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INSTITUTE FOR SOCIAL AND ECONOMIC RESEARCH AND POLICY

COLUMBIA UNIVERSITY IN THE CITY OF NEW YORK

POLICYMAKING AND CASELOAD DYNAMICS: HOMELESS SHELTERS

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Abstract

Many social problems can be understood as "conceptual caseloads", e.g., people in poverty, and actual programmatic caseloads are a major, specific concern for policymakers and public administrators. Thus, a crucial and fairly general concern is how caseloads—whether conceptual or programmatic—can be reduced. To address this concern, officials often fall back on politically or intuitively attractive ideas—preventing people from entering caseloads, for example. Failure to incorporate caseload dynamics, however, may mean prevention and other caseload reduction policies will deliver much less than promised, and may cause caseloads to grow. In this paper, we first show how caseload size depends only on the number of entrants to a caseload and the rate at which people leave a caseload. With this framework in mind, we then address two common, seemingly appropriate policy responses: preventing entrants and hastening leaving. However, we show how too little is now known about homeless prevention to pin high hopes on its utility and that some social welfare problems, like homelessness, may be too inhospitable for prevention's logic. We then explain how allocating resources to hasten leaving is not as straightforward as policymakers have assumed but rather must recognize caseload dynamics to avoid unintended growth. We conclude by explaining some limits and opportunities in using caseload dynamics for policymaking.

Keywords: homelessness; dynamics; prevention; caseloads; shelters

1. Introduction

One way to think about some public problems is to transform them into caseloads. We can think about whether some entity, such as people or households, are in a particular problematic status and whether others are likely to enter that status. Poverty, for example, can be thought of as a caseload of those households with incomes below the poverty level, and we can imagine a population of households whose incomes are likely to fall below that level. One policy concern, then, might be to reduce the size of this "conceptual" caseload.

This is, of course, a more abstract construction of problems defined by the programmatic caseloads of government initiatives, such as those in social welfare: poor families receiving financial assistance; children living in foster care; and adults and families living in homeless shelters. A major concern of policymakers, public administrators and nonprofit staff is how to manage programmatic caseloads for which they are formally responsible, and, frequently, how to reduce their size.

Conceptual frameworks for translating caseload concerns into effective management strategies are lacking, however. Public officials more commonly fall back on intuitively appealing ideas. To slow the number of people going onto foster care caseloads, for example, they establish "family preservation" prevention programs. To reduce the number of people in homeless shelters, they provide permanent housing for people soon after they become sheltered. But intuitive appeal may not translate into actual impact. Social welfare problems have conditions that undermine the effectiveness of prevention. As a result, public officials and nonprofits should not be sanguine about how much prevention can slim caseloads. And reducing caseloads by emphasizing exits requires understanding the dynamic movement of social welfare populations. Programs not heeding dynamics can unintentionally increase caseload size, making officials look bad and government programs ineffective. And what is true of more formal caseloads like TANF and homeless shelters is true for more conceptual caseloads such as households in poverty and those who are unemployed.

This paper uses homeless shelter caseloads to examine logical and empirical issues in addressing caseload management, particularly caseload reduction.[2] It focuses on caseload dynamics as the crucial conceptual framework for effective policymaking and utilizes this framework to examine the logic and empirics of prevention and discharge policies. The paper makes two empirical points. First, despite the alluring chords of its logic, prevention may be a siren song. For example, efforts to prevent homelessness run aground because we lack sufficient knowledge about who becomes homeless and what services work. Further, the nature of homelessness—and of other social welfare problems—may disable prevention's logic, suggesting it may never be a major tool for reducing homeless, and perhaps other social welfare, populations. Second, resource allocation to reduce caseload size should be in tune with caseload dynamics. Allocating permanent housing subsidies to homeless shelter users is a useful example. Ignoring shelter dynamics when making these allocations could make shelters a less costly path to relatively scarce subsidies. One result could be—has been—an increase in shelter caseloads.

Although the analysis focuses on homeless caseloads, these issues are not peculiar to it. They confront public officials addressing welfare and foster care (Bane & Ellwood 1983; Ellwood 1986; Schuerman et al 1993; Wulczyn et al 1990b) and are not unique to social welfare problems (Clark & Summers 1979). Furthermore, our analysis uses evidence from foster care indicating a story similar to homelessness. In the early nineties (the time of the empirical study in this paper), the number of people in foster care soared, just as with homeless shelters, tearing up families and burdening government budgets (Wulczyn et al 1990b; Wulczyn 2003); too, foster care and homelessness may share some causes (Berlin & McAllister 1994); and policy responses have been similar. The early nineties foster care crisis spawned "family preservation" programs that tried to prevent placements by keeping children and parents together with brief, intensive doses of counseling and social services, and officials tried to reduce the foster care population by hastening exits. Further, as we later show, the importance of caseload dynamics in discharge policy is similar to what the analysis finds in homelessness.

