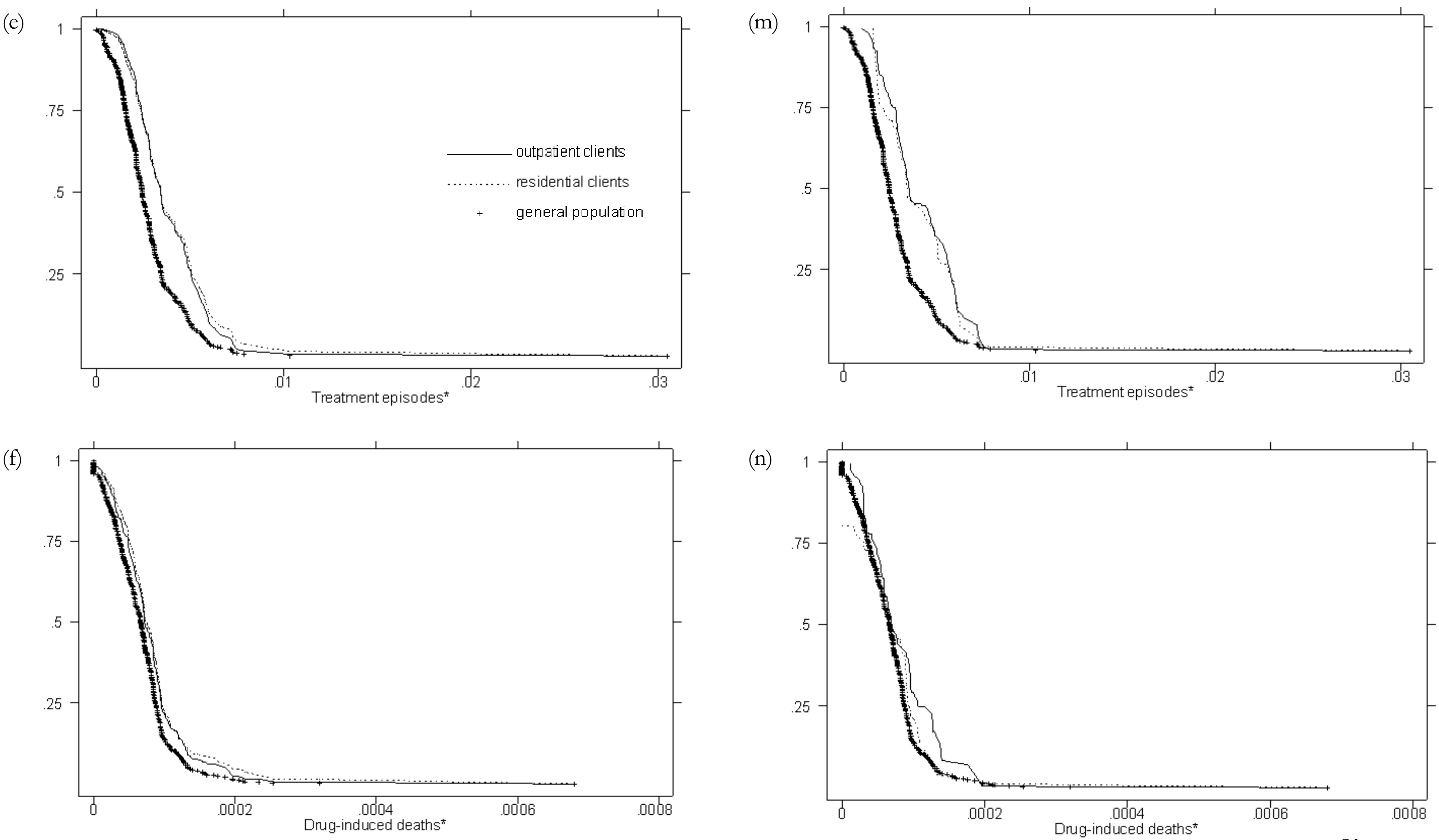

Figure 2-2 Distribution of contextual measures by residential and treatment location, non-homeless clients, continued

Residential Location **Treatment Location**

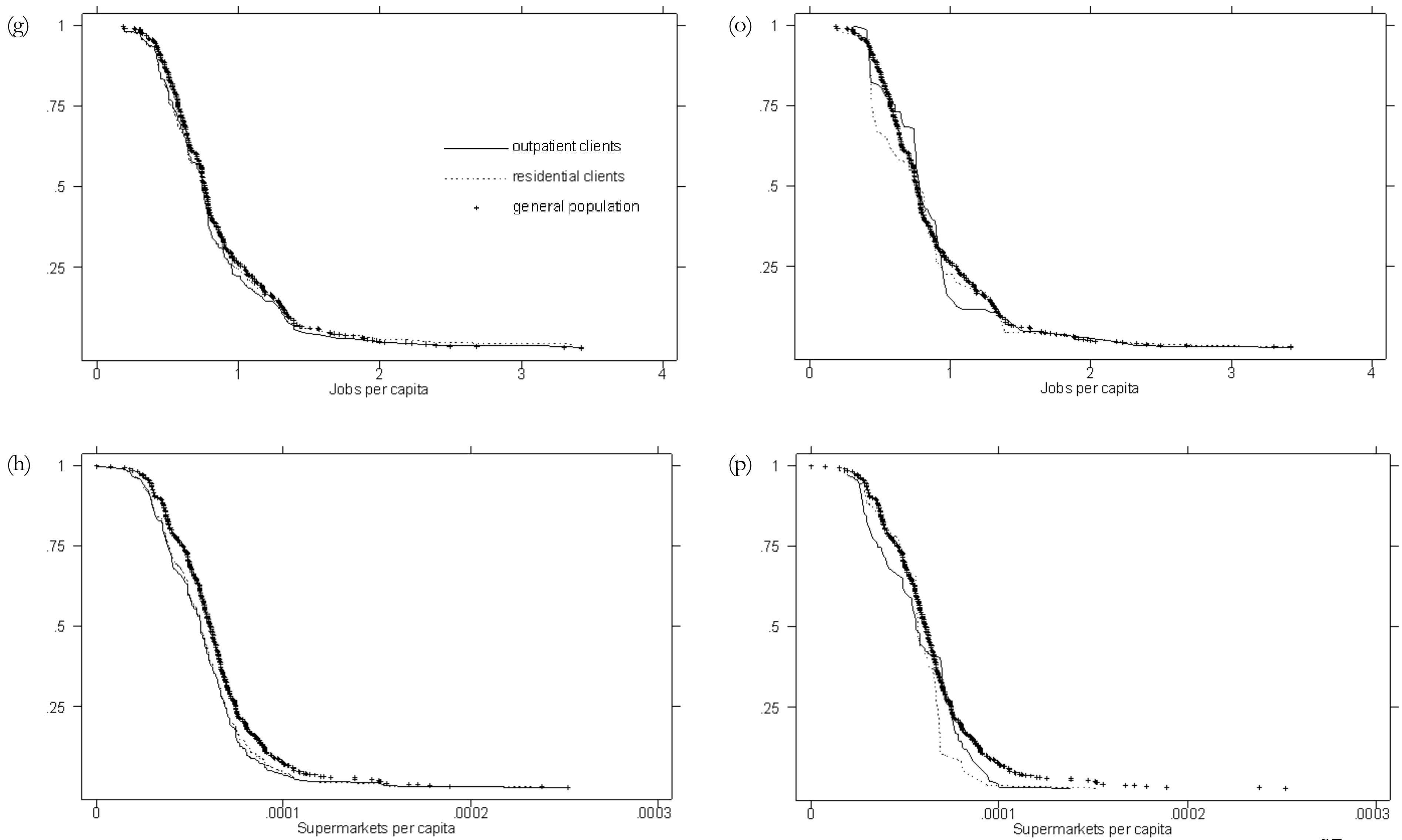

Figure 2-3 Distribution of contextual measures at treatment location, homeless clients

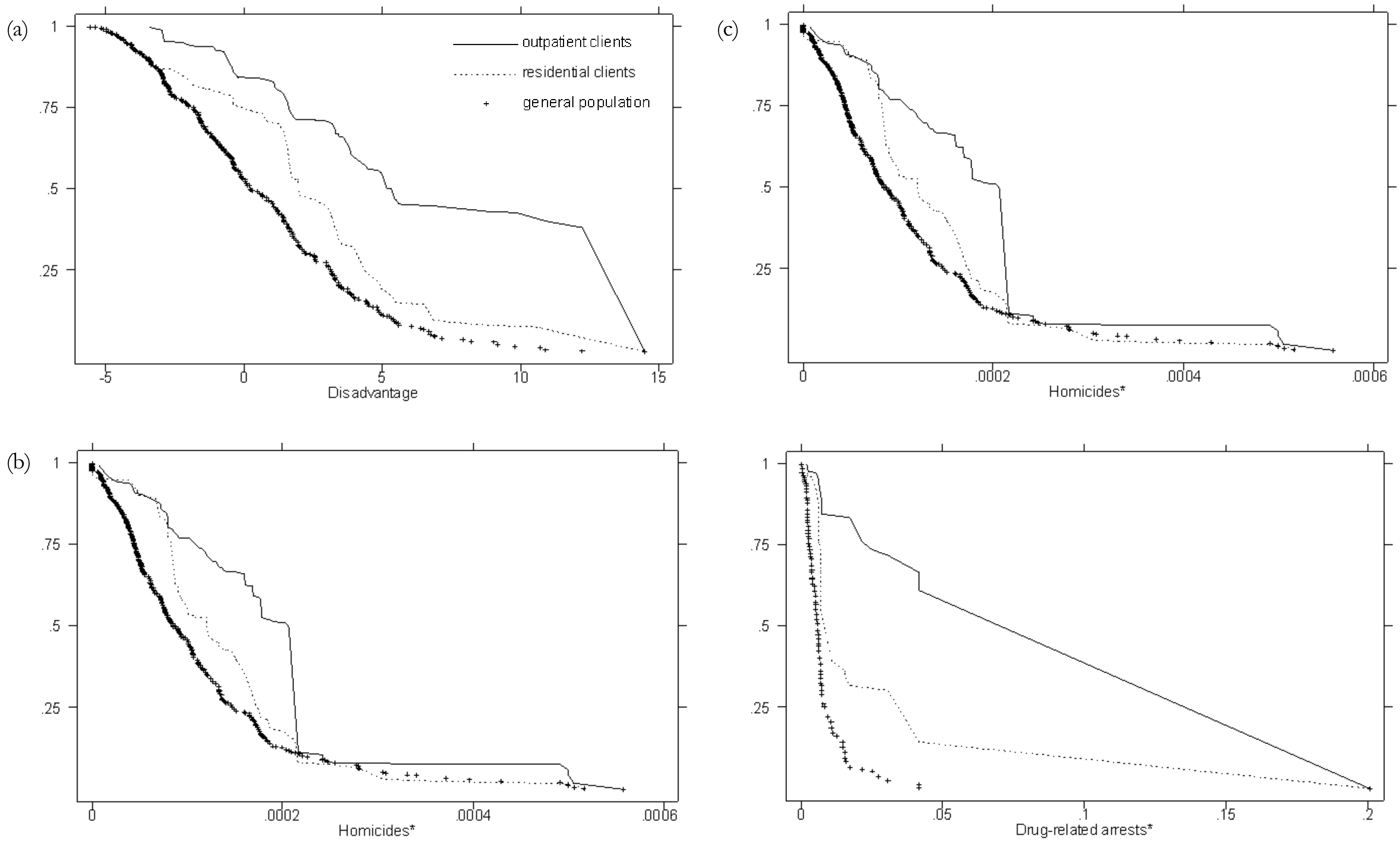

Figure 2-3 Distribution of contextual measures at treatment location, homeless clients, continued

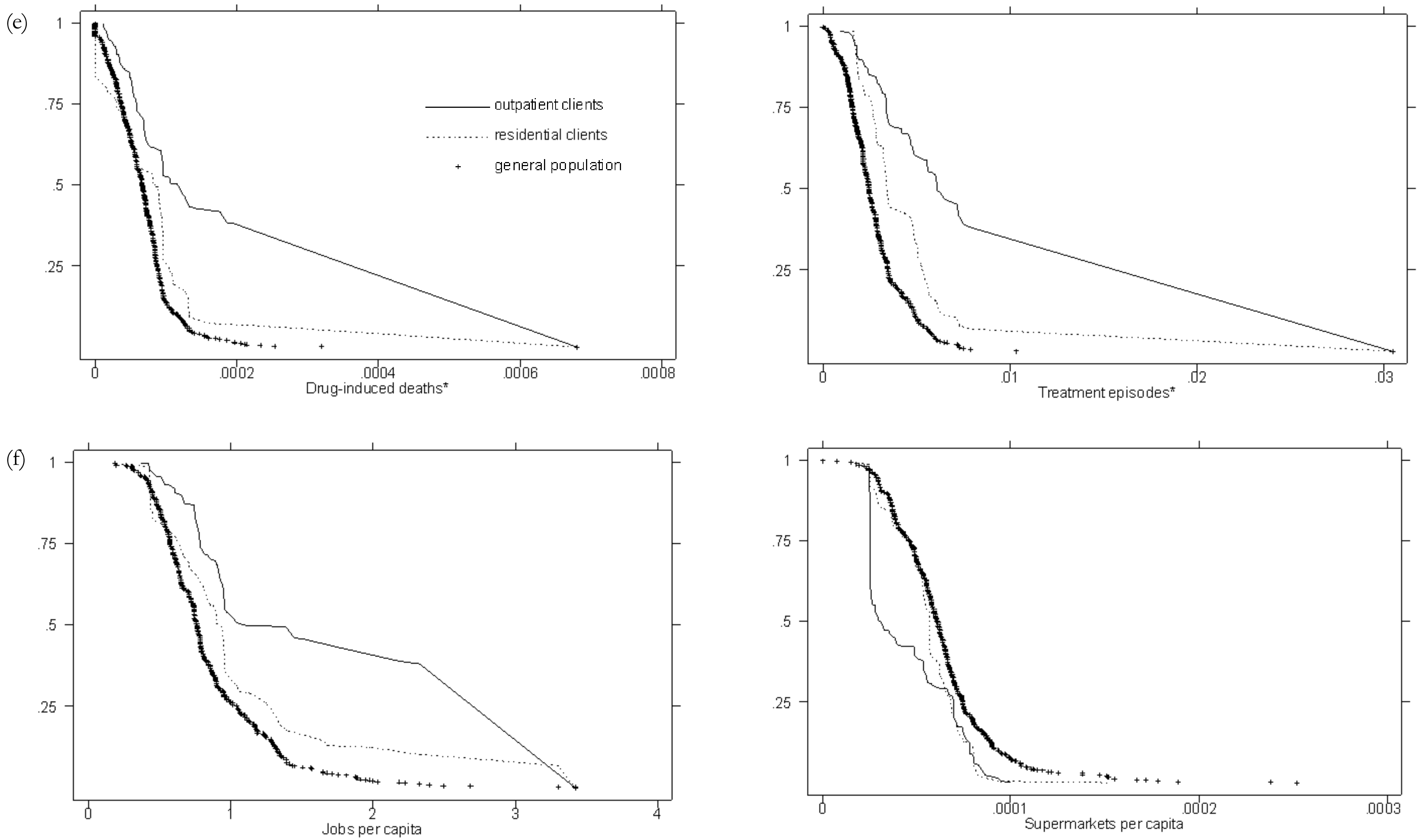

Table 2-4 Ratio of mean contextual measure for client residential locations relative to general population

	Neighborhood Measure									
	L.A. County							*L.A. City*		
Client Stratum	N	Disadv	Homic.	Drug Deaths	Treatment Episodes	Jobs	Mkts	N	Calls	Drug Arrests
Modality										
Outpatient	14001	3.5 (0.04)	1.5 (0.01)	1.2 (0.01)	1.4 (0.01)	1.0 (0.00)	0.9 (0.00)	4361	1.2 (0.01)	1.7 (0.05)
Residential	5684	3.2 (0.07)	1.4 (0.01)	1.3 (0.02)	1.5 (0.02)	*1.0* (0.01)	0.9 (0.01)	1822	1.3 (0.02)	2.2 (0.11)
Meth. Maint.	1867	3.7 (0.11)	1.4 (0.02)	1.3 (0.02)	1.5 (0.03)	1.0 (0.01)	0.9 (0.01)	664	1.3 (0.03)	2.0 (0.15)
Sex										
Female	9028	3.9 (0.05)	1.5 (0.01)	1.3 (0.01)	1.5 (0.01)	1.0 (0.01)	0.9 (0.00)	2725	1.3 (0.01)	1.8 (0.07)
Male	12524	3.1 (0.04)	1.4 (0.01)	1.2 (0.01)	1.4 (0.01)	1.0 (0.00)	0.9 (0.00)	4122	1.2 (0.01)	1.9 (0.06)
Age										
Adult	19197	3.6 (0.04)	1.5 (0.01)	1.3 (0.01)	1.5 (0.01)	1.0 (0.00)	0.9 (0.00)	6081	1.3 (0.01)	2.0 (0.05)
Juvenile	2355	1.7 (0.08)	1.1 (0.02)	1.0 (0.01)	1.2 (0.01)	0.9 (0.01)	1.0 (0.01)	766	1.1 (0.01)	1.1 (0.04)
Race, Ethnicity										
Asian / Pac. Isl.	394	2.7 (0.25)	1.2 (0.04)	1.1 (0.04)	1.3 (0.04)	*1.0* (0.02)	0.9 (0.02)	129	1.1 (0.04)	*1.3* (0.21)
Black	7355	6.6 (0.06)	2.2 (0.02)	1.4 (0.02)	1.8 (0.02)	0.9 (0.01)	0.7 (0.00)	2495	1.6 (0.02)	2.7 (0.10)
Hispanic	6878	2.8 (0.05)	1.2 (0.01)	1.1 (0.01)	1.3 (0.01)	1.0 (0.01)	0.9 (0.00)	2116	1.1 (0.01)	1.5 (0.05)
Native American	167	2.6 (0.35)	1.2 (0.07)	1.2 (0.11)	1.4 (0.11)	*1.0* (0.05)	0.9 (0.03)	60	1.3 (0.11)	2.3 (0.66)
White	6407	0.6 (0.05)	0.8 (0.01)	1.2 (0.01)	1.3 (0.01)	1.0 (0.01)	1.1 (0.01)	1907	1.0 (0.01)	1.3 (0.06)
Other	351	1.6 (0.24)	*1.0* (0.04)	1.1 (0.04)	1.2 (0.04)	1.1 (0.03)	1.1 (0.02)	140	*1.0* (0.04)	*1.3* (0.19)
Education										
< 12yrs	9998	3.6 (0.05)	1.4 (0.01)	1.2 (0.01)	1.4 (0.01)	1.0 (0.01)	0.9 (0.00)	3168	1.2 (0.01)	1.7 (0.06)
= 12 yrs	11554	3.3 (0.05)	1.4 (0.01)	1.3 (0.01)	1.5 (0.01)	1.0 (0.01)	0.9 (0.00)	3679	1.3 (0.01)	2.0 (0.07)
Drug problem										
Marijuana only	1208	3.9 (0.14)	1.6 (0.03)	1.1 (0.02)	1.4 (0.02)	0.9 (0.01)	0.9 (0.01)	390	1.2 (0.02)	1.3 (0.08)
Other drug only	4718	3.8 (0.07)	1.5 (0.02)	1.2 (0.01)	1.5 (0.01)	1.0 (0.01)	0.9 (0.01)	1564	1.3 (0.02)	1.7 (0.08)
Polydrug	15626	3.3 (0.04)	1.4 (0.01)	1.3 (0.01)	1.5 (0.01)	1.0 (0.00)	0.9 (0.00)	4893	1.3 (0.01)	1.9 (0.06)
Severity of use										
Less than daily use	11894	3.4 (0.05)	1.4 (0.01)	1.2 (0.01)	1.4 (0.01)	1.0 (0.00)	0.9 (0.00)	3774	1.3 (0.01)	1.9 (0.06)
Daily use	9658	3.5 (0.05)	1.5 (0.01)	1.3 (0.01)	1.5 (0.01)	1.0 (0.01)	0.9 (0.00)	3073	1.3 (0.01)	1.9 (0.07)
Non-injection user	17224	3.6 (0.04)	1.5 (0.01)	1.2 (0.01)	1.5 (0.01)	1.0 (0.00)	0.9 (0.00)	5349	1.3 (0.01)	1.9 (0.05)
Injection user	4328	2.8 (0.07)	1.2 (0.01)	1.3 (0.02)	1.4 (0.02)	1.0 (0.01)	0.9 (0.01)	1498	1.2 (0.02)	1.8 (0.09)
Prior treatment										
None	10204	3.2 (0.05)	1.4 (0.01)	1.2 (0.01)	1.4 (0.01)	0.9 (0.00)	0.9 (0.00)	3010	1.2 (0.01)	1.6 (0.06)
1 prior episode	5356	3.6 (0.07)	1.5 (0.02)	1.2 (0.01)	1.5 (0.02)	1.0 (0.01)	0.9 (0.01)	1738	1.3 (0.02)	1.9 (0.09)
2 or more	5992	3.7 (0.07)	1.5 (0.01)	1.3 (0.02)	1.5 (0.02)	*1.0* (0.01)	0.9 (0.01)	2099	1.3 (0.02)	2.2 (0.10)
Employment										
Not employed	18115	3.7 (0.04)	1.5 (0.01)	1.3 (0.01)	1.5 (0.01)	1.0 (0.00)	0.9 (0.00)	5765	1.3 (0.01)	2.0 (0.05)
Employed	3437	1.8 (0.08)	1.1 (0.02)	1.2 (0.01)	1.3 (0.01)	1.0 (0.01)	1.0 (0.01)	1082	1.1 (0.01)	1.2 (0.04)
Drug diagnosis										
No mental illness	19961	3.5 (0.04)	1.5 (0.01)	1.2 (0.01)	1.5 (0.01)	1.0 (0.00)	0.9 (0.00)	6267	1.3 (0.01)	1.9 (0.05)
Mental illness	1591	3.0 (0.13)	1.3 (0.03)	1.3 (0.03)	1.5 (0.03)	*1.0* (0.01)	1.0 (0.01)	580	1.3 (0.03)	2.0 (0.17)
Source of referral										
Not criminal justice	14223	3.8 (0.04)	1.5 (0.01)	1.3 (0.01)	1.5 (0.01)	1.0 (0.00)	0.9 (0.00)	4733	1.3 (0.01)	2.1 (0.06)

	Neighborhood Measure									
	L.A. County							L.A. City		
Client Stratum	N	Disadv	Homic.	Drug Deaths	Treatment Episodes	Jobs	Mkts	N	Calls	Drug Arrests
Non-injection user	17224	3.6 (0.04)	1.5 (0.01)	1.2 (0.01)	1.5 (0.01)	1.0 (0.00)	0.9 (0.00)	5349	1.3 (0.01)	1.9 (0.05)
Injection user	4328	2.8 (0.07)	1.2 (0.01)	1.3 (0.02)	1.4 (0.02)	1.0 (0.01)	0.9 (0.01)	1498	1.2 (0.02)	1.8 (0.09)
Prior treatment										
None	10204	3.2 (0.05)	1.4 (0.01)	1.2 (0.01)	1.4 (0.01)	0.9 (0.00)	0.9 (0.00)	3010	1.2 (0.01)	1.6 (0.06)
1 prior episode	5356	3.6 (0.07)	1.5 (0.02)	1.2 (0.01)	1.5 (0.02)	1.0 (0.01)	0.9 (0.01)	1738	1.3 (0.02)	1.9 (0.09)
2 or more	5992	3.7 (0.07)	1.5 (0.01)	1.3 (0.02)	1.5 (0.02)	*1.0 (0.01)*	0.9 (0.01)	2099	1.3 (0.02)	2.2 (0.10)
Employment										
Not employed	18115	3.7 (0.04)	1.5 (0.01)	1.3 (0.01)	1.5 (0.01)	1.0 (0.00)	0.9 (0.00)	5765	1.3 (0.01)	2.0 (0.05)
Employed	3437	1.8 (0.08)	1.1 (0.02)	1.2 (0.01)	1.3 (0.01)	1.0 (0.01)	1.0 (0.01)	1082	1.1 (0.01)	1.2 (0.04)
Drug diagnosis										
No mental illness	19961	3.5 (0.04)	1.5 (0.01)	1.2 (0.01)	1.5 (0.01)	1.0 (0.00)	0.9 (0.00)	6267	1.3 (0.01)	1.9 (0.05)
Mental illness	1591	3.0 (0.13)	1.3 (0.03)	1.3 (0.03)	1.5 (0.03)	*1.0 (0.01)*	1.0 (0.01)	580	1.3 (0.03)	2.0 (0.17)
Source of referral										
Not criminal justice	14223	3.8 (0.04)	1.5 (0.01)	1.3 (0.01)	1.5 (0.01)	1.0 (0.00)	0.9 (0.00)	4733	1.3 (0.01)	2.1 (0.06)
Criminal justice	7329	2.7 (0.05)	1.3 (0.01)	1.2 (0.01)	1.4 (0.01)	0.9 (0.01)	0.9 (0.00)	2114	1.1 (0.01)	1.4 (0.06)
All Clients	21552	3.4 (0.03)	1.4 (0.01)	1.2 (0.01)	1.5 (0.01)	1.0 (0.00)	0.9 (0.00)	6847	1.3 (0.01)	1.9 (0.05)

Cells report the ratio of the mean value for clients in the stratum divided by the mean for the general population. The difference in means is significant at the 5% level for all estimates except those that appear in italics, based on a two-tailed t-test. Standard error of the ratio in parentheses.

[a] *Outpatient* and *Residential* exclude methadone maintenance; *Meth. Maint.* is outpatient methadone maintenance

Table 2-5 Ratio of mean contextual measure for client treatment locations relative to general population

	Neighborhood Measure									
	L.A. County							L.A. City		
Client Stratum	N	Disadv	Homic.	Drug Deaths	Treatment Episodes	Jobs	Mkts	N	Calls	Drug Arrests
Modality										
Outpatient	16656	4.3 (0.04)	1.5 (0.01)	1.7 (0.02)	2.0 (0.02)	1.2 (0.01)	0.9 (0.00)	5625	1.9 (0.02)	5.7 (0.11)
Residential	9558	3.2 (0.06)	1.3 (0.01)	1.3 (0.02)	1.8 (0.02)	1.1 (0.01)	0.9 (0.00)	3747	1.5 (0.02)	3.6 (0.11)
Meth. Maint.	2034	4.0 (0.08)	1.4 (0.02)	1.2 (0.01)	1.4 (0.01)	1.1 (0.01)	0.8 (0.01)	958	1.2 (0.01)	1.9 (0.06)
Sex										
Female	11658	4.2 (0.05)	1.5 (0.01)	1.5 (0.02)	1.8 (0.02)	1.1 (0.01)	0.8 (0.00)	3947	1.6 (0.02)	3.9 (0.11)
Male	16590	3.7 (0.04)	1.4 (0.01)	1.6 (0.02)	1.9 (0.02)	1.2 (0.01)	0.9 (0.00)	6383	1.8 (0.02)	5.0 (0.10)
Age										
Adult	25662	4.2 (0.04)	1.4 (0.01)	1.6 (0.01)	1.9 (0.01)	1.2 (0.01)	0.9 (0.00)	9496	1.7 (0.01)	4.8 (0.08)
Juvenile	2586	1.4 (0.06)	1.0 (0.01)	1.1 (0.01)	1.2 (0.01)	0.9 (0.01)	1.1 (0.00)	834	1.1 (0.01)	1.6 (0.06)
Race, Ethnicity										
Asian / Pac. Isl.	474	3.6 (0.20)	1.4 (0.04)	1.3 (0.05)	1.6 (0.06)	*1.0 (0.03)*	0.9 (0.02)	145	1.4 (0.07)	2.7 (0.39)
Black	10355	6.5 (0.06)	1.9 (0.01)	2.0 (0.03)	2.5 (0.03)	1.3 (0.01)	0.7 (0.00)	4407	2.1 (0.02)	6.8 (0.14)
Hispanic	8234	2.9 (0.05)	1.2 (0.01)	1.2 (0.01)	1.5 (0.01)	1.1 (0.01)	0.9 (0.00)	2706	1.3 (0.02)	2.9 (0.10)
Native American	217	3.1 (0.34)	1.2 (0.05)	1.5 (0.13)	1.8 (0.14)	1.2 (0.05)	0.9 (0.02)	87	1.7 (0.12)	4.4 (0.73)
White	8388	1.7 (0.05)	1.0 (0.01)	1.2 (0.02)	1.6 (0.02)	1.0 (0.01)	1.0 (0.00)	2673	1.4 (0.02)	2.9 (0.10)

Client Stratum	N	Disadv	Homic.	Drug Deaths	Treatment Episodes	Jobs	Mkts	N	Calls	Drug Arrests
	Neighborhood Measure									
	L.A. County							*L.A. City*		
Other	580	2.8 (0.16)	1.2 (0.02)	1.3 (0.04)	1.6 (0.04)	1.1 (0.02)	1.0 (0.01)	312	1.3 (0.03)	3.6 (0.17)
Education										
< 12yrs	12590	3.7 (0.05)	1.4 (0.01)	1.4 (0.01)	1.7 (0.02)	1.1 (0.01)	0.9 (0.00)	4381	1.5 (0.02)	3.8 (0.10)
= 12 yrs	15658	4.1 (0.05)	1.4 (0.01)	1.6 (0.02)	2.0 (0.02)	1.2 (0.01)	0.9 (0.00)	5949	1.8 (0.02)	5.2 (0.10)
Drug problem										
Marijuana only	1319	3.4 (0.13)	1.5 (0.03)	1.2 (0.03)	1.5 (0.03)	0.9 (0.01)	0.9 (0.01)	402	1.3 (0.03)	2.2 (0.17)
Other drug only	5729	4.0 (0.07)	1.5 (0.01)	1.4 (0.02)	1.8 (0.02)	1.1 (0.01)	0.9 (0.00)	2158	1.5 (0.02)	3.7 (0.14)
Polydrug	21200	3.9 (0.04)	1.4 (0.01)	1.6 (0.01)	1.9 (0.01)	1.2 (0.01)	0.9 (0.00)	7770	1.8 (0.01)	4.9 (0.09)
Severity of use										
Less than daily use	14220	3.6 (0.04)	1.4 (0.01)	1.3 (0.01)	1.6 (0.01)	1.1 (0.01)	0.9 (0.00)	4849	1.5 (0.01)	3.1 (0.08)
Daily use	14028	4.3 (0.05)	1.4 (0.01)	1.7 (0.02)	2.1 (0.02)	1.3 (0.01)	0.8 (0.00)	5481	1.9 (0.02)	5.9 (0.12)
Non-injection user	22344	4.0 (0.04)	1.4 (0.01)	1.5 (0.01)	1.9 (0.01)	1.1 (0.01)	0.9 (0.00)	7872	1.7 (0.01)	4.5 (0.08)
Injection user	5904	3.7 (0.07)	1.3 (0.01)	1.6 (0.03)	2.0 (0.03)	1.2 (0.01)	0.9 (0.00)	2458	1.7 (0.03)	4.7 (0.15)
Prior treatment										
None	12402	3.1 (0.04)	1.3 (0.01)	1.3 (0.01)	1.6 (0.01)	1.0 (0.01)	0.9 (0.00)	3769	1.5 (0.02)	3.0 (0.09)
1 prior episode	7209	4.3 (0.07)	1.4 (0.01)	1.6 (0.03)	2.0 (0.03)	1.2 (0.01)	0.9 (0.00)	2776	1.8 (0.02)	5.1 (0.15)
2 or more	8637	4.7 (0.06)	1.5 (0.01)	1.8 (0.03)	2.2 (0.03)	1.3 (0.01)	0.8 (0.00)	3785	1.9 (0.02)	5.7 (0.14)
Employment										
Not employed	24475	4.2 (0.04)	1.4 (0.01)	1.6 (0.01)	1.9 (0.01)	1.2 (0.01)	0.9 (0.00)	9081	1.7 (0.01)	4.9 (0.08)
Employed	3773	2.4 (0.07)	1.2 (0.01)	1.3 (0.02)	1.5 (0.02)	1.0 (0.01)	1.0 (0.01)	1249	1.4 (0.02)	2.3 (0.09)
Dual diagnosis										
No mental illness	26104	3.9 (0.03)	1.4 (0.01)	1.5 (0.01)	1.9 (0.01)	1.1 (0.00)	0.9 (0.00)	9395	1.7 (0.01)	4.4 (0.08)
Mental illness	2144	4.0 (0.13)	1.3 (0.02)	1.9 (0.05)	2.2 (0.06)	1.3 (0.02)	0.9 (0.01)	935	2.0 (0.04)	6.1 (0.28)
Source of referral										
Not criminal justice	19919	4.5 (0.04)	1.5 (0.01)	1.7 (0.02)	2.1 (0.02)	1.2 (0.01)	0.9 (0.00)	7868	1.8 (0.01)	5.5 (0.09)
Criminal justice	8329	2.6 (0.05)	1.3 (0.01)	1.1 (0.01)	1.5 (0.01)	*1.0* (0.01)	0.9 (0.00)	2462	1.2 (0.01)	1.7 (0.05)
Homeless status										
Not homeless	21537	3.3 (0.03)	1.4 (0.01)	1.2 (0.01)	1.5 (0.01)	*1.0* (0.00)	0.9 (0.00)	6781	1.3 (0.01)	2.2 (0.04)
Homeless	6711	5.7 (0.09)	1.4 (0.01)	2.6 (0.04)	3.1 (0.04)	1.6 (0.01)	0.8 (0.00)	3549	2.4 (0.03)	9.2 (0.17)
All Clients	28248	3.9 (0.03)	1.4 (0.01)	1.5 (0.01)	1.9 (0.01)	1.2 (0.00)	0.9 (0.00)	10330	1.7 (0.01)	4.6 (0.07)

Cells report the ratio of the mean value for clients in the stratum divided by the mean for the general population. The difference in means is significant at the 5% level for all estimates except those that appear in italics, based on a two-tailed t-test. Standard error of the ratio in parentheses.

[a] *Outpatient* and *Residential* exclude methadone maintenance; *Meth. Maint.* is outpatient methadone maintenance

Table 2-6 Regression results for contextual measures at residential location by client strata

Independent Variable	Disadv	Homic.	Treatment Episodes	Jobs	Mkts	Calls	Drug Arrests
	Dependent Variables (standardized contextual measure)						
	L.A. County					*L.A. City*	
Modality							
Outpatient	***0.12 (0.02)	***0.11 (0.02)	**-0.01 (0)	***-0.01 (0)	***-0.03 (0.01)	0 (0)	***-0.01 (0)

	Dependent Variables (standardized contextual measure)						
	L.A. County					L.A. City	
Independent Variable	Disadv	Homic.	Treatment Episodes	Jobs	Mkts	Calls	Drug Arrests
Meth. Maint	***0.2 (0.03)	***0.16 (0.03)	0 (0)	*-0.01 (0.01)	***-0.1 (0.02)	0 (0)	-0.01 (0.01)
Residential†							
Sex							
Female	*0.02 (0.01)	**0.03 (0.02)	0 (0)	-0.01 (0)	***-0.03 (0.01)	***0 (0)	***-0.01 (0)
Male†							
Age							
Juvenile	***-0.37 (0.02)	***-0.34 (0.02)	***-0.03 (0)	**-0.01 (0.01)	***0.13 (0.01)	***-0.01 (0)	***-0.02 (0)
Adult†							
Race, Ethnicity							
Asian / Pac. Isl.	***0.46 (0.05)	***0.38 (0.05)	0 (0.01)	*0.02 (0.01)	***-0.24 (0.03)	0 (0)	0 (0.01)
Black	***1.23 (0.02)	***1.56 (0.02)	***0.06 (0)	***-0.02 (0)	***-0.54 (0.01)	***0.02 (0)	***0.04 (0)
Hispanic	***0.43 (0.02)	***0.41 (0.01)	*0 (0)	***0.04 (0)	***-0.26 (0.01)	***0 (0)	**0.01 (0)
Native Am.	***0.4 (0.07)	***0.4 (0.08)	0.02 (0.01)	0.03 (0.02)	***-0.25 (0.04)	**0.01 (0)	*0.04 (0.02)
Other	***0.2 (0.05)	***0.18 (0.04)	0 (0.01)	***0.05 (0.01)	-0.02 (0.04)	0 (0)	0 (0.01)
White†							
Education							
< 12yrs	***0.15 (0.01)	***0.13 (0.02)	**0 (0)	***-0.01 (0)	***-0.07 (0.01)	0 (0)	0 (0)
≥ 12 yrs†							
Drug Problem							
Marijuana only	0.03 (0.03)	0.03 (0.04)	0 (0)	0 (0.01)	0.01 (0.02)	0 (0)	-0.01 (0)
Polydrug	**-0.04 (0.02)	***-0.07 (0.02)	***0.01 (0)	***0.01 (0)	**0.02 (0.01)	0 (0)	***0.01 (0)
Other drug only†							
Severity of use							
Daily use	-0.01 (0.01)	-0.02 (0.02)	**0 (0)	0 (0)	**-0.02 (0.01)	***0 (0)	***-0.01 (0)
Less than daily†							
Injection user	*-0.04 (0.02)	***-0.07 (0.02)	0 (0)	**0.01 (0.01)	***0.04 (0.01)	0 (0)	0 (0)
Non-Injection user†							
Prior treatment							
1 prior episode	0.02 (0.02)	0.02 (0.02)	0 (0)	**0.01 (0)	-0.01 (0.01)	0 (0)	0.01 (0)
2 or more	-0.02 (0.02)	0.03 (0.02)	*0 (0)	***0.02 (0)	0 (0.01)	**0 (0)	***0.01 (0)
None†							
Employment							
Not employed	***0.27 (0.02)	***0.22 (0.02)	***0.02 (0)	0 (0)	***-0.11 (0.01)	***0.01 (0)	***0.02 (0)
Employed†							
Dual diagnosis							
Mental illness	***-0.07 (0.02)	***-0.14 (0.03)	0 (0)	0.01 (0.01)	***0.08 (0.02)	0 (0)	0 (0.01)
Not mental ill. †							
Source of referral							
Criminal justice	***-0.06 (0.01)	***-0.07 (0.02)	***-0.01 (0)	***-0.03 (0)	0 (0.01)	***0 (0)	***-0.01 (0)
Not crim just†							
Adjusted R^2	0.26	0.3	0.07	0.02	0.15	0.13	0.04
N	21552	21552	21552	21552	21552	6847	6847

Cells report the coefficient for a robust regression, standard error of the coefficient, and significance of a t-test that the coefficient is different from 0.
† Reference group
* p-value ≤ 0.10, ** p-value ≤ 0.05, *** p-value ≤ 0.01
[a] *Outpatient* and *Residential* exclude methadone maintenance; *Meth. Maint.* is outpatient methadone maintenance

Table 2-7 Regression results for contextual measures at treatment location by client strata

	Dependent Variable (standardized contextual measure)						
	L.A. County					*L.A. City*	
Independent Variable	Disadv	Homic.	Treatment Episodes	Jobs	Mkts	Calls	Drug Arrests
Modality							
Outpatient	***0.57 (0.02)	***0.22 (0.02)	***0.13 (0)	***0.15 (0.01)	***-0.12 (0.01)	***0.05 (0)	***0.2 (0.01)
Meth. Maint	***0.31 (0.03)	***0.16 (0.03)	***-0.01 (0)	***0.03 (0.01)	***-0.1 (0.01)	0 (0)	0.01 (0.01)
Residential†							
Sex							
Female	*0.02 (0.01)	0.01 (0.01)	***-0.02 (0)	***-0.04 (0)	***-0.05 (0.01)	***-0.01 (0)	***-0.03 (0)
Male†							
Age							
Juvenile	***-0.35 (0.02)	***-0.43 (0.02)	***-0.01 (0)	***-0.02 (0)	***0.26 (0.01)	***-0.01 (0)	***-0.02 (0.01)
Adult†							
Race, Ethnicity							
Asian / Pac. Isl.	***0.48 (0.04)	***0.5 (0.04)	***0.03 (0.01)	*0.02 (0.01)	***-0.28 (0.03)	0 (0)	**0.03 (0.01)
Black	***0.92 (0.02)	***1.01 (0.02)	***0.09 (0)	***0.07 (0)	***-0.4 (0.01)	***0.03 (0)	***0.1 (0.01)
Hispanic	***0.27 (0.01)	***0.23 (0.01)	***0.01 (0)	***0.07 (0)	***-0.17 (0.01)	***0 (0)	***0.04 (0)
Native Am.	***0.23 (0.07)	***0.21 (0.06)	0.02 (0.02)	**0.05 (0.02)	***-0.13 (0.04)	**0.01 (0.01)	*0.05 (0.02)
Other	***0.17 (0.03)	***0.24 (0.03)	***-0.02 (0.01)	***-0.03 (0.01)	***-0.06 (0.02)	***-0.01 (0)	***-0.02 (0.01)
White†							
Education							
< 12yrs	***0.06 (0.01)	***0.09 (0.01)	***-0.01 (0)	***-0.02 (0)	***-0.05 (0.01)	***0 (0)	**-0.01 (0)
≥ 12 yrs†							
Drug Problem							
Marijuana only	-0.05 (0.03)	***0.12 (0.04)	***-0.03 (0)	***-0.04 (0.01)	0.02 (0.02)	***-0.01 (0)	***-0.04 (0.01)
Polydrug	-0.01 (0.02)	**-0.04 (0.02)	0 (0)	***0.02 (0)	*0.01 (0.01)	0 (0)	0 (0.01)
Other drug only†							
Severity of use							
Daily use	***0.11 (0.01)	***-0.04 (0.01)	***0.05 (0)	***0.06 (0)	***-0.07 (0.01)	***0.01 (0)	***0.07 (0)
Less than daily†							
Injection user	***0.06 (0.02)	0 (0.02)	***0.04 (0)	***0.04 (0.01)	**-0.02 (0.01)	***0.01 (0)	***0.04 (0.01)
Non-Injection user†							
Prior treatment							
1 prior episode	***0.12 (0.02)	***0.05 (0.02)	***0.03 (0)	***0.03 (0)	***-0.05 (0.01)	***0.01 (0)	***0.04 (0.01)
2 or more	***0.18 (0.02)	***0.11 (0.02)	***0.05 (0)	***0.06 (0.01)	***-0.06 (0.01)	***0.01 (0)	***0.05 (0.01)
None†							

	Dependent Variable (standardized contextual measure)						
	L.A. County					*L.A. City*	
Independent Variable	Disadv	Homic.	Treatment Episodes	Jobs	Mkts	Calls	Drug Arrests
Employment							
Not employed	***0.24 (0.02)	***0.22 (0.02)	***0.02 (0)	***0.01 (0)	***-0.13 (0.01)	***0.01 (0)	***0.04 (0)
Employed†							
Dual diagnosis							
Mental illness	***-0.11 (0.03)	***-0.14 (0.02)	*0.01 (0.01)	**0.02 (0.01)	***0.11 (0.01)	0 (0)	0.01 (0.01)
Not mental ill.†							
Source of referral							
Criminal justice	***-0.19 (0.01)	***-0.12 (0.01)	***-0.03 (0)	***-0.05 (0)	0.01 (0.01)	***-0.02 (0)	***-0.07 (0)
Not crim just†							
Homelessness							
Homeless	***0.52 (0.02)	***-0.13 (0.02)	***0.19 (0.01)	***0.3 (0.01)	***-0.08 (0.01)	***0.04 (0)	***0.23 (0.01)
Not homeless†							
Adjusted R^2	0.22	0.19	0.2	0.18	0.17	0.36	0.37
N	28249	28249	28249	28249	28249	10330	10330

Cells report the coefficient for a robust regression, standard error of the coefficient, and significance of a t-test that the coefficient is different from 0.