The paper is organized by explaining in section two the logic of how caseload size changes. Section three focuses on one response that seems to follow from that logic—prevention—and explains why prevention is less likely to reduce caseload than we initially might think. Section four addresses the other major policy response—discharge by allocating resources to those in the caseload—and explains why caseload dynamics is crucial for allocating effectively. The paper concludes by explaining some limits and opportunities in using caseload dynamics for policymaking.

2. Caseload dynamics: homeless shelters

Every day, hundreds of people are entering and leaving shelters. Of the entrants, most are coming in for the first time; others have hit bottom before. Of those leaving, some have stayed just one night, or maybe a week; many have been sheltered for months. The rest may never leave. In this way, homeless people are no different from those entering and leaving either other assistance programs, such as welfare and foster care, or fiscally problematic statuses such as poverty or unemployment. And so the same principles of dynamics are instructive for policy making and implementation in all these areas.

These principles can be summarized in the determinants of population size: the *number* of people entering a condition and the *rate* at which people already in that condition leave. The following equation

[2] Policymakers and public administrators often emphasize reducing caseloads, hence we take this goal as a given to explain the utility of understanding dynamics in policymaking. As the conclusion explains, however, understanding caseload dynamics has a policy utility beyond this particular objective, and, indeed, is useful for achieving the unorthodox, but perhaps politically important, goal of effectively increasing programmatic or conceptual caseload size. Also, we use shelters for the analysis, but this should not be taken to mean shelters define homelessness. A "conceptual caseload" of homelessness could include those on the streets, in abandoned buildings, and doubled- and tripled-up with friends or relatives. We simply do not have good enough measures of these populations and their caseload dynamics to analyze. It would be an important step in resolving many social welfare problems to develop these measures.

shows how this is so, assuming both the number and rate are constant:

$$S = E * (1 - R) * (1 - (1 - R)^n)/R$$

where

S	=	size of caseload at a point-in-time
E	=	number of entrants per time period (e.g., per month)
R	=	exit rate of entrants per time period
n	=	number of time periods caseload has existed

Sometimes populations grow or diminish because just *E* or *R* changes, sometimes because both do; and the comparable impact of each may not be the same. Other times, population size remains unchanged because one factor corrects in response to changes in the other, as when the exit rate increases to match the increase in entrants (see, for example, Wulczyn et al 1990a, 6). In homelessness, the first determinant can be understood as the number of people entering shelters, say, each month, and the second as the probability of shelter users leaving each month. In making homeless policy, as in foster care, welfare and other areas, knowing which determinant(s) is causing the population to grow (or decline) is obviously crucial. And an unchanging population can mask an underlying dynamic policymakers may not want to maintain. Knowing what causes each determinant and what policies will correctly affect each is critical to making sure caseloads do not grow. Further, the values both statistics take are related to the difference between the discounted value of benefits, such as subsidized permanent housing, and their discounted cost, the circumstances of shelter living and the qualifying shelter length-of-stay.[3] The next section builds on these insights by considering prevention policy, commonly thought to be a useful strategy for reducing the number of shelter (caseload) entrants.

3. Prevention: logic and evidence

The logic underlying prevention initiatives is mischievously simple: identify people before they have a specific problem and provide what they need to avoid the problem. Prevention, then, requires knowing two things: who has a strong likelihood of having the problem and what services work to help people avoid it. In homelessness, we know little about the first and virtually nothing about the second. As a consequence, the argument that prevention can be a major tool in combating homelessness, in helping people avoid homeless shelters, is vastly overstated (e.g., Schwartz et al 1991, 8; Lindblom 1991; 1996).[4]

Identification. Becoming homeless is rare. Evidence suggests that 3 percent of people living in the United States have been street or shelter homeless over a five year period (Link et al 1994), and perhaps somewhat less than one million adults without children are homeless at any one time (Burt et al 2001).[5] Even if all homeless people (including children) come from households in poverty—a likely source of homeless adults—about one in sixteen poor people would become homeless (Burt et al 2001). And not all homeless people come from poverty.

Two studies of would-be homeless families in New York City show, practically speaking, about 30 to 40 percent of families entering a homeless shelter may be able to be identified in advance.[6] One collected

[3] Here we are concerned only with the housing subsidy benefit, but we state the proposition generally since this may be only one such benefit to be gained from entering a shelter.
[4] The effectiveness of prevention depends, in part, on how we define homelessness. Consider a definition that includes people on the streets and in shelters, and another which also incorporates those doubled-up. Because doubling-up, rather than living on the street or in shelters, is a more common response to losing one's home, a greater percentage of a larger pool of would-be homeless people could be prevented from becoming homeless under the second definition.
[5] Here and below, Burt et al (2001) data are for 1996.
[6] To our knowledge (and that of Shinn & Baumohl 1998), these are the only homeless entrant identification studies that have been conducted.

survey data from families requesting (not necessarily entering) shelter (Knickman & Weitzman 1989).[7] The OLS prediction model included variables such as family housing history (including prior shelter use), the parents' substance abuse history, current pregnancy or child under one, degree of personal disruptions in the parents' lives, such as child abuse and foster care, and parents' age and ethnicity. The study found targeting three percent of Aid to Families with Dependent Children population (AFDC) would find 30 percent of families requesting shelter.[8] (For the most part, only AFDC families entered shelters at that time.) At the time of the study, over the course of a year, this would mean providing homeless prevention services to about 9,000 families to reach 3,000 who would go into a shelter (assuming all requesters entered).[9] Arguably, this is promising. But it still leaves 70 percent of entering families unidentified, and 60 percent of families receive prevention services they do not need to avoid entering a shelter.

Moreover, the model is less useful than it initially appears. The variable "current pregnancy", for example, had the largest coefficient, and the authors include it among the "at-risk" factors on which the public assistance population could be sorted for vulnerability to homelessness. But its statistical importance is almost certainly more an artifact of New York City policy than of social and economic conditions. At the time of the study, city policy gave shelter priority to pregnant women, raising the likelihood they would be more likely to enter a shelter, other things being equal. Absent this policy, the model would have predicted much less well. When the policy was changed, the proportion of entering families with a pregnant mother decreased.[10] More important, the data intensity of the model makes it impractical. The model requires frequent, extensive surveys of all public assistance families that local agencies may find too costly or time consuming to carry out.

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[7] The survey was conducted on families formally *requesting* shelter at social welfare offices, not on families actually entering a shelter. Not all requesting families decide to enter. At the time of the survey, about 20 percent of requesters did not enter a shelter on the day they requested it, though they may have entered at a later date. Whether requesters differ from those who enter is unknown. The study assumes, i.e., the estimates reported here assume, there is no difference.

[8] Knickman and Weitzman also developed a model that identified three percent of the AFDC population to find 42 percent of families requesting shelter. The sensitivity of the survey questions on which the study is based, however, combined with the problems of the model reported in the text, makes this model even more impractical.

[9] Given the transformations involved in changing AFDC into TANF, adapting these estimates to current populations is not straightforward. The large reduction in the New York City welfare (AFDC/TANF) caseloads suggest that these rates would improve, all other things remaining unchanged.

[10] Pregnancy could also cause a shelter request if the mother is living with a boyfriend who refuses to support her and the child; or if the mother is living with a relative or friend, pregnancy could cause such household problems that she is asked to leave. If such scenarios figured largely in pregnant women requesting shelter, the regression coefficient would be a result of social and economic conditions and not New York City policy.

A more recent analysis using the same data found a best model that identified 66 percent of actual shelter requesters and misidentified 10 percent of the population of shelter requesters (Shinn et al 1998). This suffers from some of the same problems as the prior analysis, in particular, the importance of pregnancy in the model. And from a policy perspective, Shinn and Baumohl (1998) point out that, employing this best model and using New York City population sizes at the time of the study, prevention services would have to be provided to 27,000 families who would not request shelter to reach the 6,000 that would. Eighty percent of prevention services would go to families not in danger of entering a shelter (though such services might be otherwise useful).

Using more readily available data from New York City's social service records, a second study found that families who had moved at least twice before entering a shelter and who had their welfare eligibility ended (and reinstated) at least twice were more likely than other AFDC families to enter a shelter. These criteria fit 12 percent of New York's AFDC population, among whom were 42 percent of the families entering shelters (Towber & Flemming 1989, 3). Such identification would be useful in reducing the number of families entering shelters, *assuming effective prevention services*. Still, the sensitivity rate would be about 60 percent and the specificity rate would be over 85 percent.

Many homeless—and other social service—prevention programs try to solve the identification problem by using "late intervention" models. The model calls for helping a family or person when the loss of their home is absolutely imminent or has recently occurred, usually due to landlord eviction. Compared to servicing, say, all extremely poor people, this obviously increases the chances of finding those likely to enter a shelter, but probably not by much. Instead of going to a shelter, for example, evicted people may double-up or move to cheaper or other housing. A New York City study reported 75 percent of the households in its sample who lost housing did not enter a shelter but stayed with friends and relatives until they found their own permanent housing (Towber & Flemming 1989, 6). And perhaps only 25 to 30 percent of homelessness is due directly or indirectly to landlord eviction (New York State Department of Social Services 1990, 14).

A final problem is these studies only try to distinguish potentially homeless *families*. But because homeless families are less mobile than homeless adults and because they tend to come from the population of families already known to local welfare agencies, identifying and finding would-be homeless families is much easier than identifying and finding similarly fated single adults. And adults without children make up about 85% of those who become homeless (Burt et al 2001, 57). Thus, the overwhelming majority of would-be homeless households will be much harder to distinguish and locate than these family studies suggest.

These problems are not unique to homeless prevention. In foster care prevention, Illinois' Family First program used criteria that identified children at "imminent risk of placement", criteria that identified families far along in their deterioration (comparable to "late intervention" in homelessness). Yet only seven percent of these children would be expected to be placed into foster care during the first month of the program; sixteen percent by the sixth month (Schuerman et al 1993, 102-103).