† Reference group

* p-value ≤ 0.10, ** p-value ≤ 0.05, *** p-value ≤ 0.01

[a] *Outpatient* and *Residential* exclude methadone maintenance; *Meth. Maint.* is outpatient methadone maintenance

Figure 2-4 Expected distance from client's zip code to the treatment facility

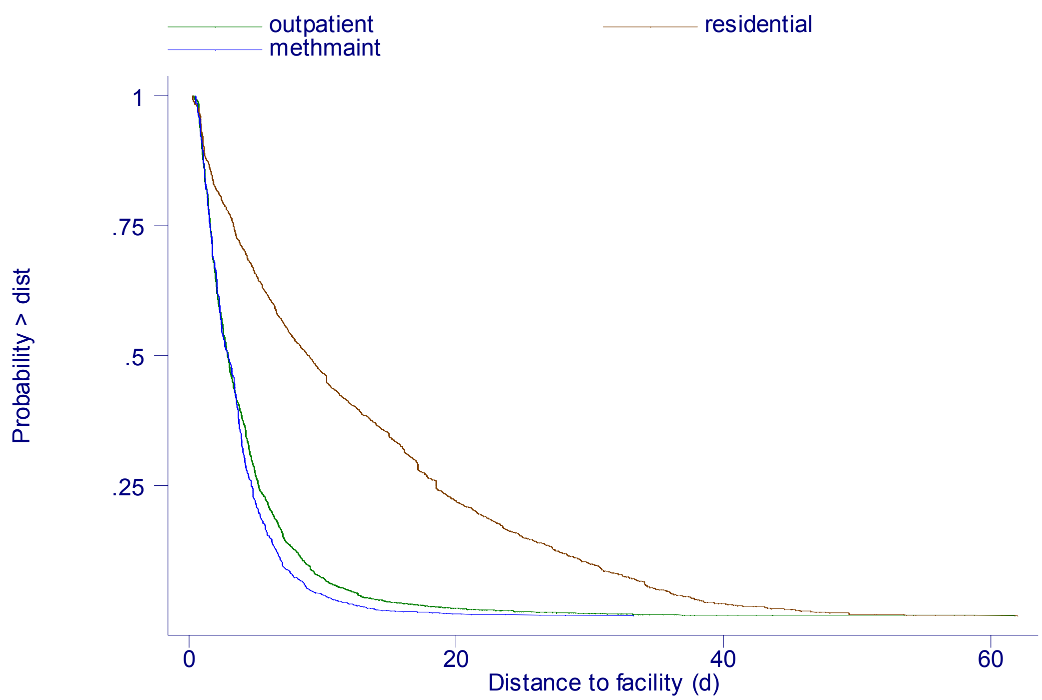

Table 2-8 Regression results for expected distance from client's zip code to treatment facility

	Dependent Variable is Log Expected Distance (miles)		
Independent Variable	**Outpatient**	**Residential**	**Meth. Maint.**
Sex			
Female	0.03 (0.08)	**-0.09 (0.16)	-0.15 (0.28)
Male†			
Age			
Juvenile	***-0.13 (0.06)	***-0.39 (0.13)	-0.14 (0.18)
Adult†			
Race, Ethnicity			
Asian / Pac. Isl.	-0.13 (0.02)	-0.11 (0.04)	-0.04 (0.05)
Black	***-0.09 (0.08)	***-0.24 (0.2)	*0.31 (0.14)
Hispanic	***-0.08 (0.06)	***-0.02 (0.13)	0.06 (0.18)
Native Am.	-0.05 (0.02)	-0.12 (0.04)	**0.03 (0.04)
Other	-0.07 (0.03)	0.03 (0.14)	-1.39 (0.14)
White†			
Education			
< 12yrs	***-0.01 (0.02)	***-0.22 (0.04)	-0.05 (0.04)
≥ 12 yrs†			
Drug Problem			
Marijuana only	**0.06 (0.02)	0.16 (0.04)	***-0.06 (0.06)
Polydrug	-0.03 (0.02)	***0 (0.05)	-0.19 (0.1)
Other drug only†			
Severity of use			
Daily use	***-0.05 (0.02)	***-0.03 (0.04)	-0.02 (0.09)
Less than daily†			
Injection user	-0.03 (0.02)	-0.11 (0.04)	*0.01 (0.08)
Non-Injection user†			
Prior treatment			
1 prior episode	***-0.13 (0.02)	-0.25 (0.08)	-0.04 (0.06)
2 or more	0.16 (0.03)	**-0.02 (0.07)	0 (0.08)
None†			
Employment			
Not employed	***0.12 (0.01)	***0.19 (0.04)	0 (0)
Employed†			
Dual diagnosis			
Mental illness	***1.18 (0.06)	2.47 (0.15)	1.38 (0.22)
Not mental ill. †			
Source of referral			
Criminal justice	***0 (0)	***0 (0)	0 (0)
Not crim just†			
Adjusted R^2	0.03	0.08	0.00
N	13949	4710	1867

† Reference group

* p-value ≤ 0.10, ** p-value ≤ 0.05, *** p-value ≤ 0.01

Figure 2-5 Percent of clients from one neighborhood who receive treatment in another

(a) Disadvantage	Quartile	Treatment Neighborhood 1	2	3	4	Total	N
	1	0.11	0.31	0.29	0.29	100	1233
Residential	2	0.07	0.29	0.36	0.27	100	3067
Neighborhood	3	0.05	0.13	0.56	0.27	100	5695
	4	0.06	0.05	0.13	0.76	100	10531

(b) Drug Arrests	Quartile	Treatment Neighborhood 1	2	3	4	Total	N
	1	0.30	0.02	0.28	0.40	100	445
Residential	2	0.29	0.04	0.42	0.26	100	769
Neighborhood	3	0.09	0.01	0.56	0.34	100	1402
	4	0.05	0.01	0.26	0.68	100	1987

Table 2-9 Percent of clients from one neighborhood who receive treatment in another

	Compared to residential zip, treatment zip is							
	(clients living in "worst" half of zips)				(clients living in "best" half of zips)			
Contextual Measure	N	About the same*	Better**	Worse**	N	About the same*	Better**	Worse**
Disadvantage	16226	0.76	0.17	0.07	4300	0.65	0.00	0.35
Jobs	11654	0.86	0.14	0.00	8872	0.74	0.06	0.20
Markets	13311	0.89	0.11	0.00	7215	0.81	0.00	0.19
Homicides	16333	0.78	0.15	0.07	4193	0.78	0.00	0.22
Treatment episodes	15995	0.77	0.13	0.10	4531	0.68	0.00	0.32
Drug deaths	12461	0.79	0.15	0.05	8065	0.83	0.01	0.16
Drug arrests	3389	0.81	0.05	0.14	1214	0.78	0.00	0.22
Calls	3077	0.75	0.12	0.13	1526	0.81	0.00	0.19

* within one standard deviation; ** better/worse by more than one standard deviation

VII Discussion

This report has produced the first population-level estimates characterizing the environments where drug treatment clients in publicly-funded and community-based recovery programs live and receive treatment. Five contextual factors were examined: neighborhood disadvantage, drug availability, social stressors, proximity to jobs, and proximity to retail

services. We summarized how clients' home and treatment neighborhoods differ in terms of these factors, as well as their separation in terms of geographic distance, and then charted the extent to which clients who live in one kind of neighborhood travel to treatment in another.

Our findings indicate that treatment clients in Los Angeles County are found in residential neighborhoods that are markedly different than those of the general household population, with significantly higher rates of poverty and disadvantage, violence and victimization, and drug availability (as measured by drug arrests, drug-related deaths, and participation in drug treatment by neighborhood residents). For example, the average treatment client resides in a neighborhood with a rate of disadvantage more than triple the rate of the average county resident, and with a rate of violence and victimization 30 to 40 percent higher. Treatment locations follow a similar pattern, but with even starker differences.

These results hold across virtually all client subgroups typically examined in the literature, but some clients face significantly worse environments than others at both the residential and treatment location. Among them are clients who typically face other social and economic disadvantages: racial and ethnic minorities, less educated clients, and clients who are not employed. Homeless clients and black clients receive treatment in the worst locations with respect to all measures. White clients are the one group found to reside in areas of the county better than the general county population average, though their treatment environments are notably worse, similar to those of other race and ethnic groups.

This last finding suggests that the geographic distribution of treatment centers may be skewed toward less desirable neighborhoods. In fact, we find that while a large majority of clients travel to treatment sites that are similar to their home neighborhoods, from 16 to 35 percent of those clients who come from the best neighborhoods attend treatment in areas worse by a standard deviation or more. In contrast, a much smaller fraction travel from worse to better neighborhoods, providing some evidence of maldistribution in treatment center location. Conclusive evidence of maldistribution, however, would have to be based on a more direct analysis of the geographic distribution of treatment center supply and client demand that takes into account the degree of clustering among clients. That is, if clients in better neighborhoods are very dispersed, then it could be the case that relocating treatment centers to better neighborhoods would only increase the total distance traveled by all travel clients. Our findings regarding distance traveled indicate that 90% of outpatient clients live fairly close to treatment—within 9 miles—and 99% within 22 miles of the treatment site.

All of the measures reported, particularly the travel distance approximations, could be improved by employing data on clients' household locations rather than zip codes, which are relatively coarse units for contextual analysis. Future analyses that find such information available should incorporate both finer level locational information as well as self-report data on mode of transport in order to compute true measures of travel distance and time to improve the straight-line approximations applied here.

Finally, these findings confirm exposure of the treatment population to environmental factors that have been hypothesized to impact recovery, which motivates the need to improve understanding of the effect of these factors on treatment outcomes, and to investigate the prevalence of other potentially important contextual factors, such as local availability of self-help and mutual support groups. Given the unique geography and high levels of racial and economic residential segregation in Los Angeles, it remains to be seen whether the findings reported here can be replicated for treatment clients living or receiving treatment in other areas.

VIII Appendices

VIII.A Appendix 1: Precision of Rates Based on Rare Event Counts in Small Areas

This study uses counts of rare events—homicides, drug-induced deaths, and substance abuse treatment episodes—from administrative data to construct per capita rates as proxies for other measures—social stressors and drug availability—in each zip code. These rates must be viewed as estimates because the observation of counts in each zip code is taken over a finite time period, a number of years. In this section we consider the precision of these rates and how it responds to population size within each zip code and decisions regarding the choice of time horizon to compute the average. It is important to note beforehand that the homicide rate *per se* is of little interest for our purposes, other than for what it suggests about unobserved rates of violence and victimization more generally in an area, so that a count in any particular year would be inadequate, though it might be known with certainty from administrative data.

At least three options are available to improve the precision of an estimate of an average annual per capita rate in a zip code. We can average the estimate over a longer period of historical data, assuming a stationary process over the period included in the analysis. Additionally, we can cluster zip codes to eliminate those with small populations, or similarly, use a Bayesian model, such as a Poisson-Gamma (Congdon 2001), to adjust estimates in smaller zip codes by weighting toward values in more precisely estimated larger zip codes (i.e., "borrowing strength"). Both clustering and borrowing strength require the assumption that the underlying process is the same in all zip codes, or at least in neighboring zip codes.

In the present case, we prefer to take a longer history of data. Though we know of no study that has examined whether a neighborhood rates of violence at a point in time is more similar to rates in the same neighborhood in previous years than rates in contemporaneous, nearby neighborhoods, we suspect that the former is true, that over short time periods neighborhoods retain differences that distinguish them from their neighbors. More likely, there is some optimal combination of "same place, different time" and "nearby place, same time" data that would yield the closest approximation. An examination of this tradeoff would be informative, but we do not investigate it here. Below, we estimate how the precision might be improved by averaging the rare event count for each measure over a reasonably recent period, say, no more than five years since zip code boundaries change over time as do the composition of neighborhoods, economic and societal conditions, and so on.

Consider the simple model below, in which the count, y_t, of some rare event in a zip code in year *t* follows a Poisson distribution with a mean per capita rate of *λ*. The population in the zip code is *p*. To increase precision, we might consider including counts from *n*-1 prior years:

$$y_t \sim Poisson(\lambda p)$$

$$Y = \sum_{t=1}^{n} y_t$$

The mean, *λ*, is unobserved, but we observe Y ~Poisson(*nλp*) and estimate the average annual per capita rate as

$$\hat{\lambda} = \frac{Y}{np}.$$

The precision of this estimate, i.e., the probability that $\hat{\lambda}$ is within some percentage, *δ*, of *λ*, can be calculated from the density of *Y*:

$$\Pr\left\{\hat{\lambda} \in [\lambda(1-\delta), \lambda(1+\delta)]\right\} = \sum_{k=np\lambda(1-\delta)}^{np\lambda(1+\delta)} \frac{e^{-np\lambda}(np\lambda)^k}{k!} \qquad \text{(A1-1)}$$

defined for integer-valued *k*. As one would expect, the precision depends on the size of the zip code population, the time horizon of data included, and the true annual rate. To determine how the precision would respond to changes in *n*, a reference population and rate are needed.

About 98% of LACPRS discharges in fiscal year 1999-2000 involved clients who lived or received treatment in zip codes with 5000 or more residents, as shown in Figure 2-6, which plots the distribution of zip code population size (1-CDF) separately for clients' residential and treatment zip codes.[24] Fixing *p*=5000 is a reasonable starting point for evaluating A1-1 since precision will be worst in the smallest areas. As a starting point for λ, we use the observed countywide annual per capital average over the period 1998-2000. However, we exclude from the average zip codes with a zero count in all three years, to more closely approximate the average in zip codes where events do occur. The county homicide rate during this period was 1.1 per ten thousand residents (2883 homicides / (2 yrs * 9133515 people)).

For these values of λ and *p*, Figure 2-7 displays the precision of the estimate as a function of *δ* for n=2, 5, and 10 years of data. For all of these choices of *n*, there is a high probability of producing an estimate within a factor of 2 of the true value. Estimates that draw

on a 5 or 10-year history are likely to come within 100%, but even averaging over a 10-year span is unlikely yield high precision, say, within 25%. Notably, moving from 5 to 10 years of data improves precision only slightly and is probably not worth the loss in relevance of the measure to the timeframe of the study. For larger zip codes, the precision improves considerably. Figure 2-8 plots A1-1 versus population (for population $\geq$ 5000) with n fixed at 5 years. Again, even the median zip code (about 40000 residents) is unlikely to come within 25% of the true value, but most are likely to produce an estimate within 75% of it.

Finally, what level of statistical power would be achieved by including 5 years of data in the average? Although the measures constructed from these estimates are to be entered as covariates in a regression testing for differences in levels of attrition, it is worthwhile to examine a simple comparison of the rates themselves to build intuition. For the example of homicide rates, Figure 2-9 shows the likelihood of detecting a statistically significant difference in the annual per capita rate between two zip codes of the same population size, if one were at the county average of λ=1.1 per ten thousand and the other were to differ from the average by a factor of 0.5, 2, 3, or 4. The calculations are displayed for the range of zip code population in the Los Angeles data, and illustrate the power of a one-sided test assuming the Poisson distribution and a 5% confidence level. The figure shows that power is greatest for larger differences, of course, but for the same relative difference (e.g., 2λ vs. 0.5λ), the test is less powerful if both rates are smaller in magnitude. Thus, as statistical controls, the estimates are unlikely to differentiate well among zip codes with very low rates of homicide. However, large differences will be detected reasonably well. For example, a comparison of Figure 2-6 and Figure 2-9 suggests that most clients in the sample are in neighborhoods with 25000 or more residents, and that at this population size, including 5 years of data to compute the estimate will yield power sufficient to detect differences between a zip code whose true rate is at the county mean and one with a relatively high rate of homicide (e.g., two times greater than the county mean).

Compared to homicides, the rate of drug-induced deaths is slightly lower at seven per ten thousand residents and roughly equivalent precision with a five-year time span. The rate of drug treatment episodes in the county is considerably higher at 28 per ten thousand, so that a

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[24] The curve is constructed from counts of discharges, the unit of analysis in this study, rather than clients.

slightly better level of precision compared to homicides can be achieved with just two years of data (1999-2000).

Figure 2-6 Population size of clients' residential and treatment zip codes

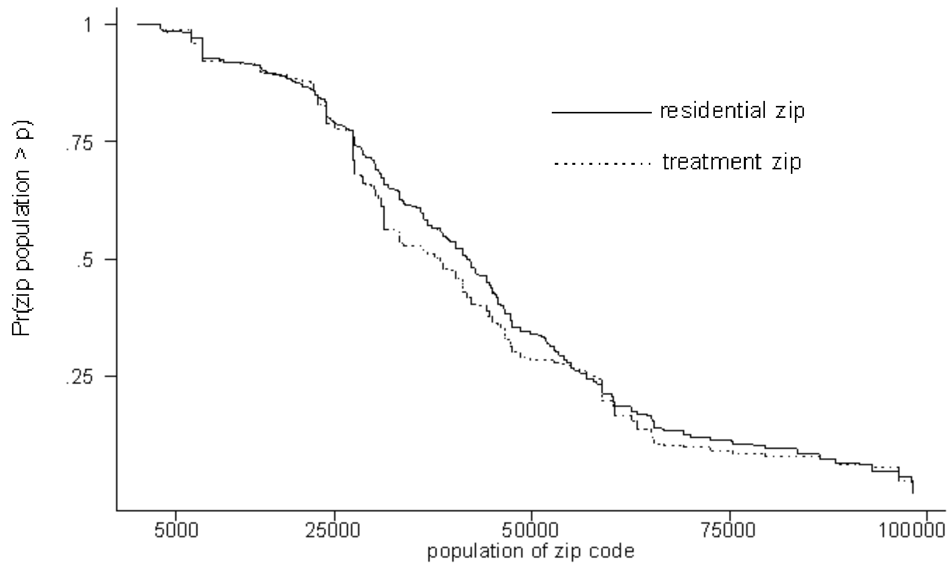


Figure 2-7 Precision of estimates of a zip code's homicide rate as a function of the time horizon of the data

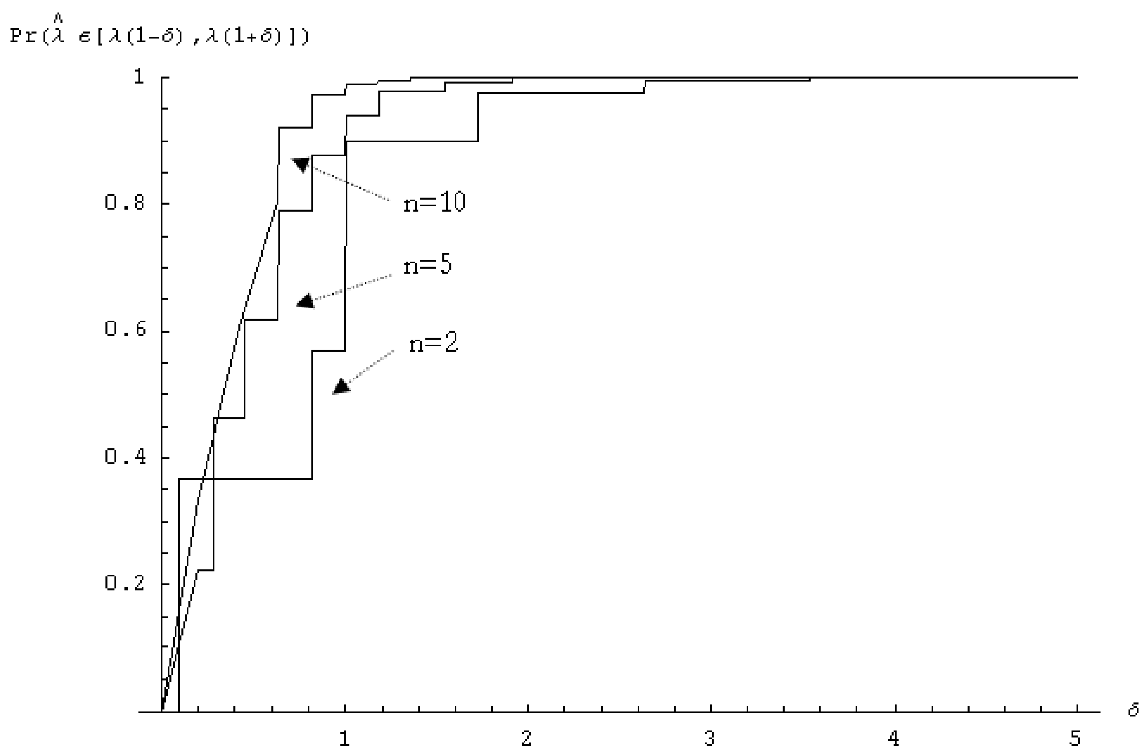


Figure 2-8 Precision of estimates of a zip code's homicide rate as a function of population size

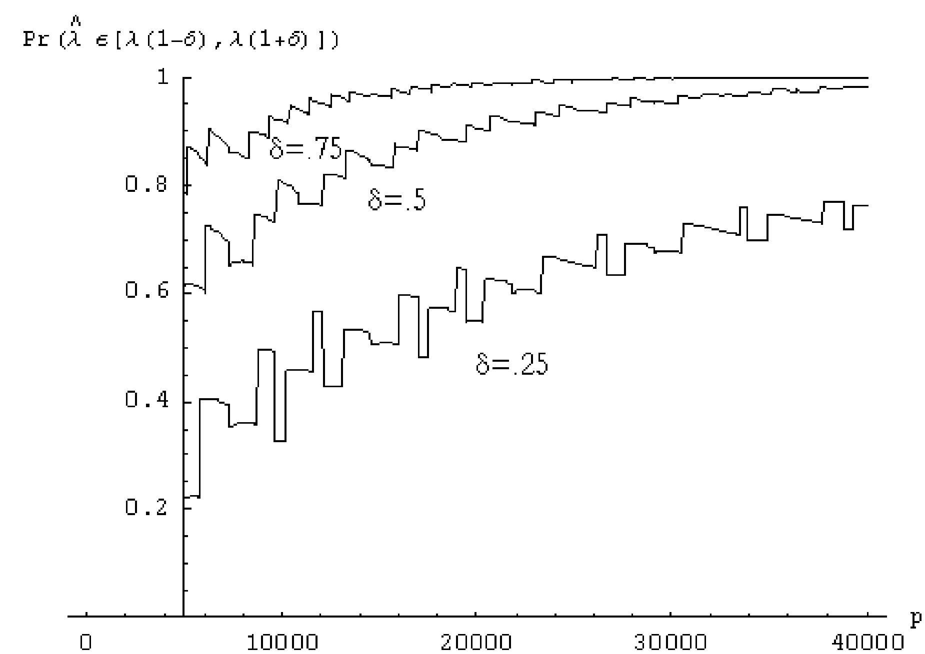
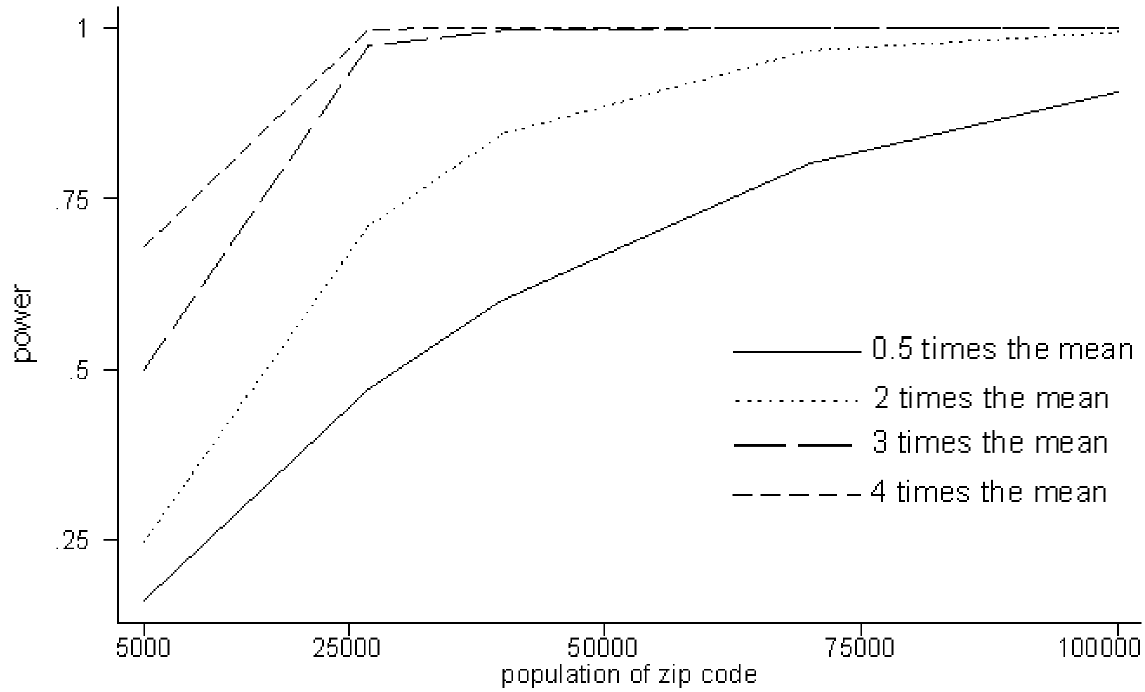


Figure 2-9 Power to detect a difference in homicide rate as function of zip code population



Chapter 3

Do Neighborhood Conditions Influence Attrition?