Services. Even assuming we could identify those likely to become homeless, what can be done to avoid that outcome? Most prevention programs deal with specific housing problems, particularly the threat of eviction, and typically offer one-time rent, utility or mortgage payments, financial counseling or landlord-tenant mediation. Evidence that these and other prevention services work, however, is weak or non-existent.[11] Programs or studies do not report comparative data specifying what proportion of those helped would have stayed housed absent the program (see, for example, Schwartz et al 1991; Feins et al 1994). Critically, most programs do not follow-up to see whether or not assisted clients remain housed. In

[11] Shinn and Baumohl (1998) reach a similar conclusion and provide a more comprehensive explication of these programs and the problems with their effectiveness claims than is possible here.

reviewing 42 programs, the United States Government Accounting Office (GAO) found only eight that followed-up, and the agency found the follow-ups were not done well enough to support programmatic claims of success (GAO 1990). Further, these claims usually do not distinguish between being evicted and becoming homeless by entering a shelter. One study making this distinction, a study of late-intervention prevention programs across New York State, projected that 12 to 16 percent of those assisted would have entered a shelter (New York State Department of Social Services 1990, 19-21). But the estimated proportion of evictions that would have been averted absent prevention programs is not based on experimental data. Rather, service providers supplied the data, and since programs were not fully operational, the numbers are projections of what service providers expected would happen. Typically, service providers are likely to overestimate their abilities, do not track their efforts very well, and have incentives to present an optimistic picture. And we do not know from the study whether assisted families stayed housed.[12]

Last, most prevention services address problems of people who do not make up the bulk of those homeless at any one time—people homeless for over a year and suffering severe problems. These are mostly destitute men who are mentally ill or physically disabled or use illegal drugs (or some combination; Burt 1992, 17-30). But services typically address the short-term housing problems of families with incomes far above those of the average homeless person or family (Schwartz et al 1991, 7). And there are logical problems as well. The section on caseload dynamics shows prevention programs' impact on population size decreases to the extent they service those who would enter shelters for shorter rather than longer periods of time. This is because longer-term users are most responsible for caseload size at any one time.[13]

Evidence from foster care also shows how difficult it is to provide effective preventive services. Illinois' Family First programs, for example, worked intensively with targeted families for several months, providing an array of material and social psychological help. Using a two group, random assignment design, one study concluded services did too little in the face of deeper economic and social ills, such as poor job prospects, and did it too late in the process of family disintegration to reduce the number of foster care placements (Schuerman et al 1993, 169).

This conclusion points to a troubling issue for social welfare prevention. Waiting until a problem is imminent improves targeting and lowers costs, but it may also mean waiting until the problem becomes too difficult for prevention services to resolve. The earlier the intervention, however, the more service provision becomes costly, intrusive, and politically and practically difficult. If public officials are to use prevention to reduce caseloads, the intersection of these continua that is optimal for reducing entrants has to be better calibrated. And, as we will now suggest, even then we cannot now say such a point will yield a large reduction in entrants.

Evaluating prevention effects. One simple way to estimate a "preventive effect" *(P)* is to multiply the proportion of would-be homeless single adults and families identified in advance *(I)* by the proportion avoiding shelters due to prevention program services *(E)*, i.e., $P = I * E$. Available data allow only rough, but arguably not unrealistic, estimates of these parameters.[14] We assume programs can identify 40

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[12] Even if we expand the concept of prevention programs to include structural programs such as providing permanent housing which may not have the problems suggested here, to the extent that such programs rely on a static supply of permanent housing, they do have the problem of merely reallocating homelessness among the population of very poor people (Shinn & Baumohl 1998).

[13] We do not know enough about how people become homeless to say conclusively that those with short-term problems, e.g., problems responsive to a one-time infusion of money or legal services, would not become long-term homeless if that problem were not resolved. Redburn and Buss (1986) suggest that short-term adult shelter users may have been evicted or suffered a singular disaster or personal crisis, whereas long-term users may have serious mental or physical disabilities or other dysfunctions. See also Kuhn and Culhane (1998).

[14] The identification estimate for families is the highest given by the New York City models reported in the text. The

percent of shelter-entering homeless families and 30 percent of entering homeless single adults. We further assume services can keep 45 percent of families or assisted persons in their current housing, move them into other permanent housing, such as a residential facility for the mentally ill, or get them other services that work to prevent entering a shelter. Using Burt et al's (2001) estimate that 85 percent of homeless households are single adults and that 15 percent are families, we can thus estimate P = .14 = ((.15*.4)+(.85*.3))*.45.[15] Arguably helpful, 14 percent is not the major impact on homelessness prevention's proponents aver.[16]

Another way to estimate a preventive effect uses the logic and evidence of random assignment studies. Social welfare programs are often thought to "work" even when such studies show statistically significant differences of 5 to 10 percent between experimental and control groups. Should this perspective affect the previous conclusion concerning prevention's small caseload effects?