I Introduction

The previous chapters motivated attention to ecological context of attrition outcomes and showed empirically that clients' residential and treatment environments in Los Angeles County are indeed characterized by high levels of a number of the risk factors identified, compared to the residential neighborhoods of the non-client, household population. In this chapter, we move from a characterization of clients' environments to investigation of the hypotheses raised in Chapter 1. Namely, do characteristics of these environments affect the likelihood of attrition from treatment? Our analysis is motivated by three research questions:

1. Do rates of attrition vary meaningfully among clients' residential neighborhoods? Among treatment neighborhoods?
2. What are the relative contributions of residential and treatment neighborhood characteristics to the likelihood of attrition, net of individual factors?
3. Do these effects support the hypotheses forwarded in Section 3?

We continue with the same data from the Los Angeles Participant Reporting System (LACPRS) discharge data sets for fiscal years 1998-2000 and contextual measures that were analyzed in Chapter 2, with some additional transformations and adjustments to improve fit to our estimation models, as described below in the section on methods. The postal zip code continues to be the operational definition of "neighborhood" and all of the caveats raised in Chapter 2IV.D continue to apply.

An additional, more substantive caveat relevant to the analysis in this section is that LACPRS data are observational rather than experimental data, which has important consequences for the interpretation of results. In theory, any effects identified, even if statistically significant, may stem from variables not included in the analysis that are correlated with both retention and neighborhood characteristics. For example, if clients with less social support tend both to dropout more often and live in more disadvantaged neighborhoods, then this analysis would overemphasize the effect of local disadvantage on retention, since we have no control for social support. One can think of other examples. However, the greater threat to internal validity is the possibility of self-selection of clients into neighborhoods. If clients' choice of where they live or where they attend treatment is related in any way to their motivation to complete treatment, then estimates of the effect size of any neighborhood

characteristic related to this choice will be upward biased. This kind of bias would exaggerate findings of our drug availability proxies, for example, if highly motivated clients systematically relocate to drug-free neighborhoods before entering treatment (recall that LACPRS locational information is recorded at entry), or seek out treatment centers located in areas where drugs are less available. In practice, the risk of mistakenly attributing effects that are really due to neighborhood choice to the neighborhood attributes we examine is greatest if clients, in reality, are free to choose where they live, free to choose their treatment program, and capitalize on those choices. While we know of no study that has measured the desire or tendency of clients to switch residences as part of recovery, Table 2-1 provides some basis to speculate as to the mobility of clients in our sample. From the table, 87% of clients in the sample are not employed, about 70% are people of color (African American or Hispanic), at least 30% are involved in the criminal justice system, and 25% are homeless. Nearly all clients in the sample (97%) have at least one of these characteristics. One might speculate that the mobility of these clients is limited owing to lack of income, segregation in the market for residential housing (Massey 1990), screening procedures that discriminate against individuals with a criminal history, and local tolerance for homelessness, respectively. Furthermore, since LACPRS includes only publicly subsidized treatment programs, then one could argue that all clients in the sample probably have limited financial resources (so long as we assume that clients prefer private programs). Of course, it could be the case that clients without jobs have fewer community ties and are thus even more mobile than others. Further, restricted residential mobility would do less to solve the endogeneity problem if more motivated clients tend to stay with family members or friends who live in better neighborhoods while in treatment, but neither our data nor the literature allow us to estimate either kind of behavior.

More likely to be a source of bias is self-selection of motivated clients into better *treatment programs*, since all but criminal justice clients are free to apply to any program in the county, as long as they can afford the nominal fee charged by most publicly-subsidized programs. Assuming for the moment that clients are able to find out about programs in a variety of different neighborhoods (e.g., by word of mouth, advertisements), then it is conceivable that more motivated clients seek out programs with higher rates of retention, while less motivated clients are happy to choose whatever program, or perhaps even tend toward those programs known to be lenient with respect to noncompliance and relapse. If in addition, better programs tend to be located in better areas, then contextual effects estimated using the

LACPRS data will be overstated. On the other hand, if better programs tend locate in *worse* areas—perhaps because they attempt to serve the areas with the highest rates of addiction—then estimated contextual effects will be understated. While it is essential to be clear about and recognize these influences, in this case as well there is currently no information in the literature to assess the extent or direction of these threats to validity.

A final caveat relates to the well-known risk of omitted variables, but at the neighborhood level. High spatial correlations among rates of drug abuse and other "social pathologies" including poverty, crime, risky health behaviors, and social disorganization have been reported (Nurco, Shaffer and Cisin 1984) and suggest that any contextual study will be hard-pressed to include all potential neighborhood-level cofounders. We guard against this source of bias by simultaneously examining poverty, violence and victimization, proximity to job and retail outlets, and measures of drug activity in a multivariate framework, and by basing our selection of contextual covariates on theoretical considerations implied from past literature. These measures were discussed earlier in Chapter 1, Section III and Chapter 2, Section II.

Given the caveats mentioned above and the inevitable limitations they place on the interpretation of any relations identified using the present data, it is reasonable to ask what possible use such analysis can serve: Why should the first direct inquiry into the "treatment ecology" hypothesis begin with an analysis of these data? There are three reasons. First, though the LACPRS data are limited because they are observational, conducting a randomized experiment to address neighborhood influences on treatment outcomes would be expensive—clients would have to be randomly assigned to residential and treatment neighborhoods—which would be logistically infeasible and arguably immoral. Second, even if a conclusive argument for causation cannot be made, any model that is highly *predictive*, linking observable neighborhood characteristics with attrition, can still serve two purposes: (a) to predict the location of "problem" neighborhoods where proposed centers should avoid locating; and (b) to identify clients coming from such areas, who may require additional treatment components or ancillary services than may be suggested by the typical entry assessment. Third, analysis of administrative data is inexpensive, can lend support to hypotheses and help prioritize future investigations by identifying statistical associations even without establishing causation conclusively.

Below, we outline our conceptual model. Section III details our methods. In Section IV we present our findings and Section V concludes with a brief discussion.

II Conceptual Model and Hypotheses

The conceptual model developed in Section III.C classifies the mechanisms through which geographic and neighborhood context might affect attrition, but in this section we require a broader framework that allows us to disentangle contextual from individual-level influences. With no prior theoretical work in this area to draw on, we postulate simply that a client's probability of completing treatment is influenced by contributions from three sources: client attributes, the character of the residential neighborhood, and the character of the treatment program, including its location (Figure 3-1). We refer to each of these sources of variation as a "level" to match the terminology of our estimation framework, introduced in the next section. At the client level are characteristics that vary among individuals, both within and between neighborhoods, including age, addiction severity, motivation, treatment history, employment, status in the criminal justice system, living situation, and other individual-level factors identified in the treatment literature to date. At the residential neighborhood level are all of the contextual factors discussed earlier in Chapter 1 and summarized in the figure in Section III.C. As in Chapter 2, here we consider only a subset of these contextual factors: local drug availability, social stressors defined as neighborhood disadvantage and violence and victimization; proximity to retail services; and proximity to jobs. Figure 3-2 summarizes the hypothesized relationships from Chapter 1 graphically. The third level comprises the treatment program, which includes the treatment neighborhood (again with relevant features identified in Figure 3-2) as well as other aspects of the program that could impact attrition (e.g., treatment process components, staff, modality and treatment intensity). The impact of the three levels on attrition is not assumed to be independent: we expect some client characteristics to mitigate or intensify the influence of both contexts (e.g., whether the client is employed will affect the influence of proximity to jobs on attrition) and some features of the home neighborhood to mitigate or intensify the influence of the treatment neighborhood. Dotted lines in Figure 3-1 represent these cross-level interactions.

Figure 3-1 Conceptual model: Residential context, treatment context, and attrition

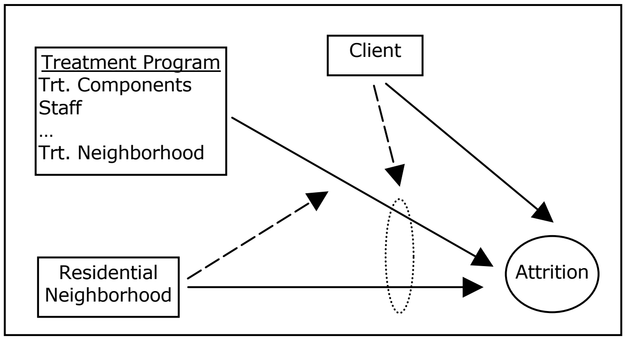
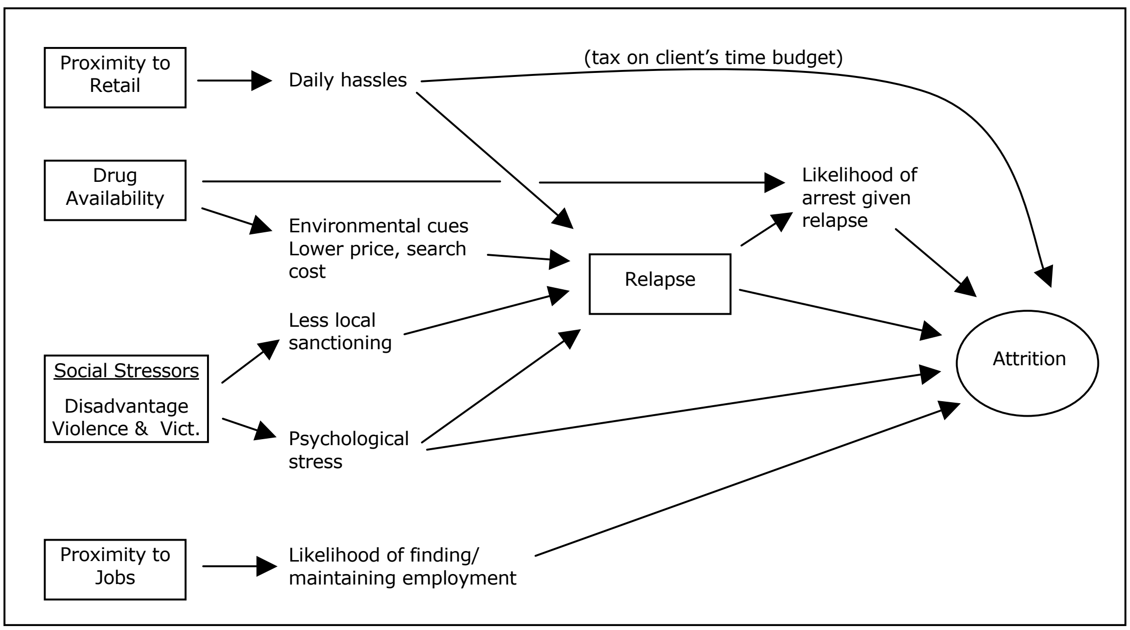


Figure 3-2 Conceptual model: Neighborhood context and attrition



From the discussion in Chapter 2, Section II, we form the following hypotheses for the main contextual effects, pertaining to both the residential and treatment neighborhoods:

H_1: There is a positive relationship between attrition and each of the following neighborhood-level factors: drug activity, disadvantage, and violence and victimization.

H_2: There is a negative relationship between attrition and each of the following neighborhood-level factors: proximity to jobs, proximity to retail.

We also reason that certain clients will be more responsive than others to some of the neighborhood influences (i.e., client-context interactions):

H_3: The negative relationship between attrition and proximity to jobs is stronger for clients who are not employed at admission than for those who are employed.

H_4: The positive relationship between attrition and drug availability is stronger for clients who are more severely addicted (i.e., daily users).

Finally, we hypothesize that the size of the contextual effects will depend on the relationship between the residential and treatment neighborhoods (i.e., between-neighborhood interactions):

H_5: The positive relationship between attrition and disadvantage in the treatment neighborhood is stronger for clients whose home neighborhood is less disadvantaged.

H_6: The positive relationship between attrition and victimization in the treatment neighborhood is stronger for clients whose home neighborhood is characterized by less victimization.

H_7: The positive relationship between attrition and drug availability in the treatment neighborhood is stronger for clients whose home neighborhood has less drug availability.

H_8: The negative relationship between attrition and proximity to jobs in the treatment neighborhood is stronger for clients whose home neighborhood is characterized by less proximity to jobs.

H_9: The negative relationship between attrition and proximity to retail in the treatment neighborhood is stronger for clients whose home neighborhood is characterized by less proximity to retail.

III Data and Methods

III.A Measures

Our response variable is *retention* and is derived from the discharge status item as recorded in LACPRS: *retention* is coded as 1 if discharge status is "completed

treatment/recovery plan, goals" and coded as 0 if discharge status is either "left before completion with satisfactory progress" or "left before completion with unsatisfactory progress". The remaining cases, "referred or transferred for further treatment" (N=6158, 19% of discharges in the fiscal period 1998-2000), are excluded from the analysis because their final retention status is unknown. Note that retention defined in this way depends on each treatment provider's policies regarding what constitutes grounds for removal from the program. These data also do not reveal whether the discharge resulted from a decision on the part of the client (i.e., voluntary) or on the part of the provider or an external actor such as the criminal system (i.e., involuntary).

We examine four classes of explanatory measures: (1) level one variables describing client characteristics previously shown to be correlated with retention in the literature; (2) other client-level variables (race, education level) shown in Chapter 2 to identify subpopulations more exposed than others to the contextual "risk factors" (having discovered that these clients are disproportionately exposed to these factors, we naturally want to include them as controls when attempting to isolate the impact of the contextual variables); (3) level two variables describing conditions in the residential neighborhood (i.e., the contextual measures); (4) level three variables describing conditions in the treatment neighborhood.

Some of these variables are modified based on findings from Chapter 2. The distinction between clients who have had one versus more than one prior treatment episodes is eliminated in the *prior treatment episodes* because it was found to be uncorrelated with the contextual measures. To eliminate skew and bring in outliers in order to improve the fit of the regressions and satisfy assumptions of the bivariate tests, a square root transformation is applied to each contextual measure, with the exception of *neighborhood disadvantage*, which is a measure used previously in the literature and is already quite symmetric.[25] Each measure is then standardized among zip codes so that a value of zero represents the average zip code and variations interpreted in terms of the variation among all zip codes in the county. These transformed and standardized versions of the contextual measures are used in all of the analyses to follow.

[25] In this case, the more common natural log transform is not possible due to the presence of zero values for all contextual variables in several zip codes. The square root, however, improves symmetry and, like the log, has the attractive property of increasing monotonically.

III.B Statistical Analysis

We conduct a bivariate exploration of retention rates at each level to get a preliminary sense of differences and identify any potential problems due to lack of variation with respect to each variable. Retention rates are computed at level one for each of the client strata in each modality, and test the null hypothesis that the proportion of clients who complete treatment is equal across strata, using a two-sample test of proportions (Table 3-1).[26] The "Z" column in each modality reports the result of the test for each client-level measure. We also report the mean retention rate for all clients in each modality as a point of reference.

The comparison at levels two and three is different because the contextual measures are continuous rather than categorical. For each contextual measure, we test the null hypothesis that the mean contextual measure (e.g., neighborhood disadvantage) for clients who complete treatment, $\mu_{complete}$ is equal to the mean for clients who do not complete, $\mu_{incomplet.}$ using a two-sided t-test. Positive differences imply that clients who complete treatment experience higher levels of the contextual measure on average than clients who do not complete.

Table 3-2 reports the differences in means and results of the test for measures characterizing the residential neighborhood. The comparison excludes homeless clients, whose residential neighborhood is unknown. Table 3-4 repeats the analysis for the *treatment* neighborhood, separately for homeless and non-homeless clients.

We then examine whether differences between the residential and treatment neighborhoods influence retention. Clients are classified according to whether their treatment neighborhood is "about the same" (within a standard deviation), "better", or "worse" (by a standard deviation or more) than the residential neighborhood, with respect to each contextual measure. Next we compute the retention rate in each of these three group and test two hypotheses: (1) the retention rate in the "better" group is equal to the retention rate in the "about the same" group; (2) the retention rate of the "worse" group is equal to the retention rate of the "about the same" group. Each is tested using a two-sample test of proportions.

A problem with the bivariate analyses above is that findings of neighborhood impact on retention may just result from correlation of neighborhood characteristics with individual characteristics. That is, the kind of clients more prone to dropout may tend to cluster in certain

types of neighborhoods. A multivariate analysis is required to isolate the effect of neighborhood-level variables conditional on client-level variables. A simplistic multivariate approach would be to estimate the usual logistic regression in which the log odds of the probability of completing treatment is by client-level and neighborhood-level covariates. In the usual formulation, a single error term appears in the equation for each observation to absorb deviation from the model for each client. A problem with this approach is that this client-level error term would also absorb omitted neighborhood-level factors, so that the error term would be correlated across individuals within the same neighborhood, a violation of the basic assumption of independent errors. Multi-level regression is designed to address this problem by explicitly modeling variation at each level. For each modality we estimate four multi-level models to partition variance in retention among the client, residential neighborhood, and treatment program levels. All of the models are technically *cross-classified* rather than *hierarchical* multi-level models because the three levels are not neatly "nested" in the form of a hierarchy: clients from the same residential neighborhood may attend different treatment programs (Goldstein, 1994; Browne, Goldstein, and Rasbash, 2001).

Model 1 (M_1) is an empty model (i.e., with no covariates) in which the log odds of the probability of completing treatment for client *i* residing in residential neighborhood *r* and attending treatment program *t* is the sum of a grand mean, b_o, a random residential neighborhood effect, U_r, and a random treatment program effect, U_t:

$$\begin{aligned} &y_{irt} \sim Bernoulli(p_{irt}) \\ &log\left(\frac{p_{irt}}{1-p_{irt}}\right) = b_0 + U_r + U_t \end{aligned} \qquad (M_1)$$

The random effects are unobserved, but estimates can be obtained for them by assuming a sampling structure and distribution of each. We adopt the standard formulation and assume that each random effect is drawn independently from populations of normally distributed effects with zero mean and unknown variance, i.e., U_r~Normal(0, σ_r^2) and U_t~Normal(0, σ_t^2). The interpretation is that each residential zip code contributes an effect that is randomly drawn from a population of zip code effects and likewise each treatment center contributes an effect that is randomly drawn from a population of treatment center effects. The point of the model

[26] One measure, *drug problem*, comprises three instead of two categories. In this case, a Kruskal-Wallis was used instead of the two-sample test.

is to estimate the variance components, σ_r^2 and σ_t^2, which under the null hypothesis are nil. Values greater than zero imply that retention does indeed vary at the neighborhood and treatment program levels. As described by Snijders and Bosker (1999), the model is conceptually valid only if one assumes the binary retention outcome is determined by an underlying, continuous latent (i.e., unobserved) variable, $\breve{y}$, which in the present context we might think of as propensity to complete the treatment program. Note that no individual-level random effect or error term, U_i, appears in M_1 because, as Snijders and Bosker explain, the total variance of the log odds is determined entirely by the probability, p_{irt}, since var(p_{irt}) = $p_{irt}(p_{irt})$. However, U_i does appear in the latent variable model below because $\breve{y}$ is continuous rather than binary:

$$\breve{y}_{irt} = b_0 + U_r + U_t + U_i$$

What allows us to relate the level-1 variance to the other variance components in M_1 is the fact that the two models are equivalent so long as we assume that U_i has the logistic distribution. Since the variance of a logistic distribution is $\pi^2/3$=3.29, the total variance of the log odds can be partitioned as follows, where the denominator in each expression below is the total variance:

Percent of variance at client level = $3.29 / (3.29 + \sigma_r^2 + \sigma_t^2)$

Percent of variance at residential neighborhood level = $\sigma_r^2 / (3.29 + \sigma_r^2 + \sigma_t^2)$

Percent of variance at treatment program level = $\sigma_t^2 / (3.29 + \sigma_r^2 + \sigma_t^2)$

The variance components above are called *unexplained* or residual variances because they summarize the variance in the outcome variable net of covariates (in M_1, of course, there are no covariates yet). That is, they summarize how much variance can be accounted for by the grouping structure of the data alone. This contrasts with *explained variance*, which is similar to the R^2 measure in single-level regression in that it summarizes the variance in the outcome measure accounted for by the covariates. The distinction becomes important as we begin to add covariates in the models to follow. Note that in absolute value, σ_r^2 and σ_t^2, have little meaning in the logistic framework and should not be compared across models that include different sets of covariates. Meaning is derived instead by examining their relative value, the ratios that summarize explained and unexplained variance (Snijders and Bosker, 1999).