To begin, this experimental difference is not the same measure as the preventive effect described above. Realistically, the preventive effective for homelessness—and probably for other caseloads—will be less than the experimental difference. Most of the reason for this is that those in the experimental group whose shelter or caseload entrance could be affected by preventive services do not constitute all would-be entrants. They are some subset identified by the program's targeting criteria. The lower the proportion of all would-be entrants who are identified, the less likely is the preventive effective to be equal to or greater than the experimental difference. Optimistically, this number could be as high as 40 percent and would still be too low to raise the preventive effect above the experimental difference.[17]

To see why, let's assume a 10 percent difference between experimental and control groups in a hypothetical, random assignment, homeless prevention study. In the context of random assignment, social welfare experiments, this is a relatively strong difference and so we may take it as an upper bound for experimental/control differences. We designate this effect as X_1. However, arguments have been made that this underestimates an experiment's impact. If only half the control group needed services to avoid entering a caseload, the effect of the experiment on those *who could be affected* would be 20 percent (0.1/0.5). Let's designate this effect as X_2 and take the 20 percent as an upper bound for this measure. Now, this latter

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estimate for single adults is three-fourths the family estimate: the social amorphousness of this population combined with its mobility suggest that at-risk adults are harder to identify and find. The estimate for the proportion effectively assisted arbitrarily assumes that program services addressing *all* reasons for homelessness would be half the estimated effectiveness of services that address landlord eviction (New York State Department of Social Services 1990, 19). Because eviction usually has to do with rent or other landlord/tenant disputes, it can be resolved with a grant or with legal representation, both relatively quickly and easily provided. For people with mental or addiction disabilities or social problems, prevention services are much more difficult to provide and take a longer time to administer, suggesting they will not be as effective as eviction prevention.

Identifying and finding those at-risk could be improved by using institutions would-be shelter users pass through, e.g., jails and mental health and other hospitals. In theory, these places could identify and hold on to would-be homeless single adults and prevent their becoming homeless by, for example, placing them in a residential mental health facility or getting them to some kind of prevention service. At present, these places do not do these things and it may be unrealistic to expect they will. For instance, mental hospitals, among all these institutions, would seem most likely to take some responsibility for the whereabouts of their discharges. Yet some do not have out-placement services and others do not follow through on the ones they do have. If mental hospitals cannot be convinced to do these things, what chance have we of getting other institutions, such as jails, to do so?

[15] Burt et al's (2001) estimates are for those homeless on any day (point prevalence). Although Link et al (1994) do not break out their five-year and lifetime prevalence rates by type of homeless household, an implication of their findings is that homeless adults households dominate for these periods as well.

[16] Also note that this ignores the inefficiency of a high specificity rate: identified families getting services who would not have entered shelters.

[17] This analysis assumes experimental effects calculations *(X_1)* based on everyone in the control and experimental groups and not some subset, such as program participants. Thus, it somewhat underestimates X_1.

measure is equivalent to E in the equation estimating the preventive effect *($P = E * I$)*. That is,

$$E = X_1 / (1 - C)$$

and so

$$P = I * (X_1 / (1 - C))$$

where

- E = effectiveness of prevention programs
- X_1 = the experimental effect expressed as a difference in proportions between control and experimental groups
- C = the proportion in the control group not needing a program's services to avoid entering a caseload
- P = proportion of would-be sheltered entrants deterred from entering ("preventive effect")

These equations tells us several things about the relationship between the preventive effect and the two measures of experimental effects:

(a) since $E = X_2$, the preventive effect P will never be greater than X_2, the larger experimental effect measure;

(b) however, except when all would-be entrants are identified *($I = 1$)*—a singular condition—the preventive effect will be less than X_2;

(c) whether P is greater or less than X_1 depends mostly on whether the proportion of all entrants who are identified *(I)* is high or low, respectively. The proportion of the population needing services to avoid entering a caseload *($1 - C$)* is much less important. Obviously, however, the greater this is, the more likely is P to be less than X_1. The preventive effect will equal the difference between the experimental and control groups *(X_1)* under the extremely unlikely condition that $I = 1$ *and* $C = 0$.

Thus, to restate our point, current knowledge about homelessness suggests I is unlikely to be greater than 0.4. And it is rare in social welfare experiments for C to be less than 0.5. That is, 50 percent of the experimental group can avoid some specified outcome without the tested program's services, and no more than 40 percent of all would-be shelter entrants can be identified by current targeting. These optimistic parameters show a preventive effect just under $X_1 = 0.1$. For a more likely set of parameters, i.e., $I < 0.4$ and $C > 0.5$, the preventive result will be much less than 10 percent. This suggests our rough estimate of a preventive effect of 14 percent may be optimistic, and, more generally, *if prevention's goal is to reduce caseloads by deterring a large proportion of would-be entrants*, its effects now seem too slight to achieve this aim.[18]

More important, what does it mean in this context for a prevention program to "work"? One way to answer this question is to use a broad cost-benefit perspective that incorporates societal, program participant, and non-participant perspectives (Kemper et al 1983). The first evaluates all benefits and costs to society as a whole, ignoring the transfer of benefits and costs among groups within the society. The second weighs the intended and perhaps unintended benefits and costs to participants. And the third values the impact on government budgets, or more completely, the effectiveness of the government's investment in the program. Proponents of homeless prevention programs argue that they produce positive cost-benefit outcomes from one or more of these perspectives. Few, however, have tried to formally make estimates from one of these perspectives. More important, cost-benefit analysis is not directly relevant to our argument. A large cost-benefit ratio does not necessarily mean prevention works to reduce would-be entrants.