In models M_2, M_3, and M_4, covariates are added in a staged fashion to allow us to observe how each set of variables affects the residual variance components. In particular, we are interested in whether addition of level-1 covariates changes the share of unexplained variance at levels 2 and 3, which would imply that our earlier bivariate findings regarding neighborhood impacts are just due to clustering of certain kinds of clients within certain kinds of neighborhoods. In the equations below, $\boldsymbol{X_1}$, and $\boldsymbol{X_2}$ are vectors of client-level covariates; $\boldsymbol{R_1}$ and $\boldsymbol{R_2}$ are vectors of residential neighborhood covariates; and $\boldsymbol{T_1}$ and $\boldsymbol{T_2}$ are vectors of treatment neighborhood covariates. The vectors $\boldsymbol{b_1}$, $\boldsymbol{b_2}$, $\boldsymbol{b_3}$, $\boldsymbol{b_4}$, $\boldsymbol{b_5}$ and $\boldsymbol{b_6}$ contain coefficients. M_2 builds on the empty model by adding a layer of level-1 covariates (i.e., characteristics specific to each client as well as distance-to-provider). M_3 adds a set of variables characterizing the residential and treatment neighborhoods. Finally, M_4 adds cross-level interactions.

$$log\left(\frac{p_{irt}}{1-p_{irt}}\right) =$$

$$b_0 + \mathbf{b}_1\mathbf{X}_{1i} + U_r + U_t \qquad (M_2)$$

$$b_0 + \mathbf{b}_1\mathbf{X}_{1i} + \mathbf{b}_2\mathbf{R}_{1r} + \mathbf{b}_3\mathbf{T}_{1t} + U_r + U_t \qquad (M_3)$$

$$b_0 + \mathbf{b}_1\mathbf{X}_{1i} + \mathbf{b}_2\mathbf{R}_{1r} + \mathbf{b}_3\mathbf{T}_{1t} + \mathbf{b}_4\mathbf{X}_{2i}\mathbf{R}_{2r} + \mathbf{b}_5\mathbf{X}_{3i}\mathbf{T}_{2t} + \mathbf{b}_6\mathbf{R}_{1r}\mathbf{T}_{1t} + U_r + U_t \qquad (M_4)$$

Naturally, the explanatory power of the model is expected to improve with each additional set of covariates. One way to measure the improvement is by examining the explained variance measure, which we now define:

$$\text{Explained variance} = \text{Var}(\mathbf{bX}) \,/\, (\text{Var}(\mathbf{bX}) + 3.29 + \sigma_r^2 + \sigma_t^2),$$

where the denominator is again the total variance in the outcome model (it's different now that covariates have been added) and where the vector **X** without any subscripts refers to the entire fixed part of the model (e.g., all covariates).

Our approach to interpretation of the variance components is summarized by the points below:

- Large and significant residential and treatment neighborhood components in M_1 support the notion that place matters, without yet revealing why;

- Large reductions in unexplained variance at levels 2 or 3 from the addition of level-1 covariates in M_2 means that client attributes related to attrition are also correlated with neighborhood characteristics;
- Large reductions in unexplained variance at levels 2 or 3 due to neighborhood covariates added in M_3 mean the particular neighborhood variables examined are the primary drivers of differences between treatment programs and residential neighborhoods.

The relationship between the exponentiated coefficient estimates and our hypotheses is as follows, where the subscripts *R* and *T* refer to the residential and treatment neighborhoods, respectively:

Hypothesis	Variable	Predicted exp(coefficient)
H_1	*Disadvantage*$_R$, *Disadvantage*$_T$	<1
H_1	*Homicides*$_R$, *Homicides*$_T$	<1
H_1	*Drug Deaths*$_R$, *Drug Deaths*$_T$	<1
H_1	*Drug Treatment*$_R$, *Drug Treatment*$_T$	<1
H_2	*Proximity to Jobs*$_R$, *Proximity to Jobs*$_T$	>1
H_2	*Proximity to Retail*$_R$, *Proximity to Retail*$_T$	>1
H_3	*Employed* x *Proximity to Jobs*$_{R/T}$	<1
H_4	*Daily user* x *Drug Deaths*$_{R/T}$	>1
H_5	*Disadvantage*$_R$ x *Disadvantage*$_T$	>1
H_6	*Homicides*$_R$ x *Homicides*$_T$	>1
H_7	*Drug Deaths*$_R$ x *Drug Deaths*$_T$	>1
H_7	*Drug Treatment*$_R$ x *Drug Treatment*$_T$	>1
H_8	*Proximity to Jobs*$_R$ x *Proximity to Jobs*$_T$	<1
H_9	*Proximity to Retail*$_R$ x *Proximity to Retail*$_T$	<1

The *calls for service* and *drug arrests* variables do not appear in the regressions because they are only unavailable for zip codes within the City of Los Angeles. Including these variables would require restricting the sample to clients both live and receive treatment in the city, which would eliminate much of the between-neighborhood variation and reduce sample size and statistical power considerably.

Finally, the models control for distance-to-provider through addition of a centred term ($\mathrm{Dist} - \overline{\mathrm{Dist}}$) and a squared centred term, where distance is measured as in Chapter 2, Section IV.C.

Estimates of the exponentiated coefficients, variance components, and explained and unexplained variances appear in Tables 3-8, 3-9, and 3-10 and were obtained using Markov chain Monte Carlo (MCMC) Bayesian updating techniques implemented in the WinBUGS software program. When using MCMC, one must specify prior distributions representing previous knowledge or expectations about each parameter. The final estimate is then a balance between information contained in the data and the prior, where the relative "weight" on each type of information is inversely proportional to its variance (Congdon 2001). With no prior empirical evidence to draw on, we specify standard diffuse or "non-informative" prior distributions and allow the data to speak for themselves: for coefficients, Normal($0,1.0^6$); for the variances of each of the three random effects (i.e., the σ's), Gamma(0.001, 0.001). Because MCMC is an iterative method, it is also important to check for convergence of the estimates.[27] In all models, we ran a burn-in of 2500 iterations and then examined the chains for all parameters. If no signs of non-convergence were found, additional iterations were run until the markov error for each parameter was reduced to 5% or less than the parameter's standard error, as recommended in the WinBUGS user manual (Speigelhalter, Thomas, Best, & Lunn 2003). Otherwise additional burn-in iterations were run until convergence seemed apparent. The run-time of the models is long compared to single-level regression. Models M_1 and M_3 required up to 8 hours to converge (with differences depending mainly on the number of observations in each modality), while models M_3 and M_4 required up to 2 days due to the additional covariates.

IV Findings

IV.A Bivariate results

Table 3-1 shows clear differences in retention across modalities as well as across client subgroups. On aggregate, methadone maintenance clients have much worse retention than others (5% compared to 30%), which may simply reflect differences in the meaning of the discharge status variable (i.e., "recovery goals") across modalities. Subgroup dynamics also differ by modality: Juveniles have significantly better outcomes in residential care, but worse outcomes in outpatient programs. More educated clients do significantly better in residential,

[27] More accurately, it is important to check for signs of non-convergence, since strictly speaking convergence can never be established with absolute certainty.

but not in the other modalities. Homeless clients are half as likely as other clients to complete outpatient treatment, but have virtually identical retention in the other modalities. These differences also indicate that separate analysis by modality is warranted.

In outpatient and residential care, the highest retention rates are found among whites and criminal justice clients. Note that the significant finding for race and education are at odds with most retention studies, which generally find no differences along these strata. The disagreement may be to due to our study area, whose high levels of residential segregation could lead to confounding among these variables and program quality.

Table 3-1 also reveals very low sample size in the methadone maintenance sample for criminal justice and marijuana only clients, suggesting these categories should be recoded for the multivariate analysis. Low sample size overall in the methadone maintenance sample compared to the other modalities means less power to detect differences and caution is recommended when comparing results in this modality to others.

Table 3-1 Retention rates by client-level measures

	Outpatient			**Meth. Maint.**			**Residential**		
Client stratum	N	%Ret.	Z	N	%Ret.	Z	N	%Ret.	Z
Age			***-7.25			N/A			***7.27
Juvenile	12757	0.26		0	N/A		9669	0.33	
Adult	2176	0.33		1579	0.05		274	0.12	
Race, Ethnicity			***10.17			1.57			***7.16
White†	4079	0.33		436	0.06		3398	0.37	
Other	10854	0.25		1143	0.04		6545	0.30	
Education			1.62			-1.61			***-8.24
< 12yrs	7296	0.27		707	0.04		3756	0.27	
≥ 12 yrs	7637	0.26		872	0.05		6187	0.35	
Drug Problem‡			*5.61			0.06			2.81
Marijuana only	1165	0.27		1	0.00		98	0.26	
Other drug only	2406	0.25		944	0.05		1895	0.33	
Polydrug	11362	0.27		634	0.05		7950	0.32	
Severity of use									
Less than daily	10076	0.29	***9.99	129	0.11	***3.52	3214	0.33	1.13
Daily use	4857	0.22		1450	0.04		6729	0.32	
Non-Injection user	13228	0.27	***5.24	50	0.04	-0.21	7943	0.32	0.22
Injection user	1705	0.22		1529	0.05		2000	0.32	
Prior treatment			**2.43			0.71			1.14
None	7893	0.28		80	0.06		4021	0.33	
1 or more prior episodes	7040	0.26		1499	0.05		5922	0.32	
Employment			***-14.83			0.51			-1.02
Not employed	11894	0.24		1445	0.05		9667	0.32	
Employed	3039	0.37		134	0.04		276	0.35	
Dual diagnosis			***4.63			1.60			***2.94
No mental illness	13785	0.27		1459	0.05		9127	0.33	
Mental illness	1148	0.21		120	0.02		816	0.28	

Source of referral			***-25.46			0.22			***-9.49
Not criminal justice	9618	0.20		1578	0.05		7197	0.30	
Criminal justice	5315	0.39		1	0.00		2746	0.40	
Homelessness			***12.64			-0.27			1.31
Not homeless	13254	0.28		1543	0.05		5382	0.33	
Homeless	1679	0.14		36	0.06		4561	0.32	
All clients	14933	0.27		1579	0.05		9943	0.32	

* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$
† White Hispanics are classified as "other".
‡The *Z* column in all rows but this one shows the z-statistic and significance of a two-sample test that the retention rates in across strata in the modality are equal; in this row, because there are three groups, it shows the results of a Kruskal-Wallis test of equal retention rates across groups: the Chi-Squared statistic and significance.

Table 3-2 summarizes the relationship between retention and the contextual measures by comparing the mean of each contextual measure in the residential neighborhood (level 2) of clients who complete treatment to the mean for clients who do not complete. We refer to the former group as "completers" for convenience, and to the latter as "dropouts", even though some "dropouts" may have had their treatment terminated involuntarily. In Table 3-2, negative differences mean completers experience lower levels of the contextual measure in their residential neighborhoods than dropouts. The magnitude of the difference is in terms of standard deviations. For example, the average completer lives in a residential zip code about a quarter (0.23) of a standard deviation less disadvantaged than the average dropout. To help build some intuition about the size of the differences, the table also shows the total number of standard deviations on each scale, or the full "range" of each neighborhood measure in terms of standard deviations. For example, on the disadvantage scale, the worst and best zip codes in Los Angeles County are 6 standard deviations apart, so the 0.23 figure above corresponds to 0.23/6 = 3.8% of the total difference in disadvantage among zip codes.

The findings are relatively consistent across modalities. In the outpatient and methadone maintenance samples, we find support for H_1, that completers have less disadvantage, victimization, and drug activity where they live than dropouts, but only partial support for H_2. Completers have better proximity to retail but the relationship regarding proximity to jobs is just opposite what H_2 would predict. In magnitude, the measures that separate completers from dropouts the most are disadvantage and victimization (homicides).

In residential care, the direction of effects is similar but the size of the effects smaller. One difference is that there is no difference with respect to drug activity, which suggests that living onsite may act as a buffer against negative impacts from the residential neighborhood. Alternatively, it may be that the other modalities residential and treatment effects are

confounded because clients attend treatment close to home and the treatment environment is really the primary driver.

Table 3-3 Difference in mean transformed and standardized residential neighborhood measures between clients who do and do not complete treatment ($\mu_{complete} - \mu_{incomplete}$) by modality

	Outpatient		**Meth. Maint.**		**Residential**		**Range of measure**
Neighborhood measure‡	N	Diff.	N	Diff.	N	Diff.	**(SDs)**
Disadvantage	12788	***-0.23$_{(0.02)}$	1453	***-0.36$_{(0.12)}$	4954	***-0.17$_{(0.03)}$	6
Victimization							
Homicides	12788	***-0.19$_{(0.02)}$	1453	**-0.27$_{(0.10)}$	4954	***-0.16$_{(0.03)}$	5
Calls for service†	4037	***-0.03$_{(0.01)}$	545	**-0.09$_{(0.04)}$	1583	0.00$_{(0.01)}$	8
Drug activity							
Drug deaths	12788	***0.00$_{(0.00)}$	1453	-0.01$_{(0.01)}$	4954	0.00$_{(0.00)}$	16
Treatment episodes	12788	***-0.07$_{(0.01)}$	1453	*-0.09$_{(0.05)}$	4954	-0.02$_{(0.01)}$	14
Drug arrests†	4037	***-0.05$_{(0.01)}$	545	*-0.17$_{(0.10)}$	1583	0.00$_{(0.03)}$	9
Proximity to retail	12788	***0.09$_{(0.01)}$	1453	***0.23$_{(0.07)}$	4954	***0.06$_{(0.02)}$	7
Proximity to jobs	12788	***-0.06$_{(0.01)}$	1453	-0.11$_{(0.07)}$	4954	-0.01$_{(0.02)}$	14

* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$; Standard errors in subscripts and parentheses

‡ Row variables are continuous: test is two-sided t-test (Ho: mean of row var for attrition group = mean of row var for non-attrition group)

†Restricted to clients receiving treatment in the LA City study area; all other rows refer to all of LA County

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Table 3-4 repeats this analysis for the treatment neighborhood, separately for homeless and non-homeless clients. For non-homeless clients, we again find support for H_1, consistent across modalities, again with the largest differences for disadvantage and victimization, and again partial support for H_2. Comparing Table 3-4 and Table 3-2 shows that these relationships are stronger in the treatment neighborhood than in the residential neighborhood for methadone maintenance and residential clients. In residential programs, we now see a drug availability effect in the treatment neighborhood that was not apparent in the residential neighborhood.

For homeless clients, the link between characteristics of the treatment neighborhood and retention appears even stronger, especially with respect to the calls for service and drug proxies. It could be that the treatment site for homeless clients is more likely to be the residential site, or that these measures are more relevant to the reality of homeless clients–e.g., that drug arrests better identify the markets to which homeless addicts have access.

As in Table 3-2 the result for proximity to jobs is inconsistent with our prediction. Also unexpected are the estimates for victimization in residential care, where in the case of homeless clients the two proxies intended to measure the same construct yield opposite results. One explanation is that the two measures reference different study areas (L.A. county versus

L.A. city); if the measures do in fact tap the same underlying construct of victimization, the difference may suggest that dynamics within LA city may be different from dynamics within the county as a whole. Indeed, we find that restricting all measures to the L.A. city study area eliminates the conflict—however, the results (not shown) indicate that within L.A. city, among homeless clients in residential care, completers live on average in *more* victimized areas—with respect to both measures—than dropouts, running counter to H_1.

Table 3-4 Difference in mean transformed and standardized treatment neighborhood measures between clients who do and do not complete treatment ($\mu_{complete} - \mu_{incomplete}$) by modality and homeless status

	Outpatient		Meth. Maint.		Residential	
Neighborhood measure‡	N	Diff.	N	Diff.	N	Diff.
Non-homeless clients						
Disadvantage	13201	***-0.20$_{(0.02)}$	1543	***-0.42$_{(0.09)}$	4395	***-0.29$_{(0.04)}$
Victimization						
Homicides	13201	***-0.17$_{(0.02)}$	1543	***-0.37$_{(0.08)}$	4395	***-0.24$_{(0.03)}$
Calls for service†	3919	*-0.01$_{(0.01)}$	771	**-0.05$_{(0.02)}$	1383	0.00$_{(0.01)}$
Drug activity						
Drug deaths	13201	***0.00$_{(0.00)}$	1543	0.00$_{(0.00)}$	4395	***-0.02$_{(0.00)}$
Treatment episodes	13201	***-0.06$_{(0.01)}$	1543	*-0.06$_{(0.03)}$	4395	***-0.07$_{(0.01)}$
Drug arrests†	3919	-0.02$_{(0.01)}$	771	**-0.16$_{(0.07)}$	1383	0.01$_{(0.03)}$
Proximity to retail	13201	***0.07$_{(0.01)}$	1543	***0.24$_{(0.06)}$	4395	*0.03$_{(0.02)}$
Proximity to jobs	13201	***0.04$_{(0.01)}$	1543	***-0.14$_{(0.05)}$	4395	***-0.07$_{(0.02)}$
Homeless clients						
Disadvantage	1678	***-0.76$_{(0.12)}$	36	**-1.18$_{(0.52)}$	3829	***-0.16$_{(0.04)}$
Victimization						
Homicides	1678	***-0.24$_{(0.06)}$	36	*-0.96$_{(0.50)}$	3829	***-0.17$_{(0.03)}$
Calls for service†	949	***-0.21$_{(0.03)}$	20	-0.22$_{(0.00)}$	1772	***0.06$_{(0.01)}$
Drug activity						
Drug deaths	1678	***-0.05$_{(0.01)}$	36	-0.02$_{(0.03)}$	3829	***-0.02$_{(0.00)}$
Treatment episodes	1678	***-0.50$_{(0.08)}$	36	-0.27$_{(0.21)}$	3829	-0.02$_{(0.02)}$
Drug arrests†	949	***-0.63$_{(0.09)}$	20	-0.70$_{(0.00)}$	1772	***0.19$_{(0.04)}$
Proximity to retail	1678	**0.11$_{(0.05)}$	36	*0.51$_{(0.30)}$	3829	0.00$_{(0.02)}$
Proximity to jobs	1678	***-0.23$_{(0.07)}$	36	0.22$_{(0.21)}$	3829	0.01$_{(0.03)}$

* $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$; Standard errors in subscripts and parentheses
‡ Row variables are continuous: test is two-sided t-test (Ho: mean of row var for attrition group = mean of row var for non-attrition group)
†Restricted to clients receiving treatment in the LA City study area; all other rows refer to all of LA County

Table 3-5 is the final bivariate analysis and shows retention rates for clients grouped according to the differential between their residential and treatment neighborhoods along each contextual measure. The columns, "about the same", "better", and "worse" show retention rates for clients in those groups. Comparing the "better" or "worse" columns to the "about the same" column gives an indication of residential-treatment neighborhood differences in retention, and the test results in the respective columns indicate whether this difference is

significant. The table provides only partial support for our hypotheses. For disadvantage and proximity to jobs, clients treated in neighborhoods better than their home neighborhood do better, and clients whose treatment neighborhood is worse than their home neighborhood do worse, as expected. But, for other measures the effect is just the opposite. With respect to drug deaths, *any* difference in neighborhood is linked with better outcomes.

All of the bivariate results above could be confounded with distance between the residential and treatment neighborhood, as well as differences in client composition and treatment program quality. We now turn to a multivariate analysis to control for confounding within and between levels.

Table 3-5 Retention rates for clients from one neighborhood who receive treatment in another

	Compared to residential zip, treatment zip is					
Contextual Measure	N	About the same*	N	Better‡	N	Worse‡
Disadvantage	13451	0.26	2482	***0.3$_{(-3.45)}$	2313	0.26$_{(0.34)}$
Victimization						
Homicides	14371	0.26	2080	0.28$_{(-1.17)}$	1795	0.28$_{(-1.09)}$
Calls for service†	3085	0.27	339	***0.14$_{(5.14)}$	718	0.25$_{(1.25)}$
Drug activity						
Drug deaths	14265	0.25	2140	***0.35$_{(-9.15)}$	1841	**0.28$_{(-2.18)}$
Treatment episodes	13375	0.27	1950	0.26$_{(0.52)}$	2921	0.26$_{(0.51)}$
Drug arrests†	3004	0.26	260	**0.19$_{(2.34)}$	878	0.28$_{(-1.45)}$
Proximity to retail	14708	0.26	1912	0.28$_{(-1.4)}$	1626	***0.3$_{(-3.02)}$
Proximity to jobs	13696	0.26	2696	***0.3$_{(-3.73)}$	1854	0.26$_{(0.18)}$

* "about the same" = within one standard deviation; "better"/ "worse" by more than one standard deviation

‡ z-statistics in subscripts and parenthesis refer to a test of the null hypothesis that the retention rate in the "about the same" and "better" groups are equal (similarly for the "about the same" vs. "worse" group); * $p \leq 0.10$, ** $p \leq 0.05$, *** $p \leq 0.01$

†Restricted to clients who live and receive treatment in the LA City study area; all other rows refer to all of LA County

IV.B Multivariate results

Estimated odds ratios and variance components for multi-level models M_1 to M_4 are presented in Table 3-5, Table 3-6, and Table 3-7, which correspond to the outpatient, methadone maintenance, and residential modalities, respectively.[28] We describe estimates for the unexplained variance components of the empty model, M_1, first as these allow us to attribute differences in retention, without yet controlling for specific covariates, to three broad sources of variation: differences among treatment programs (level 3), among residential

neighborhoods (level 2), and among individual clients (level 1). In all modalities, about 20% of variation is accounted for by differences between programs, about 80% by differences among individual clients, and only a negligible share by differences among residential neighborhoods. As neighborhood-level explanatory variables are added in M_3 and M_4, the increase in *explained* variance—or equivalently, the reduction in unexplained variance—is greatest for methadone maintenance (from 9 to 34%), and modest or non-existent in the outpatient and residential modalities, suggesting that in these modalities the particular neighborhood characteristics we have chosen account for only a minor influence on retention compared to unobserved factors. In fact, of the total variation among *treatment programs*, the neighborhood variables account for just 1% ((1-0.04)x0.17 – (1-0.08)x0.17) in outpatient, 4% ((1-0.03)x0.21 – (1-0.17)x0.20) in residential, and a more respectable 12% ((1-0.09)x0.23 – (1-0.34)x0.14) in methadone maintenance treatment. We now discuss results for specific variables in each modality. Note that in all modalities, most coefficients for the *Drug Deaths* variable and interactions could not be estimated from these data.

In outpatient treatment, we find a statistically significant effect for neighborhood disadvantage at the treatment site in support of H_1, with an estimated 28% reduction in the odds of retention for each standard deviation increase in disadvantage. We also find partial support for H_2 in a positive effect for residential proximity to retail. However, the unexpected inverse association between residential proximity to jobs and retention uncovered in the bivariate analysis remains even when controlling for client-level and contextual factors (models M_3 and M_4). Finally, a small, but significant distance-to-provider effect in M_2 disappears once neighborhood covariates are added in M_3, suggesting that clients observed to travel farther may be doing so in order to attend treatment in a particular kind of neighborhood.

In methadone maintenance treatment, we find few significant client-level effects and no significant contextual effects in any model. The few client-level effects disappear as additional covariates are added which suggests that these results are due to the small size of the methadone maintenance sample relative to the others, as random effects models are known to require a large number of observations to identify effects. In combination with the large increase in explained variance discussed earlier, it would appear that neighborhood factors are

[28] In the methadone maintenance models, the variables *Injection User*, *Juvenile*, and *Criminal Justice* were dropped due to lack of variation.

in fact associated with retention, but the nature of the relationship can not be determined from the present data.

In residential treatment, we find a significant effect for victimization (homicides) at the treatment site and for disadvantage at the residential site, both in support of H_1, with an estimated 65% decrease in the odds of retention for each standard deviation increase in the annual per capita homicide rate, and a 16% decrease for each standard deviation increase in disadvantage. However, examining models M_3 and M_4 together shows that this disadvantage effect is confounded with the drug availability proxy interaction (drug treatment episodes), whose magnitude is unexpected by our hypotheses and difficult to interpret. Because the estimated odds of the interaction term are greater than 1, the estimate is interpreted as follows: the more drug activity in the residential neighborhood, the larger the *positive* association between retention and drug activity in the treatment neighborhood. While H_1 and H_7 predict that drug activity at the treatment site would negatively influence retention, in such a way that the effect would be intensified for clients less exposed to drug activity at home, these findings suggest the opposite. They suggest that drug activity at the treatment site is positively associated with retention and that the relationship somehow grows stronger with drug activity at home. One might explain this result by hypothesizing that better programs tend to locate in neighborhoods with the greater need (i.e., greater drug activity), but because a random effect at level 3 controlling for unobserved differences between treatment programs has been included in the model, such an explanation would be inadequate.

Table 3-6 Estimated odds ratios from multivariate models, outpatient sample (N=12737)

	M_1	M_2	M_3	M_4
Fixed Effects				
Intercept	**0.339(0.031)**	**0.255(0.028)**	**0.208(0.066)**	**0.183(0.060)**
Juvenile		1.025(0.078)	1.023(0.078)	1.023(0.078)
White		**1.305(0.068)**	**1.296(0.069)**	**1.313(0.071)**
Edu. < 12 yrs.		0.955(0.045)	0.954(0.046)	0.956(0.046)
Marijuana only		1.091(0.105)	1.090(0.104)	1.092(0.104)
Polydrug user		1.033(0.065)	1.034(0.064)	1.041(0.064)
Daily user		**0.730(0.037)**	**0.733(0.038)**	0.905(0.120)
Injection user		**0.841(0.065)**	**0.840(0.065)**	**0.836(0.065)**
1+ prior episodes		1.088(0.050)	1.088(0.050)	1.085(0.049)
Employed		**1.370(0.072)**	**1.365(0.072)**	**1.426(0.081)**
Mental illness		0.959(0.091)	0.963(0.091)	0.966(0.091)
Criminal justice		**1.760(0.087)**	**1.760(0.087)**	**1.762(0.087)**
$Dist - \overline{Dist}$		**1.016(0.008)**	1.014(0.008)	1.009(0.008)
$(Dist - \overline{Dist})^2$		1.000(0.000)	1.000(0.000)	1.000(0.000)
Level 2: Residential Neighborhood				
Disadvantage$_R$			1.043(0.059)	1.072(0.068)
Homicides$_R$			1.021(0.065)	1.042(0.080)
Drug Deaths$_R$			1.275(1.158)	2.315(4.910)
Treatment Episodes$_R$			0.865(0.103)	0.804(0.103)
Proximity to Jobs$_R$			**0.878(0.049)**	**0.850(0.053)**
Proximity to Retail$_R$			**1.133(0.064)**	1.114(0.064)
Level 3: Treatment Program				
Disadvantage$_T$			**0.721(0.126)**	**0.719(0.120)**
Homicides$_T$			1.216(0.231)	1.309(0.285)
Drug Deaths$_T$			1.289(8.018)	0.637(4.433)
Treatment Episodes$_T$			1.327(0.534)	1.351(0.535)
Proximity to Jobs$_T$			1.249(0.228)	1.206(0.226)
Proximity to Retail$_T$			0.817(0.189)	0.811(0.192)
Cross-level Interactions (H_3-H_4)				
Employed x Jobs$_R$				1.172(0.125)
Employed x Jobs$_T$				1.098(0.124)
Daily user x DDeath$_R$				12.220(25.240)
Daily user x DDeath$_T$				5.107(12.720)
Cross-level Interactions (H_5-H_9)				
Disadv$_R$ x Disadv$_T$				0.967(0.034)
Homic$_R$ x Homic$_T$				0.984(0.047)
DDeath$_R$ x DDeath$_T$				Not Est.
DTreat$_R$ x DTreat$_T$				1.445(0.284)
Jobs$_R$ x Jobs$_T$				**0.783(0.066)**
Retail$_R$ x Retail$_T$				0.899(0.073)
Explained Variance				
Pct. Explained by covariates	0%	4%	7%	8%
Pct. unexplained at level 1	80	83	83	83
Pct. unexplained at level 2	1	1	1	0
Pct. unexplained at level 3	19	17	16	17
Random Effects				
$\sigma_2^2 = var(U_{2r})$ res. neighb.	0.026(0.015)	0.022(0.014)	0.020(0.013)	0.017(0.013)
$\sigma_3^2 = var(U_{3t})$ treat. pgm.	0.792(0.138)	0.657(0.120)	0.644(0.120)	0.663(0.124)

Bold = sig at 5% level based on z-test. “Not Est.” means the coefficient was estimated as > 1E6 but not significant. Such enormous estimates usually result when there is insufficient information in the data to produce a reliable estimate.

Table 3-7 Estimated odds ratios from multivariate models, methadone maintenance sample (N=1453)

	M_1	M_2	M_3	M_4
Fixed Effects				
` Intercept	**0.043$_{(0.011)}$**	**0.247$_{(0.186)}$**	0.683$_{(0.339)}$	0.116$_{(0.334)}$
White		1.291$_{(0.406)}$	1.102$_{(0.359)}$	1.046$_{(0.364)}$
Edu. < 12 yrs.		0.696$_{(0.201)}$	0.660$_{(0.196)}$	0.635$_{(0.197)}$
Polydrug user		0.746$_{(0.224)}$	0.735$_{(0.231)}$	0.717$_{(0.230)}$
Daily user		**0.355$_{(0.141)}$**	**0.372$_{(0.154)}$**	6.806$_{(53.820)}$
1+ prior episodes		0.936$_{(0.848)}$	0.816$_{(0.621)}$	0.830$_{(0.688)}$
Employed		0.723$_{(0.371)}$	0.722$_{(0.386)}$	0.731$_{(0.422)}$
Mental illness		0.455$_{(0.356)}$	0.500$_{(0.407)}$	0.562$_{(0.470)}$
$Dist - \overline{Dist}$		1.062$_{(0.074)}$	1.034$_{(0.077)}$	0.999$_{(0.083)}$
$(Dist - \overline{Dist})^2$		0.992$_{(0.009)}$	0.994$_{(0.009)}$	0.995$_{(0.010)}$
Level 2: Residential Neighborhood				
$Disadvantage_R$			1.200$_{(0.426)}$	1.433$_{(0.630)}$
$Homicides_R$			1.380$_{(0.581)}$	1.412$_{(0.895)}$
Drug $Deaths_R$			Not Est.	Not Est.
Treatment $Episodes_R$			0.711$_{(0.614)}$	0.459$_{(0.582)}$
Proximity to $Jobs_R$			0.662$_{(0.226)}$	0.522$_{(0.203)}$
Proximity to $Retail_R$			1.536$_{(0.559)}$	1.759$_{(0.701)}$
Level 3: Treatment Program				
$Disadvantage_T$			0.598$_{(0.329)}$	0.616$_{(0.339)}$
$Homicides_T$			0.970$_{(0.688)}$	0.888$_{(0.698)}$
Drug $Deaths_T$			Not Est.	Not Est.
Treatment $Episodes_T$			1.045$_{(1.428)}$	0.836$_{(1.384)}$
Proximity to $Jobs_T$			0.579$_{(0.357)}$	0.688$_{(0.493)}$
Proximity to $Retail_T$			1.695$_{(1.531)}$	1.635$_{(1.358)}$
Cross-level Interactions (H_3-H_4)				
Employed x $Jobs_R$				4.195$_{(6.011)}$
Employed x $Jobs_T$				10.480$_{(43.980)}$
Daily user x $DDeath_R$				Not Est.
Daily user x $DDeath_T$				Not Est.
Cross-level Interactions (H_5-H_9)				
$Disadv_R$ x $Disadv_T$				0.869$_{(0.297)}$
$Homic_R$ x $Homic_T$				1.342$_{(0.529)}$
$DDeath_R$ x $DDeath_T$				Not Est.
$DTreat_R$ x $DTreat_T$				77.270$_{(727.900)}$
$Jobs_R$ x $Jobs_T$				1.902$_{(1.454)}$
$Retail_R$ x $Retail_T$				0.684$_{(0.398)}$
Explained Variance				
Pct. explained by covariates	0%	9%	31%	34%
Pct. unexplained at level 1	75	75	94	78
Pct. unexplained at level 2	1	3	1	7
Pct. unexplained at level 3	24	23	4	14
Random Effects				
$\sigma_2^2 = var(U_{2r})$ res. neighb.				
$\sigma_3^2 = var(U_{3t})$ treat. pgm.				
Deviance	493.1	488.1	490.1	487.8

Bold = sig at 5% level based on z-test. "Not Est." means the coefficient was estimated as > 1E6 but not significant. Such enormous estimates usually result when there is insufficient information in the data to produce a reliable estimate.

Table 3-8 Estimated odds ratios from multivariate models, residential sample (N=4056)

	M_1	M_2	M_3	M_4
Fixed Effects				
Intercept	**0.345 (0.048)**	**0.330 (0.063)**	**0.302 (0.159)**	**0.276 (0.157)**
Juvenile		0.783 (0.388)	0.813 (0.392)	0.749 (0.366)
White		1.171 (0.100)	1.183 (0.108)	1.174 (0.108)
Edu. < 12 yrs.		**0.721 (0.059)**	**0.723 (0.059)**	**0.720 (0.059)**
Marijuana only		1.197 (0.484)	1.175 (0.477)	1.157 (0.468)
Polydrug user		1.130 (0.105)	1.123 (0.105)	1.128 (0.106)
Daily user		0.950 (0.080)	0.946 (0.079)	0.839 (0.152)
Injection user		**0.796 (0.077)**	**0.788 (0.077)**	**0.794 (0.078)**
1+ prior episodes		1.038 (0.081)	1.037 (0.081)	1.039 (0.081)
Employed		0.951 (0.191)	0.938 (0.189)	1.050 (0.252)
Mental illness		**0.726 (0.119)**	**0.725 (0.119)**	**0.718 (0.120)**
Criminal justice		**1.557 (0.135)**	**1.568 (0.136)**	**1.591 (0.139)**
$Dist - \overline{Dist}$		1.004 (0.006)	1.004 (0.006)	1.004 (0.006)
$(Dist - \overline{Dist})^2$		1.000 (0.000)	1.000 (0.000)	1.000 (0.000)
Level 2: Residential Neighborhood				
$Disadvantage_R$			**0.837 (0.071)**	0.855 (0.072)
$Homicides_R$			1.119 (0.103)	1.078 (0.112)
Drug $Deaths_R$			3.333 (5.259)	6.472 (92.150)
Treatment $Episodes_R$			1.238 (0.213)	1.165 (0.208)
Proximity to $Jobs_R$			1.039 (0.079)	1.011 (0.084)
Proximity to $Retail_R$			0.859 (0.073)	0.860 (0.074)
Level 3: Treatment Program				
$Disadvantage_T$			1.403 (0.359)	1.437 (0.384)
$Homicides_T$			**0.340 (0.115)**	**0.359 (0.133)**
Drug $Deaths_T$			4.876 (93.080)	5.444 (134.100)
Treatment $Episodes_T$			2.397 (1.443)	1.489 (0.989)
Proximity to $Jobs_T$			0.796 (0.225)	0.734 (0.209)
Proximity to $Retail_T$			0.621 (0.238)	0.657 (0.268)
Cross-level Interactions (H_3-H_4)				
Employed x $Jobs_R$				1.763 (0.750)
Employed x $Jobs_T$				1.065 (0.505)
Daily user x $DDeath_R$				13.050 (41.390)
Daily user x $DDeath_T$				0.288 (0.582)
Cross-level Interactions (H_5-H_9)				
$Disadv_R$ x $Disadv_T$				0.945 (0.047)
$Homic_R$ x $Homic_T$				1.077 (0.091)
$DDeath_R$ x $DDeath_T$				Not Est.
$DTreat_R$ x $DTreat_T$				**1.879 (0.565)**
$Jobs_R$ x $Jobs_T$				0.986 (0.122)
$Retail_R$ x $Retail_T$				0.974 (0.152)
Explained Variance				
Pct. explained by covariates	0%	3%	14%	17%
Pct. unexplained at level 1	79	79	81	80
Pct. unexplained at level 2	0	0	0	0
Pct. unexplained at level 3	21	21	19	20
Random Effects				
$\sigma_2^2 = var(U_{2r})$ res. neighb.	0.010 (0.012)	0.010 (0.012)	0.011 (0.013)	0.010 (0.012)
$\sigma_3^2 = var(U_{3t})$ treat. pgm.	0.881 (0.256)	0.910 (0.268)	0.778 (0.255)	0.846 (0.276)
Deviance	4513.0	4465.0	4461.0	4460.0

Bold = sig at 5% level based on z-test. "Not Est." means the coefficient was estimated as > 1E6 but not significant. Such enormous estimates usually result when there is insufficient information in the data to produce a reliable estimate.

V Conclusions

We opened this chapter with four research questions, which we are now in a position to address. The first asked whether rates of attrition vary with clients' residential or treatment neighborhood and on both counts our bivariate analyses shows that for some neighborhood variables, especially neighborhood disadvantage and victimization, they do. In the strongest relations identified, the average neighborhood of the non-homeless client who completes is up to 0.4 standard deviations better than the average "dropout's" neighborhood and the difference goes up three-fold to 1.2 SDs among homeless clients. Although most of the bivariate relationships identified were quite small, on the order of 0.10 SDs or less. We then asked about the relative contribution of neighborhoods to attrition outcomes—that is, whether the relationship could be explained away by neighborhood differences in client composition—and in fact, controlling for confounding client-level and treatment program-level factors, we found that the relative contribution of neighborhood characteristics is quite small, with residential neighborhoods exerting only a negligible independent effect and the effect of treatment neighborhoods making up just a small fraction (1-12 percent depending on modality) of the total effect of all differences between programs.[29] But although the relative contribution is small and no statistically significant relationship was found for most variables, a strong and independent association between some neighborhood characteristics and retention was identified, particularly at the treatment site. Importantly, the nature of the relationship depends heavily on modality. In outpatient settings, a one SD increase in disadvantage at the treatment site is associated with a 28% decline in the odds of retention; in residential settings, a one SD increase in the homicide rate is associated with a 16% decline in the same. No effects were identified for methadone maintenance clients, but considering other results, we attribute this to lack of sufficient data.

Our third research question asked whether the Los Angeles data support the hypotheses raised in Chapter 1 and conceptual model elaborated earlier in the present chapter.

[29] Examining additional neighborhood characteristics could increase this figure.

In this regard, we only find support for our principal hypothesis, H_1, related to disadvantage and violence and victimization. Interestingly, neither the bivariate nor multivariate analysis supports the notion that clients who attend treatment in neighborhoods worse than their own are more likely to drop out than others. Further, while the data do support a relationship between the commercial structure (proximity to jobs and retail) and attrition in outpatient care, it is not one that supports our hypotheses. In fact, the data show a *negative* relationship between proximity to jobs and retention, a result which deserves follow-up qualitative investigation to understand the source and nature of the effect, followed by appropriate revision of our conceptual model. One possibility is that this could be a "stigma" effect. [30] Neighborhoods with plentiful retail jobs are probably neighborhoods with middle class people and middle class conformity values. Even if there is no direct social interaction, social psychologists have shown that simply walking through what is seen as a hostile or stigmatizing social environment is extremely aversive to people (Cialdini, Reno and Kallgren 1990, Reno and Kallgren 1990, Kallgren, Reno and Cialdini 2000, Feldman and MacCoun 2003).

Though not one of our research questions, it is worth highlighting findings regarding homeless clients, as it has been argued that the social costs of addiction among homeless is greater than for the non-homeless (Wenzel, Ebener, Koegel and Gelbert 1996). The bivariate association between the treatment site and retention among homeless clients is many times greater than the for non-homeless clients, along a number of measures and particularly in outpatient care. If the strong relationships identified are not just due to confounding with the residential neighborhood—which, unfortunately, cannot be determined using these data—then the result suggests that one way to improve treatment outcomes among the homeless is finding ways to allow them to attend treatment in better areas. Future analysis using data that identify the general area where each homeless client spends his/her time while outside of treatment, and qualitative research to identify why neighborhoods may matter more for the homeless, are thus justified to determine whether such a policy intervention is warranted and to predict how effective it would be.

Before outlining our research questions, we went to some length to point out the observational nature of the data and related issues of interpretation. Underscoring these concerns is the observed positive relationship between distance-to-provider and retention in

[30] The author is grateful to Robert MacCoun for sharing this potential explanation of the findings..

outpatient care. Clearly, traveling farther to treatment on a regular basis in and of itself does not cause treatment completion. This result contrasts with analysis of a Baltimore City sample in which Beardsley et al. (2003) found a significant, negative association between distance and retention. That study used the same distance measure as we do here and examined treatment clients in publicly-funded programs, as we do, but in contrast to the present study included alcohol along with illicit drug users. The disagreement could be due to that difference if in fact the alcohol subsample is driving the Baltmore result (they are 22% of the sample), conceivable if the geographic configuration of alcohol treatment centers differs from that of others. Alternatively, it could result from differences in geography and transit infrastructure between the two study areas. A third possibility is that distance-to-provider and neighborhood factors are confounded. While we control for neighborhood factors and in fact witness the disappearance of the distance effect when those factors are introduced into the regression, the Baltimore study includes no such controls. All of these arguments notwithstanding, the Baltimore result remains more intuitive and is supported by other recent research examining transportation assistance and on-site vs. off-site location of auxiliary services (De Leon 1990, Umbricht-Schneiter, Ginn, Pabst and Bigelow 1994, Friedmann, D'Aunno, Jin and Alexander 2000, Marsh, D'Aunno and Smith 2000, Friedmann, Lemon, Stein, Etheridge and D'Aunno 2001).

Thus, this particular result suggests that future analysis should (1) investigate the relationship between distance and neighborhood factors, which is likely to vary with the study area; (2) be cautious of drawing conclusions regarding distance-to-provider from observational data and seek to confirm or reject such results through qualitative interviews with treatment clients; and, since the effect disappeared with the introduction of neighborhood factors in Los Angeles, (3) future research should examine the hypothesis that clients who travel farther may actually be doing so in order to attend treatment in more desirable neighborhoods.[31]

Our findings generally, and specifically the unexpected result that in residential care drug activity at the treatment site is positively associated with retention and that the relationship somehow grows stronger with drug activity at home, support the need to find out via qualitative methods whether and how neighborhood characteristics enter into treatment program choice and dropout decisions. This could be accomplished by way of interviews with

clients at admission and discharge, building on Stahler, et al. (1993)'s ethnographic approach to explaining retention outcomes. Additional research should also aim to clarify whether the effects observed are due to the real effects of social stressors or the "face-saving excuses" that these stressors may provide in justifying their decision to drop out to others.

Finally, our application of multi-level cross-classified modeling, which allows for the parceling of variation among latent or unobserved grouping factors, produced for the first time a broader picture of relative contributions to substance abuse treatment outcomes. The results indicate that in Los Angeles, in all modalities, approximately 20% of variation in retention can be attributed to differences among treatment programs, only a negligible portion to differences among clients' residential neighborhoods, and 80% to additional individual differences among clients, and this result holds after accounting for composition of clients within programs. The 80% labeled "individual differences" of course could be the result of other factors in common among groups of clients that have yet to be identified. It is also entirely possible that redefinition of the home "neighborhood"—that is, moving from postal zip codes to a unit more in line with residents' perceptions—could change these results. Finally, the assumption that random, unexplained variation at the client level follows a logistic distribution should be tested when theoretical results in multi-level cross-classified modeling allow for computation of variance terms under alternative distributions. These issues notwithstanding, the finding reaffirms the view that considerable gains are possible in the treatment system simply by identifying best practices among treatment programs, including, of course, decisions regarding program location.

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[31] The result does not suggest that these clients are traveling farther to get to better *treatment programs* because we have controlled for differences between programs with a program-level random effect.

Chapter 4

The Policy Implications of These Findings

I Introduction

Treatment location is a potential policy lever to improve treatment outcomes, through changes in zoning ordinances, incentives to private programs to locate in certain areas, public sector response to disputes involving community resistance to siting, and indirectly through selection of treatment programs for criminal justice clients by probation and health agencies. In the previous chapter, we found evidence of a systematic association between the location of the treatment neighborhood and treatment completion. This result suggests that location-oriented policies are worth investigating further. An analysis of the feasibility and/or cost-effectiveness of any one of the policy levers mentioned would draw on our results regarding the impact of neighborhood characteristics on retention, in combination with analysis of legal, social, and pecuniary aspects, any one of which would deserve a dissertation in its own right. Here, our analysis is limited to rephrasing our results in a form more palatable for use in such studies and more interpretable from a policy perspective. Although in the previous chapter we found that the relative contribution to attrition of our neighborhood-level factors is quite small, this says nothing about whether a large number of would-be dropouts could be converted into treatment completions through changes in treatment locations in a large population like that of Los Angeles County.

In this section we use the models developed in the previous chapter to carry out two numeric exercises to bound the potential impact of policies that aim to improve the treatment location of clients in the public treatment system. Each is a kind of counter-factual or "what-if" model that uses the relationship estimated in the previous chapter to compute how the outcome for each client would change when explanatory variables, namely treatment neighborhood characteristics, are altered. The assumption of these exercises is that the relationships identified are causal and, without revisiting the issue again in any depth, we simply remind the reader that although the models are based on observational data, the estimated relationship is the only kind of relationship available at present. Our purpose is to bound the implications of our findings if one assumes they are in fact causal. If the results of these exercises indicate that only slight gains would be achieved through changes in treatment location, then future analyses that include variables omitted from the present models will in all likelihood only downgrade the predicted gains further. If on the other hand the exercises suggest that large gains are in fact possible, then going through the trouble of identifying omitted variables and refining the models estimated in the previous chapter will be worth the

effort. An additional caveat to keep in mind is that we will be using our estimates to make predictions about treatment completion in some L.A. zip codes in which there currently are no treatment facilities. Consequently the reader should be aware that our results for these zip codes—and in fact the key figures produced in the two analyses to follow—rely on "out-of-sample" predictions that could possibly be incorrect if the relationship between neighborhood factors and attrition turns out to be substantially different in these zip codes than in the rest of the county. That, of course, is unknowable at present since there are currently no treatment facilities in those areas.