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[18] This assumes prevention deters randomly with regard to caseload length-of-stay. If prevention diverts longer-term or repeat users, its impact on caseload size might be great even if it prevents a small proportion of entrants. But if prevention deters shorter-term users, it's impact will be less than under the random assumption. Because no evidence exists on this point, we make the random assumption. The logic of this argument is explained in the next section.

Taking the governmental cost-benefit perspective, for example, many suggest prevention is more cost effective than shelters for government budgets and so should be a primary tool in dealing with homelessness. In the only cost-benefit study we know, the New York State Department of Social Services (1990, 23) estimated a budgetary savings of $4 for every $1 spent in homeless family prevention programs. But, as noted earlier, because this study relied on estimates and projections from service providers and because it lacked experimental and follow-up data, the reported benefit-cost ratio is probably greater than the true ratio.[19] Studies of homeless prevention programs require better data than currently exist to make useful cost-benefit estimates. The obvious fiscal appeal of prevention's logic does not mean savings will necessarily be realized. This claim must remain open.

Moreover, fiscal cost-benefit analyses show whether prevention programs save the government money, not that they can reduce the number of sheltered people. If shelters are very expensive and preventing homelessness is very cheap, the benefit can be much greater than the cost without our necessarily witnessing a large drop in the proportion of shelter entrants. Welfare to work programs, for example, can make fiscal sense because they cost very little to run and reduce slightly the cost of the average welfare grant. But they can have very little or no impact on welfare caseloads (Friedlander et al 1993, 42-56).

But even if cost-benefit calculations showed prevention programs cost the government more than shelters, prevention programs might deserve support. The social benefits could outweigh the fiscal (and social) cost. Shelters may be worse than a person's current living situation, and the path to shelters can be long and painful, winding through physical disabilities, the homes of relatives and friends and through harsh circumstances that people, some of them children, should not have to endure. Note, however, these and other social benefits can be obtained without much of an impact on the number of shelter entrants.

Last, some argue prevention programs should be funded because they help people in need, even if they do little to reduce homelessness. (See, for example, NYC Family Homelessness Special Master Panel 2003, 37.) And we would agree—prevention programs probably do help impoverished families and individuals, few of whom would ever enter a shelter. Similarly, foster care prevention programs provide "considerable benefit" to very troubled families even as they have no effect on foster care placement (Schuerman et al 1993, 163-168). This suggests prevention programs be evaluated and supported on their ability to provide needed help to families who would otherwise not be assisted.

[19] Making its own count, the Human Services Administration of New York City, for example, had a smaller estimate of the number of program beneficiaries in New York City and of its impact on shelter use (McAllister 1994).

4. Allocating Resources

Policy choice. A straightforward and oft suggested homeless policy is to provide subsidized permanent housing to all those already homeless. How would this be done? To start, even if the definition of homelessness is limited to those living on streets or in shelters, there have historically been far more homeless than newly available subsidized or nonprofit housing. During the course of a recent year, perhaps as few as 870,000 and as many as 2.1 million adult and family households are street or shelter homeless (Burt et al 2001). And these estimates do not include the hundreds of thousands of displaced single adult or family households who are homeless or live doubled-up (Nelson and Khadduri n.d., 9).[20] However, net new rental commitments (Section 8 certificates, vouchers, loan management set aside and other programs) have averaged less than 100,000 units in recent years (Dolbeare & Crowley 2002), and useable annual turnover in these and public housing subsidies may add about 500,000 units (Nelson 1994).[21] Clearly the amount of newly available subsidized housing is not enough to house all homeless households, even if the housing needs of other populations of poor people were completely ignored. And, of course, these subsidies could not just be targeted to homeless people. They also are expected to be used by other populations, including five million with "worst case" housing needs (Bratt 2002).[22]

Moreover, these subsidies usually do not to go to poor single men and women, the kind of household making up 85 percent of homeless households. Local governments and nonprofits allocating subsidized housing to homeless people have generally limited permanent housing subsidies to shelter dwellers.[23] Given the large number of households generally eligible for federal housing subsidies, however, even this strategy cannot meet demand. Eligible households might be especially interested in the subsidized

[20] Because income is underreported on the American Housing Survey (AHS) that is used to derive the estimate of "worst case" families, both the number of very low income renters and the proportion of renters with severe rent burdens may be high. In the past, HUD has estimated that the income of very low income renter households was underreported by at least 15 percent (HUD 1991, 46). This underestimate is somewhat offset by AHS not counting homeless families and single adults or households that are about to be displaced. In addition, are millions of other households with "priority problems"—rent burdens greater than 50 percent or severely inadequate housing—who do not qualify as "worst cases" because they were not unassisted renter households with incomes less than 50 percent of the local area median (Nelson & Khadduri n.d., 9).

[21] Nelson analysis based on 1991 and 1983 American Housing Survey. At that time, although one million holders of Federal subsidies, including public housing, moved into their current housing in the year prior to the survey, perhaps half already held subsidies (Nelson (1994). These estimates probably do not include Farmers Home Administration subsidies. Since homelessness is much more an urban than a rural problem, turnover in rural subsidies would be little help.)

The homeless population estimate is based on 1996 survey data and evidence suggests it has probably increased since then (Bratt 2002). Because the National Affordable Housing Act of 1990 includes nonelderly single adults among households eligible for Section 8 housing subsidies, we use the term "household" to mean single persons as well as families. Since state governments generally do not provide housing assistance to very poor households, we report only Federal programs (Shapiro et al. 1991, 49. One exception is New York State's New York/New York program for people who are homeless and mentally ill. See Culhane et al 2002).

[22] Worst-case estimate is for 1999. Worst case means households that rent their dwelling; do not received federal housing assistance; have incomes below 50 percent of area median family income; pay more than 50 percent of their reported gross income on rent and utilities or live in physically substandard or overcrowded housing (HUD 1996, 1). The estimate excludes homeless households. The number of households is a point-in-time estimate. Most households qualify as "worst case" because of excessive rent burdens, which HUD may be overestimating. Officially, however, this remains the pool competing with homeless households.

[23] Although this is effectively the situation, governments and nonprofits did not create shelters in order to allocate housing. Rather, as their shelter populations burgeoned, both decided to provide permanent housing as a way to control shelter size. But see the discussion in the conclusion regarding the importance of caseloads for turning personal problems into public issues.

housing they could gain by a shelter stay. In the face of these conditions, trying to “end homelessness” by providing housing subsidies to all who are sheltered creates an incentive to enter shelters to obtain the subsidies for which people qualify but are now without. The rest of this section explains why this is so and then report supportive evidence.

How much providing housing subsidies to shelter dwellers causes qualified people to try entering shelters is governed by the difference between the discounted value of those subsidies and the discounted cost of obtaining them. This difference will depend on the "nominal" value of the subsidized housing to a person, the circumstances of shelter, and the amount of time someone has to stay sheltered before obtaining the housing.[24] The nominal value of the subsidized housing is how well that housing compares with a person's current housing situation—its physical quality, rent burden, locale, social circumstances and the person's tastes. The larger the difference, the greater the value. Shelter conditions are also evaluated relative to a person's current housing situation and include the characteristics and physical quality of the shelters; required participation in social service programs; and regulations governing overnight guests, alcohol consumption, drug use and other aspects of daily life. By varying these conditions as well as the amount of time a person has to spend in a shelter before obtaining permanent housing, governments and nonprofits determine the cost of obtaining a housing subsidy.

Obviously, then, the cost-benefit difference increases as the time to obtain benefits shortens or shelter conditions "improve". As the wait for a subsidy approaches one shelter day or as shelter living becomes more like a person's current housing, the more will people seek to enter a shelter. Allocating permanent housing to more recent shelter entrants—by assigning housing randomly or by trying to house everyone who is sheltered or judged ready for permanent housing—would make more amenable the cost of a shelter stay for many of the millions of unsubsidized households who are not homeless yet qualify for subsidies. This would greatly expand shelter demand.[25]

Examples of this effect occurred in three areas where shelter agencies specified or would-be shelter entrants perceived shelter occupants would gain subsidies after a short or virtually no shelter stay. In the late 1980s, St. Louis County, Missouri informed families waiting for Section 8 vouchers the Department of Housing and Urban Affairs (HUD) had established shelter occupancy as a priority criterion for vouchers. Families thought a shelter stay of any length would qualify them, causing the demand for shelter to become so great that the City of St. Louis began to require entering families to document they were city, and not suburban, residents.[26] To further slow the flow, the city turned away those who stated as they were being assessed that they sought shelter to meet the HUD criterion (Daily 1993).

In Massachusetts, emergency preference or set-aside rental subsidies were not available for most families at-risk of homelessness. As a result, when the state established a special rental assistance program for sheltered families, it "had the unintended consequence of encouraging some desperate families to become homeless . . . to access scarce housing subsidies" (Stegman 1991, 258).

[24] Exactly how potential shelter entrants would discount future subsidies and more imminent costs is unknown, but understanding how they do so is not critical to the point being made.
[25] The logic is the same for varying shelter conditions. Shelter time can be considered a shelter condition.
[26] Some families, for example, entered shelters and immediately requested a letter attesting to their shelter stay; they left just as soon when told the shelter would not issue the letter. Since St. Louis County did not have any shelters, suburban residents would try to enter city shelters. The city eschewed informing its residents of HUD's policy because it feared the shelter influx that the county's action caused.