We conclude by examining the literature on organized resistance to a proposed facility--typically referred to as the Not In My Back Yard (NIMBY) syndrome—to reflect on whether and how these estimates would be useful for the resolution of NIMBY disputes.

II Exercise #1: Estimated gains from improving the location of current facilities

Considering the regression models estimated earlier and the statistically significant effects of attributes of the treatment neighborhood on retention, how many additional clients in Los Angeles County might successfully complete treatment per year if all clients were to attend treatment in the "most appropriate" location in the study area for them, considering their home neighborhood and personal profile? In other words, imagine that some unstated set of policy changes are to be carried out, which achieve a perfect match between clients and treatment sites (zip codes) while holding constant distance-to-provider, treatment program characteristics, and other variables outside our set of neighborhood characteristics: how many extra completes would result? The answer may be regarded as an upper bound of the impact of location-oriented policies assuming such policies do not simultaneously alter other factors associated with retention.

We first need to settle on one of the four regression models estimated for each of the modalities. In these exercises we do not analyze the case of methadone maintenance since in that modality no significant contextual effects were identified. We select model M_4, the most fully developed of the models, but re-estimate the model after eliminating non-significant covariates, non-estimable covariates, and the level-2 random residential neighborhood effect, which was shown to be negligible.

We define the "most appropriate" treatment zip code as the one—selected separately for each client in the sample—that would produce the highest probability of retention for each client according to M_4, holding all other variables constant. This is the neighborhood that maximizes the expected probability of retention, identified by the following relation:

$$A_{ir}^{\;*} = \max_j \sum_i E(P_{irj}; T^j) \tag{4-1}$$

where j subscripts the treatment zip code, P_{irj} is the estimated probability of completion for client *i* from residential neighborhood r, were the client to attend treatment in zip code *j*, $\boldsymbol{T}^j$ is the vector of neighborhood attributes for zip code *j*, and the expectation comes from the fitted model, M_4. Considering that the sample reflects two fiscal years of discharges, the expected number of additional successful treatment completions per year were all clients treated in the most appropriate neighborhood, is then

$$C = \frac{1}{2}\left(\sum_i \sum_r A_{ir}^{\;*} - \sum_i \sum_r A_{ir}\right) \tag{4-2}$$

where A_{ir} is the probability of completion with no policy change. Recalling that the data used to fit M_4 and in the expressions above reflect only the *first discharge* for each client in the observed 2-year study period, we must interpret *C* appropriately. Imagine a flow of clients presenting for treatment within a given year, some of whom may have received treatment prior to the year and some of whom have never in their lives received treatment before. *C* is the number of these clients who will complete on their first attempt during the year under the new policy, but would not have under the status quo.

What will become of these clients' second and later episodes during the year as a result of the new policy? This question is relevant if we are interested in the impact of the change in workload on the treatment system. If a client who would have dropped out now completes on the first attempt, do future episodes observed in the data during the study period disappear? From the literature (e.g., Hser, Anglin and Powers 1993), we know that recovery for most addicts is cyclical, with one success rarely leading to life-long abstinence. There are two possibilities. One is that the repeat episodes do occur, but because of the new policy, they also benefit from matching with the most appropriate treatment site. The second is that in addition to this benefit, there is also some reduction in the number of repeat episodes and/or an

increase in the time between episodes. In fact, a complete analysis of future impact would require a Markov model to assess long-term proliferation of increased completion probabilities in the system. Such an analysis would require additional information on treatment recidivism over a longer period than that available from our two-year data window. In this exercise, we simply predict the first-year effect of the policy change on clients' first episodes during the year.

Recall, additionally, that a number of first discharges (3833 cases or 19%, not including methadone maintenance cases) were dropped from the analysis due to incomplete data pertaining to the treatment episode or due to living or receiving treatment in a zip code so small that contextual measures could not be measured with adequate precision. These cases cannot be included in the prediction of *C* because of missing data, but certainly they would benefit from county-wide improvements in treatment locations. Before specifying how *C* should be augmented to account for these cases excluded from the estimation, we should acknowledge that these excluded cases fall into two categories: X_D cases known to have dropped out of treatment, and X_U cases whose completion status is unknown or missing from the data. We make the simple assumption that the first group—known dropouts—will convert to completes at the same rate as dropouts included in the estimation sample; and that in the second group—for whom it is unknown whether they dropped out or not—the percent of additional completes generated by the new policy will be equal to the percent of new completes relative to the *total* number of cases in the estimation sample. The assumption is that the fraction of dropouts among the X_U cases is the same as the fraction of dropouts in the estimation sample. The final estimate of additional completes in the first year of the policy among first-time clients, Δ, is then:

$$\Delta = \frac{k_1 X_D + k_2 X_u}{2} + C \tag{4-3}$$

where

$$k_1 = \frac{\sum_i \sum_r A_{ir}{}^* - \sum_i \sum_r A_{ir}}{N - \sum_i \sum_r A_{ir}} \tag{4-4}$$

and

$$k_2 = \frac{\sum_i \sum_r A_{ir}^* - \sum_i \sum_r A_{ir}}{N} \qquad (4\text{-}5)$$

N is the number of cases included in the estimation of M_4. $k1$ and k_2 both represent average rates of improvement in retention due to the policy among clients in the estimation sample, the first relative to the number of incompletes prior to the new policy, the other relative to total cases prior to the new policy. .

By embedding the expressions above in the MCMC model used to re-estimate M_4, we obtain not just point estimates for Δ, C, k_1, and k_2, but their entire sampling distributions. With each step of the MCMC algorithm, these variables are updated along with the regression coefficients and random effects. Frequency distributions of the Markov chain generated for the parameters following convergence correspond to their sampling distributions. Table 4-1 shows the mean, standard error, and 95% confidence intervals obtained for each variable. Figure 4-1 shows the corresponding densities. Note that the confidence intervals in the table are "credible", in the sense that may be safely interpreted as representing the probability the variable lies within the bounds listed given the data, as opposed to the less intuitive repeated sampling interpretation required of estimates produced by frequentist methods (Congdon 2001).

From the table, our best point estimate of the number of new completes resulting from the first year of a policy that would have each client attend treatment in the most appropriate neighborhood is 1670 (an increase from 4802 to 6472 completes), not counting reductions in repeat episodes. This corresponds to a rate of improvement in retention among residential clients of 41%, double that of outpatient clients, although a larger number of additional completions would result from application of the policy to the outpatient setting due to difference in size between the populations. Examining the standard errors and density plots, however, it is clear that the estimates produced from these data are highly variable: with 95% probability the figures cited above would lie within the considerably wide range of 573-3061 and 15-68%, respectively.

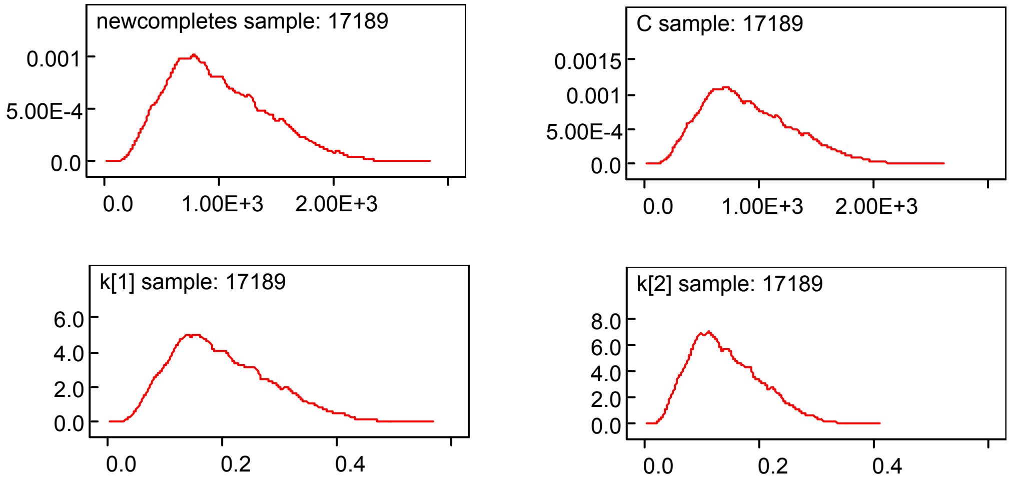
Table 4-1 Parameter estimates for Exercise #1

Parameter	Mean	S.E.	95% CI	
Outpatient				
New completes, including contribution from excluded cases, Δ	998.9	428.1	333.8	1948.0

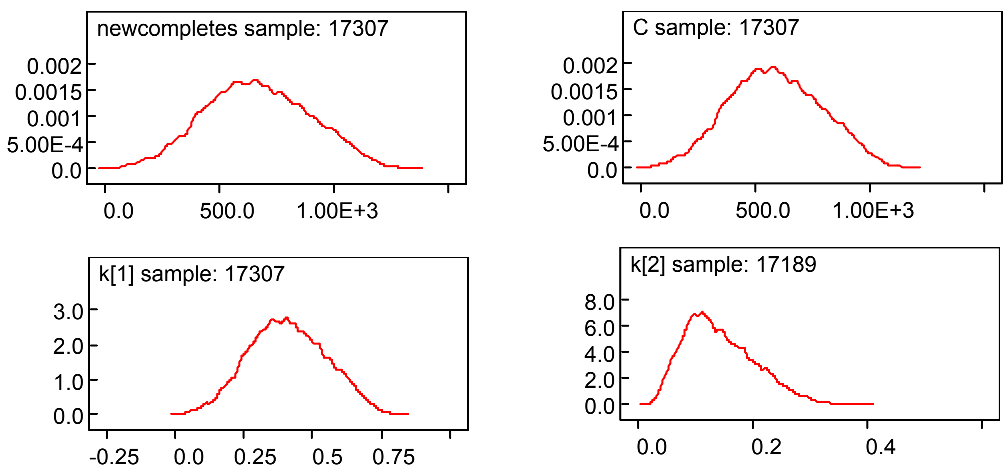
New completes, excluding contribution from excluded cases, C	914.7	392.0	305.6	1783.0
Rate of improvement in retention, relative to baseline dropouts, k_1	0.20	0.09	0.07	0.39
Rate of improvement in retention, relative to total cases, k_2	0.14	0.06	0.05	0.28
Residential				
New completes, including contribution from excluded cases, Δ	670.3	228.8	239.6	1113.0
New completes, excluding contribution from excluded cases, C	589.9	201.3	211.0	979.3
Rate of improvement in retention, relative to baseline dropouts, k_1	0.41	0.14	0.15	0.68
Rate of improvement in retention, relative to total cases, k_2	0.29	0.10	0.10	0.48

Figure 4-1 Densities for parameter estimates for Exercise #1

(a) *Outpatient*



(b) *Residential*



* "newcompletes" is Δ from equation 4-3.· *Sample* refers to the number of iterations after convergence.

III Exercise #2: Identifying promising locations for expanding treatment capacity

If the treatment location affects outcomes, then investments in treatment capacity will be more efficient in some areas than others. To quantify and map the relative efficiency of locations within the study area, it is useful to consider the following question, relevant to a decision-maker faced with choosing where in the county to expand (or reduce) treatment

capacity: Compared to the "best" neighborhood in the county, how many more treatment slots would have to be added in each neighborhood to attain one additional treatment success?

We again turn to the logistic models from the previous chapter to address this question. First note that these models predict the probability of attrition rather than a dichotomous outcome so that the thought experiment is one of expanding the number of clients in each neighborhood until there is a very high probability, say 95%, that at least one additional client completes. If treatment capacity is incremented by *n* treatment slots in a given zip code *j*, and assuming the slots will be filled (presumably by currently unmet client demand), then the probability that at least one of the additional clients completes in *j* is:

$$\pi(n,j) = 1-(1-p_j)^n \qquad (4\text{-}6)$$

where p_j is the probability of retention for a single client in zip code j. In each zip code j, then, we require the smallest n, say n_j, such that $\pi(n,j) \geq 95\%$. However, recall that the probability of retention depends not just on the treatment neighborhood, but also on client-level factors, characteristics of the client's residential neighborhood, and on the treatment program. Therefore, to estimate p_j, it is necessary to decide which clients will be filling the new treatment slots in each zip code and what kind of programs they will be attending. We assume the new slots are at new or existing programs with average effectiveness with respect to completion, i.e., that the random treatment program effect, U_t, in M_4 is equal to zero. With respect to type of clients filling the additional slots, recall that our purpose in carrying out this exercise is to predict what would happen if clients throughout the county were to have new alternative locations to attend treatment. Assuming these new slots, though introduced in particular neighborhoods, are available to anyone in the county, we estimate p_j for the average client in LAC in the same modality in 1998-2000. That is,

$$p_j = \frac{1}{N}\sum_{\mathrm{i}}\sum_{\mathrm{r}} E(P_{irj}) \qquad (4\text{-}7)$$

where $E(P_{irj})$ is the predicted probability of retention from M_4, re-estimated as in Exercise #1. The assumption that clients filling the new slots in zip code *j* will come from all over the county is more palatable given our failure to identify any inverse relationship between distance-to-provider and retention from the LAC data, even with observed distance-to-provider values in the estimation sample of up to 60 miles.

Table 4-2 shows the distribution of the p_j, computed according to equation 4-7. It is interesting to note that the variability in predicted probability of completion among zip codes is considerably higher for residential than outpatient care, which implies that the treatment location is more important for clients in the former modality than the latter, as we also saw in Exercise #1. The p_j are also highly correlated (63%) across the two modalities, suggesting that relatively good treatment locations for one modality are likely to be relatively good locations for the other. This is not surprising since the main effects linking location to retention in M_4 in the two modalities are violence and poverty, two factors that are often highly correlated among neighborhoods (Nurco, et al. 1984).

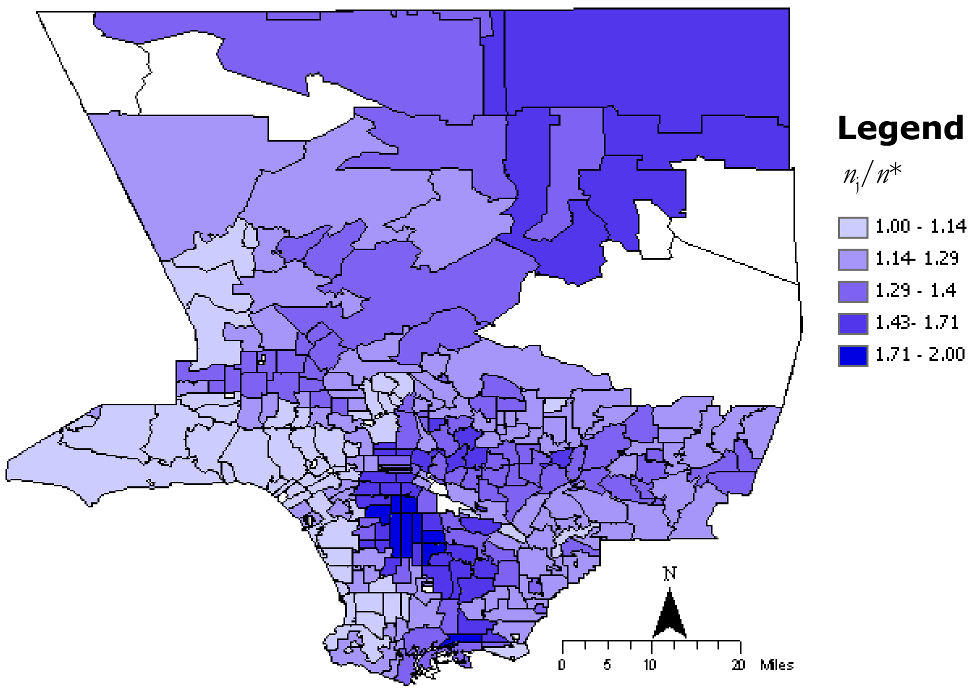
Table 4-2 Distribution of probability of completion among zip codes for the average client (N=283 zip codes)

Modality	Mean	Std. Dev.	Min	Max
Outpatient	0.29	0.03	0.20	0.39
Residential	0.34	0.09	0.12	0.57

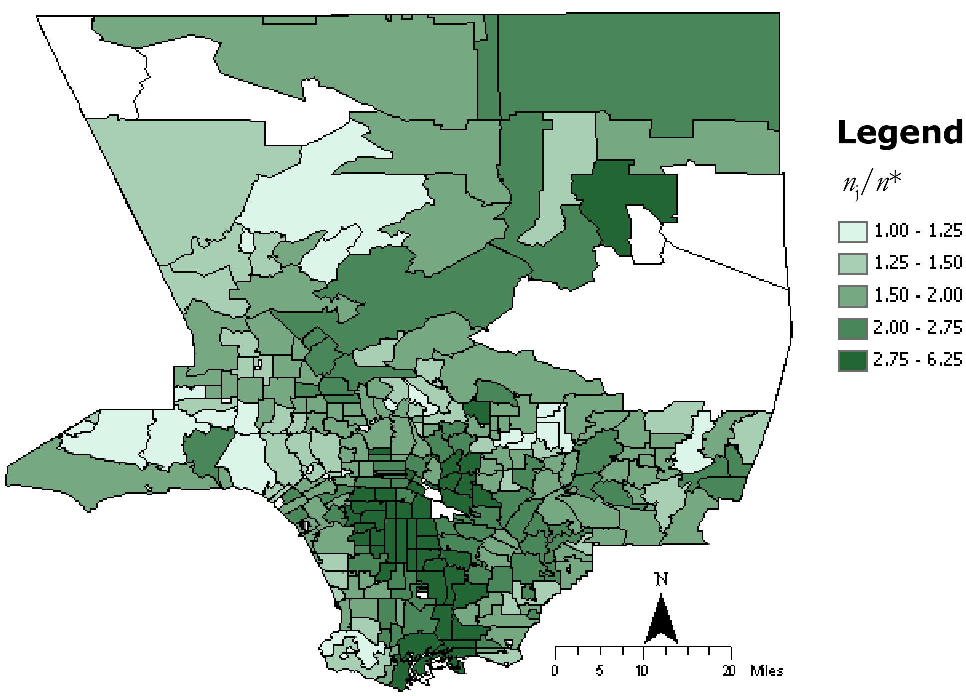
Following computation of the n_j from 4-6, we identify the best of the 283 zip codes as the minimum, $n^* = \min_j n_j$, and then compute the number of treatment slots in each zip code required to produce one treatment completion relative to the best zip code in the county, $\frac{n_j}{n^*}$. A map of the quintiles of the distribution of these ratios by modality is shown in Figure 4-2 (a) and (b), respectively. The results are most striking for residential treatment, where the range of these ratios implies that one would have to invest 6.25 times the number of treatment slots in the "worst" zip code to produce the same number of completions as one slot would produce in the best zip code in the sample. In outpatient care, that range is just 2 to 1.

Figure 4-2 Predicted ratio of treatment slots needed to produce one additional completion relative to the best zip code in Los Angeles County

(a) *Outpatient*



(b) *Residential*



- Colors are quintiles of the distribution.. Zip codes excluded from the analysis are unshaded.

Chapter 5

Application of These Findings to NIMBY Disputes

I What would the neighbors think?

The exercises in the previous chapter aid in the interpretation of the regression estimates from Chapter 3 from a policy perspective. Hinging on the assumption that the relationships identified in the regressions are causal, they demonstrate that the gains to be had from considering neighborhood character when selecting treatment center locations and matching clients with existing facilities are significant, whether one considers an individual client's probability of retention, the annual countywide number of completions, or the efficiency of individual treatment slots. The implication for a public health agency or private treatment provider facing organized community resistance to a siting proposal is that the location itself may be well worth fighting for. That is, places have innate value as potential treatment sites, value apart from considerations of local demand and travel burden on clients. As we mentioned earlier in Chapters 1 and 2, this concept is currently nowhere to be found in the treatment literature nor in practitioners' guides to siting (e.g.,Weber and Cowie 1995), which typically provide no guidance on neighborhood characteristics that are likely to facilitate the success of a planned treatment facility.

In theory, the results obtained in this dissertation have three important roles to play with respect to treatment center siting. One is simply in building awareness and recognition among scholars and practitioners that place matters for treatment outcomes, in such a way that the best neighborhood characteristics may be found in affluent neighborhoods where demand for treatment is less visible (Saxe, et al. 2001). Thus, researchers should carry out more extensive analysis to clarify the mechanisms at work and determine whether the relationships are causal. Assuming the results hold, the Substance Abuse and Mental Health Services Agency and other institutions involved in provider training should take steps to ensure that providers are aware that neighborhood character can affect treatment outcomes. Second, the analysis framework developed here can be of use in particular siting decisions. Regressions like the ones carried out in Chapter 3 or maps like the ones shown in the previous section can aid in a siting decision, so long as they are tailored appropriately to the target treatment population rather than the average county client. One can imagine the health agency's policy analyst or a treatment provider's expert consultant elaborating detailed multi-attribute decision models, with candidate locations constrained by local zoning ordinances, and all relevant factors considered: construction and maintenance costs, local property taxes, accessibility to public

transportation, expected utilization, willingness of staff to work at each site, environmental impacts, the neighborhood disadvantage index and 5-year average annual homicide rates, anticipated local resistance, etc. That is, a technocrat concerned with maximizing completion rates could make use of our contextual measures and findings.

But what would the neighbors think? That is, a potential third use of our findings is in providing objective, empirical analysis to rate the expected success of candidate treatment sites to inform NIMBY disputes. Even if made easily interpretable and displayed in attractive maps, a natural question is whether such information is likely to be viewed as relevant and/or persuasive to concerned residents.

The issue is worth considering as the NIMBY phenomenon is common in all treatment modalities and often results in prevention of or delays in siting, in addition to expensive legal battles and public education campaigns, and even deleterious impact of the process on client recovery (Weber, et al. 1995). In one case in which an Orange County, California provider sought to expand its residential capacity, an ex-client volunteered to speak in person before the local city council regarding his own personal success with the program, contrasting his prior deficiencies with his new, more socially acceptable lifestyle. The response by council members following his testimony, delivered disdainfully and in full view of other parties at the hearing, was that given all of the horrible behavior the ex-client had described *before* entering treatment, the council certainly didn't want any program that would attract his "type of person" to the community.[32] To overcome resistance to opposition by local governments, treatment providers often turn to the courts, arguing that zoning ordinances are discriminatory in their exclusion of drug users from the community. To avoid the issue altogether, some providers simply attempt to hide the true nature of their program when applying for a local permit to operate in an area. For example, one drug treatment provider who also planned to provide court-mandated training courses for individuals convicted of driving under the influence of alcohol, characterized her program as an "education" program, limiting the description of the drug treatment–and by far the biggest—component of the business to a footnote.[33]

NIMBY is certainly not limited to the siting or expansion of substance abuse treatment facilities. The more general problem of finding locations for locally unwanted land uses, or

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[32] Personal communication with Jeannie Brown, Director of Woodglen Recovery Junction of Fullerton, California, on February 4, 2003.

[33] Personal communication with an anonymous outpatient treatment provider in California in November, 2002.

LULUs, first began receiving scholarly attention in the 1970s, accompanying an increasing awareness of difficulties in the U.S. and Canada in siting toxic storage and disposal facilities, landfills and incinerators, and nuclear energy plants, which pose local environmental and health hazards (Rabe 1994). Research on these "industrial" as opposed to human service LULUs has centered most heavily on uncovering the social and political conditions that lead to NIMBY gridlock, exploring new public process mechanisms for getting past impasse between planners and communities (e.g., Rabe 1994, Inhaber 1998, McAvoy 1999), and the related empirical issue of detecting inequitable outcomes in burden sharing of LULUs among subpopulations long after decisions have been made (i.e., the environmental justice debate) (United Church of Christ Commission for Racial Justice 1987, Been 1994, Been and Gupta 1997, Waller, Louis and Carlin 1997, Mitchell, Thomas and Cutter 1999, Waller, Louis and Carlin 1999, Fricker and Hengartner 2001, Margai 2001).[34]

LULUs of both varieties, industrial and human, can be defined concisely as facilities that provide services to the general population, but which produce—or are perceived to produce—negative externalities locally (Popper 1991). With regard to industrial facilities, the negative externalities are typically perceived health and environmental impacts, noise and unsightliness, and the associated anticipated decline in property values. Human service facilities including homeless shelters, HIV/AIDS treatment centers, halfway houses, community corrections centers, and needle exchanges are also perceived to pose local negative externalities. In the case of drug treatment centers, the anticipated externalities include increases in local drug activity and crime, decline in property values, and a rupturing of the local community fabric. In contrast to industrial LULUs where emotional positions may at times be backed up by environmental impact reports that provide scientific evidence of a likelihood of risk, fear of negative externalities due to a proposed drug treatment center is more often founded entirely upon unsubstantiated anecdotes, stigma, and devaluation of a drug-using population seen as being involved in an illicit, immoral and contagious activity, perceived to be dangerous, non-productive, and personally culpable for their condition (e.g., Josey 2003). Takahashi (1997) used the latter three characteristics to describe how stigma motivates NIMBY resistance to human service LULUs more generally.

[34] Environmental justice research is more expansive than the NIMBY and facility location literature in the sense that it considers a broader class of LULUs including Petrofund, Superfund, Land Recycling, and Toxic Release Inventory sites, and even exposure to hazardous materials road accidents.

In light of the Orange County incident cited earlier and given that NIMBY resistance to treatment facilities is often grounded in the stigmatization of drug users and the view that they are personally responsible for their addiction who will quit using when and if they are ready, it is possible that opposed residents may be uninterested in or unwilling to recognize the positive benefits of treatment facilities. If so, such residents would also find little value in findings regarding the *relative* efficacy of neighborhoods. Unfortunately, the literature on human service LULUs provides no guidance to support or reject this claim, although much has been written about what motivates NIMBY in the literature on industrial LULUs. We now turn to this literature to predict what impact our analysis might have in informing NIMBY disputes in two respects: (1) Are opposed residents likely to find analysis similar to that provided here as relevant to the siting issue? (2) If so, is such information—e.g., rank orderings of neighborhoods by expected treatment efficiency—likely to persuade opposed residents in the most promising neighborhoods to accept the unwanted facility?

In examining this literature, it is useful to note that LULU scholars generally fall into one of three camps: those who favor collective deliberation or participatory siting mechanisms; those who would limit the role and influence of ordinary citizens, relying instead on policy analysts and experts to select a site, then relying on the resources and powers of state agencies to make the selected community accept; and those who favor compensation or market-based mechanisms as a means of ensuring that the community whose preferences best align with the LULU accept it voluntarily, induced by a reward that offsets their expected cost of hosting the facility. McAvoy (1999) summarizes the basic tension among these approaches as one of "Technocracy vs. Democracy" in the sense of the extent to which decision-making space is allocated to affected citizens and communities.

I.A Relevance to Concerned Residents

Herbert Inhaber is author of *Slaying the NIMBY Dragon* and an advocate of the market-based (and no other) approach. His experience in public siting hearings and review of the LULU literature have convinced him that

> In most waste siting situations, the people affected are not interested in learning. They feel, perhaps rightly, that if they become acquainted with the facts, they might be sucked into the entire siting process. So from their viewpoint, the most logical step is to chuck the informative pamphlet, prepared at great expense, into the garbage, and hoot and holler at the desperate scientists at the public meeting next week. Education, while a noble and uplifting idea in other contexts, usually does not work when an undesirable facility is in the wings (Inhaber 1998, pp. 36).

This view is later extended from hazardous waste facilities to AIDS clinics and all LULUs—albeit with no discussion of differences between the two cases—and is one which would lead one to conclude that our regressions and maps would have little relevance for residents opposed to siting.

Members of the collective deliberation camp differ from Inhaber on this point. In a case study of the Minnesota Waste Management Board's (WMB) attempts throughout the 1980's to site a stabilization and containment facility for hazardous waste, McAvoy (1999) discusses the opposition strategies taken up by residents of two counties selected as candidate sites. In one of the counties, Red Lake, a local grassroots opposition group calling themselves the Northland Concerned Citizens (NCC) was interested in the technical details. According to McAvoy, NCC studied maps of proposed flow of waste through the community, lobbied the Red Lake county government to bring in independent technical experts to evaluate risks posed by the proposed facility, and challenged the need for the facility on the grounds of the basis of its costs and financial sustainability. In addition, NCC investigated the track record of the company designated by WMB to operate the facility and disputed the fairness of locating the facility in their county, which they found accounted for just 1% of the state's production of waste.

McAvoy also summarizes the work of scholars who advocate for citizen participation in politics more generally, and several who instead favor a technocratic approach. Among evidence for the advocates is Kraft and Clary's (1991) content analysis of transcripts from siting hearings for a low-level radioactive waste disposal site. They determine that residents who testified in the hearings understood the ideas and language used by policy experts, were able to suggest informed and reasonable alternatives, and recognized anticipated benefits of the facility to society at large.

Barry Rabe, who investigated three decades of hazardous waste siting efforts in both the United States and Canada, also believes residents are capable of productive involvement in siting decisions. He finds that a common feature of successful siting agreements is "a combination of a voluntary siting process and formal commitment to burden sharing" in which public agencies and/or private institutions create spaces for collective deliberation and citizens voluntarily participate with scientific expertise available at their request (Rabe 1994, pp. 5). In contrast, a common feature of failures is citizens' view that the criteria used by the siting agency to reach its decision were arbitrary. To McAvoy and Rabe, then, technical details can be of relevance to concerned citizens and are often necessary to provide (at least the perception of) a nonarbitrary process. Particularly when they have already committed to a formal process and agreed in principle that some neighborhood ought to host the facility, citizens are receptive to notions of where the LULU *ought* to be placed, for reasons of efficiency and so forth. Babcock (1991) and Mazmanian and Stanley-Jones (1991) are also in favor of participatory approaches, but do not explicitly consider how residents respond in practice to technical analyses. The behavior observed by Barry Rabe regarding siting failures can also be understood in light of findings in the field of procedural justice, which suggest that people are less likely to accept unwanted outcomes when the procedures used to arrive at those outcomes do not appear fair, respectful of their standing, and unbiased (e.j., Lind, et al. 1989, Lind, et al. 1990).

A question on which all of these authors are silent is whether residents who demonstrate understanding of technical details and take up technical arguments do so only when it serves their interest in opposing the proposed facility.

I.B Persuasiveness

Even when residents are willing to consider technical aspects of siting in the sense of a comparative evaluation of the feasibility, cost, and benefit of siting a facility in one community versus another, they may not always find such arguments convincing enough to persuade them to drop their opposition if in the end their neighborhood is singled out as the most promising. Our results, in particular, would tend to place drug treatment facilities in more affluent neighborhoods based on an argument that doing so would reduce the rate of dropout and

increase the rate of treatment completion.[35] The NIMBY literature on industrial LULUs suggests a number of problems with this argument.

First, as Inhaber (1998) states succinctly, people do not like to be pointed at. Rank orderings of neighborhoods, whether used to persuade residents of a pre-selected neighborhood to drop their opposition or whether presented in a more preliminary phase as supplementary material to support a collective deliberation, are libel to spark suspicion among those residents regarding the credibility of the analysis and political motives behind it. They are libel to assume that the analysis is no more than a "front" and that there are other reasons their community is being singled out (e.g., in retaliation for some previous political controversy, to punish the rich). McAvoy's (1999) case study showed that residents were highly suspicious of any information disseminated by the siting agency, WMB, or its hired technical and media consultants, which went to considerable effort to convince residents of the low level of risk associated with the facility. These included public meetings, mailing educational brochures to residents, and setting up an information office in the candidate communities. The reaction of residents was to label all of these efforts as "manipulation", "brainwashing", and as attempts to "sell" the facility to "uninterested" residents (pp. 75-79). One conclusion from McAvoy's study, though based on just one case, would be that technical information should be presented by objective third parties rather than the siting agency. Further, the reaction of residents need not always be negative and whether it is will depend to a large extent—apart from the agency presenting the information—on the nature of the public process in which the siting deliberation is situated. Rabe (1994) finds that failures can generally be attributed to a siting process characterized by uncertainty, fear, distrust, and adversarialism. If a siting process is truly designed from the start as a collective deliberation intended to identify the best site for commonly agreed upon societal objectives, in which the above-mentioned characteristics are absent, Rabe concludes that communities will understand and eventually come to accept their social responsibility.

Nevertheless, notions of social responsibility and burden sharing are less clear and more contentious in the case of eliminating the drug problem than in the disposal and treatment of hazardous waste, for which, as consumers in an industrial-based society we must all take some blame to one extent or another. In contrast, who is responsible for the drug

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[35] We again ignore the issue of how such relocation of treatment facilities would impact travel burden, and the possible

users and associated harms? This is a complex question to say the least. The answer for any one person would depend on their religious stance, beliefs regarding environmental and social versus genetic predisposition, on whether social, economic, and family relations can lead to addiction—this by no means a comprehensive list. Drug use, as a morally and politically charged issue, is one in which strong and seemingly sacrosanct viewpoints are held and aggressively forwarded based on little more than a personal sense of what is right and wrong (MacCoun and Reuter 2001). Even in the unlikely event that scholars could arrive at some consensus regarding the culpability of various elements of society for drug use and associated harms, there is little reason to believe ordinary residents involved in a siting dispute would agree with it. More to the point, though residents of affluent communities singled out by our kind of analysis might be receptive to the idea that their neighborhoods can facilitate success in drug treatment, it is unlikely they would accept as a personal responsibility the (perceived) cost of hosting a treatment center to help drug treatment clients subdue their addiction. The "commonly agreed upon societal objective" that Rabe sees as a prerequisite for a successful participatory siting process is likely to be elusive in the case of siting drug treatment facilities.

A third reason residents may not view these analyses as persuasive has to do with the related issue of fairness, which is typically among the descriptors used by scholars to characterize good siting outcomes (Mazmanian, et al. 1991, Inhaber 1998). Siting opponents often invoke notions of fairness, with particular emphasis on the notion that a fair solution should allocate risks and costs of solving a problem to those communities that are most responsible for generating the problem. For example, in McAvoy's case study, many residents of suburban and rural communities resented being asked to host a facility that treated waste produced in urban areas. Analogously, residents of affluent neighborhoods who perceive little drug use in their own neighborhood are likely to perceive a decision that places a treatment center there as unfair.

Finally, residents are more willing to bear the risks of an unwanted facility when there are clear personal benefits from hosting it (Babcock 1991). Thus, Inhaber and other proponents of compensation schemes see these as the only means of winning over concerned residents. In the present case, treatment advocates are quick to point out that host communities benefit because the facility provides local drug users with greater access to treatment, which

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secondary effect of travel burden on treatment completion.

presumably helps in abating the local costs associated with drug use (Weber, et al. 1995). Nevertheless, as mentioned earlier community members whose challenge to hosting a site stems from stigmatization and devaluation of drug users may not acknowledge this benefit so readily. Other residents who do acknowledge the effectiveness and benefits of treatment may not be convinced that the metric used in our analysis, successful completion of the treatment program, is relevant. Persuading them that their neighborhood is truly the most appropriate neighborhood from the perspective of cleaning up the drug problem may require establishing a more direct link—a link difficult to test using the secondary data that are presently available—between neighborhood attributes and cessation of drug use or at least a reduction in consumption of drugs, and associated drug-related crime. Additionally, one would need to make the argument that such benefits would accrue locally, that is, that either the facility would treat clients who already live in or frequent the area, or who deal or commit other crimes there.

Together, the absence of clear social responsibility for the drug problem and the somewhat weak link between treatment completion and local, personal benefit to residents near the treatment facility, suggest that the analysis and findings presented here are more likely to be of use to planners and providers pursuing a technocratic strategy to siting than in winning over opposed residents or facilitating an attempt to site through collective and participatory deliberation.

Of course, there are many policy strategies available to providers and policy-makers interested in improving treatment outcomes in light of evidence that neighborhoods and location may influence retention apart from investing more or less in NIMBY battles. As mentioned in the introduction to this dissertation, treatment centers can be built in desirable locations or relocated at the cost of confronting NIMBY resistance, but providers can also arrange for treatment staff to travel to the client; investments can be made to improve the facility and its surroundings; and counselors may find creative ways to reduce the number of visits required for treatment.

Chapter 6

A Summary of Major Findings and Contributions

Major methodological contributions and substantive findings are summarized in this section. For details and important caveats, see the relevant chapter.

I Methodological Contributions

Multilevel cross-classified estimation models are nothing new and have been used with some success in other fields, particularly in research on education. However, the model we develop is the first formulated specifically for substance abuse treatment and allows us to isolate and measure the independent contributions of the treatment program, neighborhoods, and individual level characteristics to treatment outcomes. Exploiting the multilevel framework allowed us to avoid violating the Ordinary Least Squares (OLS) assumption that "error" terms in the model are independent when in fact they may be correlated across clients in the same program or neighborhood. One significant finding unrelated to the main thrust of the study made possible by this model is that even when differences in "clientele" have been taken into account, 9 to 17 percent of variation in attrition, depending on the treatment modality, can still be attributed to differences between treatment programs. Presumably this is due to differences in staffing, treatment approach, operations, aspects of the treatment neighborhood not included in this study, and other unobserved program-level factors. This result provides support for continued research into the contentious area of what kind of treatment program is best for a given treatment client. Notably, our models, like others in this literature, explain very little of the total variation in attrition among clients, suggesting that much work remains to be done to understand why clients drop out or are forced out of treatment.

In Chapter 2, we develop small area proxy measures of drug availability from secondary data sources, naturally a difficult undertaking given the illicit nature of the market. We suggest that area rates of drug-related arrest, drug-induced death, and local participation in drug treatment are the best of the available measures in Los Angeles. Each has serious shortcomings and together these measures are probably more indicative of drug activity than availability. Further, these data are collected, as in most counties, by various public agencies at various city and county levels, which complicates integrating them into a single model and prohibits obtaining full coverage of the county with all measures. A study seeking to incorporate all three measures would need to reduce its geographic coverage, at the cost of reduced variation in neighborhoods in the sample. Given the relative theoretical importance of

drug availability as a neighborhood-level predictor of treatment outcomes, new methods of measurement are needed. One possibility worth investigating is rapid appraisal methodology that relies on quick, inexpensive surveys, interviews, and/or focus groups. A major challenge to such methods is guaranteeing both uniformity and precision within numerous small areas throughout a larger study area.

We also improve upon the most common metric for approximating travel distance in the health and substance abuse literatures: the straight-line or Euclidean distance from the zip code population centroid to the facility. Because distance functions are non-linear and $E(f(x)) \neq f(E(x))$ for non-linear functions, the distance from the expected center (the population centroid) is not the same as the expected distance for the average zip code resident. We compute a measure that more closely approximates the expected distance given an individual's zip code by taking advantage of finer level census data (e.g., census blocks). We first compute the distance from each small census area to the facility and then take the average over these distances weighting by each census area's population to obtain a zip-code level approximation of distance-to-provider. The method is appropriate for any study in which subjects' locations are known at a geographic resolution larger than census blocks.

Also in Chapter 2 (Appendix 1), we develop a Poisson model that future studies may find useful whenever a rare-event measure per capita (such as the homicide rate) within a small area is under consideration as a potential variable for analysis. Essentially, the model can be considered a tool to determine the precision of such variables under various data selection criteria regarding the population of the geographic unit (e.g., zip code) and/or the number of years of data over which to average the rare-event measure in order to increase its precision. The model can be used in preliminary analysis to select or construct explanatory variables in order to guarantee sufficient precision, and to estimate precision if such information is needed as inputs to a later analysis, for example a weighted least squares (WLS) regression.

In Chapters 4 and 5, we use our estimated multilevel models to carry out counterfactuals (sometimes called "simulations") in order to generate numbers that are easier to interpret than our regression coefficients and arguably more useful for policy. While traditional counterfactuals only produce point estimates, we obtain full distributions for our policy-relevant figures that take into account the uncertainty in the regression coefficients. We do this by embedding the computation of the counterfactual in the same Markov Chain Monte

Carlo model used to estimate the coefficients (using WinBUGS software). While the approach is nothing new to Bayesian statisticians, it is rare if not unknown in this literature.

II Substantive Findings

In Chapter 1, we laid a theoretical foundation for the study of what we call a "treatment ecology", or the importance of environment and neighborhood for individuals undergoing treatment for drug abuse. We developed a broad range of hypotheses relating neighborhood factors to treatment outcomes based on an extensive review of the literatures on drug use, drug treatment, environmental psychology, the influence of neighborhoods on health-related and so-called risky behaviors, and the importance and meaning of neighborhoods more generally.

This review also revealed the need for research, policy, and most importantly state and county data systems to distinguish between *voluntary* vs. *involuntary* dropout so that strategies can be developed based on an understanding of the mechanisms leading to each. Similarly, information on relapse during treatment should be collected and included in uniform data collection systems as well. Of course, even when such information are collected, their use will be limited for use in comparing client outcomes across treatment programs so long as those programs differ in their drug testing and relapse detection policies.

Several recent studies have examined distance-to-provider. Our review argued for considering not just distance, but the full cost (including opportunity cost of attendance) and benefit of a particular location for a particular treatment client (see Ch.1 and Figure 1.1).

In Chapter 2, we produced the first population-level estimates characterizing the environments where drug treatment clients in publicly-funded and community-based recovery programs live and receive treatment. Five contextual factors were examined: neighborhood disadvantage, violence and victimization, drug availability, proximity to jobs, and proximity to retail services. We summarized how clients' home and treatment neighborhoods differ in terms of these factors, as well as their separation in terms of geographic distance. We charted the extent to which clients who live in one kind of neighborhood travel to treatment in another and found a stronger tendency for travel from "better" to "worse" neighborhoods than vice versa. Our findings indicate that treatment clients in Los Angeles County are found in residential neighborhoods that are markedly different than those of the general household

population, with significantly higher rates of poverty and disadvantage, violence and victimization, and drug availability (approximated by measures of drug arrests, drug-related deaths, and participation in drug treatment by neighborhood residents). For example, the average treatment client resides in a neighborhood with a rate of disadvantage more than triple the rate of the average county resident, and with a rate of violence and victimization 30 to 40 percent higher. Treatment locations follow a similar pattern, but with even starker differences.

In Chapter 3, we asked whether rates of attrition vary with clients' residential and treatment neighborhoods. On both counts our bivariate analyses show that for some neighborhood variables, especially neighborhood disadvantage and victimization, they do. In the strongest relations identified, the average neighborhood of the non-homeless client who completes is up to 0.4 standard deviations better than the average "dropout's" neighborhood and the difference goes up three-fold to 1.2 SDs among homeless clients. Although most of the bivariate relationships identified were quite small, on the order of 0.10 SDs or less. We then asked about the relative contribution of neighborhoods to attrition outcomes—that is, whether the relationship could be explained away by neighborhood differences in client composition—and in fact, controlling for confounding client-level and treatment program-level factors, we found that the relative contribution of neighborhood characteristics is quite small, with residential neighborhoods exerting only a negligible independent effect and the effect of treatment neighborhoods making up just a small fraction (1-12 percent depending on modality) of the total effect of all differences between programs.[36] But although the relative contribution is small and no statistically significant relationship was found for most variables, a strong and independent association between some neighborhood characteristics and retention was identified, particularly at the treatment site. Importantly, the nature of the relationship depends heavily on modality. In outpatient settings, a one SD increase in disadvantage at the treatment site is associated with a 28% decline in the odds of retention; in residential settings, a one SD increase in the homicide rate is associated with a 16% decline in the same. No effects were identified for methadone maintenance clients, but considering other results, we attribute this to insufficient sample size.

Our third research question asked whether these results support the conceptual model elaborated in Chapter 1. In this regard, we only find support for our principal hypothesis, H_1,

[36] Examining additional neighborhood characteristics could increase this figure.

related to disadvantage and violence and victimization. Interestingly, neither the bivariate nor multivariate analysis supports the notion that clients who attend treatment in neighborhoods worse than their own are more likely to drop out than others. Further, while the data do support a relationship between the commercial structure (proximity to jobs and retail) and attrition in outpatient care, it is not one that supports our hypotheses. In fact, the data show a *negative* relationship between proximity to jobs and retention, a result which deserves follow-up qualitative investigation to understand exactly why local commercial structure matters, whether directly or as a correlate of other underlying factors.

Next, we used our estimation equations to develop two estimates more relevant to policy-making. First, we asked what would happen if all clients received treatment in the most "appropriate" neighborhood (though appropriate only in the sense of our predictions regarding retention)? How much would dropout decrease? Second, we asked just how much the location of the treatment facility matters. For example, if the Alcohol and Drug Programs Administration wanted to expand treatment slots in a single neighborhood enough to yield just one additional treatment completion, how much would they have to invest in the "best" as opposed to the "worst" neighborhood? Both calculations suggest that the gains to be had from considering treatment location are large. However, because these results are based on observational data, we regard them as upper bounds on the potential effect of "location-oriented" policies and recommend additional qualitative and ethnographic research as a means of verifying that the figures estimated do indeed imply what our hypotheses suggest they do.

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