And in the spring of 1990, New York City began providing subsidized housing to families who had been sheltered three months; less in some cases. As a result, from April through October, when the policy was changed, the monthly number of families entering shelters increased an average of 17.2 percent over the same months in the previous year.[27]

These examples suggest that policies providing subsidies to recent shelter entrants will cause shelter demand to burgeon. Demand expands not because the causes of homelessness have worsened but because households that qualify for permanent housing see shelters as a more effective way to gain subsidies. Providing subsidized housing to sheltered people cannot be a general solution to emptying shelters. Given millions of qualified households without subsidies, it would be impossible to provide permanent housing to all who are sheltered without inducing households to enter shelters.

The next section supports this argument by analyzing a particular policy moment in New York City when dynamics was not heeded. It also demonstrates subsidies should go to those sheltered longest. Allocating subsidies in this way lessens, perhaps eliminates, demand caused by the promise of subsidies, ends living in shelters for those who most need permanent housing but are unlikely to find it themselves, discourages people from staying sheltered in order to qualify for subsidies, and uses housing subsidies more effectively in managing shelter size.

Dynamics in action. In the late 1980s and early 1990s, New York City's family shelter system provided temporary housing for any family requesting it and subsidized permanent housing to some sheltered families.[28] Using data from families entering in June 1988 and February 1989, table one reports rates at which families would leave the New York City shelters on their own if the city did not provide permanent housing.[29] To explain: reading across the second row, the chance of a family sheltered for thirty days leaving over the next thirty days would be 0.184; this means 12.8 percent of entering families or 2.0 percent of all families using shelters at any moment would leave—complete a shelter "spell"—between the 31st and 60th day of their stay.[30]

The completed spell distributions of the table show two things relevant to allocating permanent housing. One, within their first ninety days, half the entering families would leave on their own (30.5% + 12.8% + 7.1%), and, two, over 80 percent of those sheltered at any moment would, absent a housing program, be in the midst of spells greater than one year. Then, to allocate housing so as to have the largest impact on shelter size and to allocate it to families arguably most in need (i.e., most likely to stay sheltered), those who would stay sheltered longest should be identified and given subsidies. At present, the only practical way to identify such families is to wait until they have stayed a specified period of time.[31] Because most would leave anyway, recent entrants should not be given housing. That is,

[27] Authors' calculations based on data from the Human Resources Administration, City of New York.
[28] At the time of this case, New York City had an "open" shelter system, where any family requesting temporary housing was given shelter and allowed to stay as long as it wanted. In this case, the demand generated by people's weighing of benefits and costs translates directly into the number of entrants and rates of exit. The logic applies as well to "closed" shelters, where the shelter operator decides whether a family or person is homeless and how long each can stay. In this case, the relationship between demand and the number of entrants exists but is indirect.
[29] To remove the effects of policies giving apartments to shelter users, we added families who went to city-provided housing over the course of the month back into the shelter length-of-stay distributions of shelter users at the end of the month. This was based, of course, on how long those leaving had stayed in the shelters.
[30] These findings are similar to but indicate a slower rate of exit than the probabilities estimated in Culhane et al (1999) using New York City administrative data for the period 1987 to 1995. One reason for the difference may be the different time periods; perhaps a more potent reason is that the probabilities presented here do not include families leaving to City-provided housing.
[31] It would be better if officials could assign exit probabilities to all shelter users employing all available information, not just length-of-stay, and provide subsidies to those with the lowest probabilities. But this is currently impractical, seems legally and ethically dubious, and may create unwanted incentives. Optimally, these exit probabilities would estimate

allocating permanent housing to long-term shelter users has a greater impact on the exit rate than allocating to short-term users and is less likely to attract entrants who would not otherwise seek to enter shelters.

To see why, consider the hypothetical shelter in table two, based on the New York City conditional probabilities. Assuming one hundred families enter a shelter at the start of each month, panel A shows what this shelter would look thirteen to fifteen months after opening. For each entering cohort of one hundred families, the table calculates the number remaining at the end of thirty day intervals, based on exit probabilities shown in table one. Families sheltered longer than 360 days are counted as one group and are assumed to remain indefinitely. (This differs from the probabilities in table one, where the average exit rate for those remaining sheltered more than 360 days is estimated to be 0.0039.)

Panel A shows if resources such as housing are to be allocated to maximally control shelter size, families sheltered longest should receive the housing because they have the greatest chance of remaining sheltered. That is, they contribute nothing to the system exit rate which helps determine shelter size. Panels B and C make clear that if 30 units of permanent housing could be allocated each month, the population could be kept at zero growth rate (size = 508) by giving that housing to families sheltered more than 360 days, (since the shelter grows each month by adding that number of families to the > 360 days group). By extreme contrast, if the housing were randomly given to entering families, ten (30.5 percent) would go to families who would have left anyway, on their own, by the end of the first month, leaving a January population of 517. And, as panel B shows, the population difference between panels A and B would grow each month.

Another way of saying all this is that allocating housing to longer-term users causes a greater overall exit rate (families leaving to their own and to city provided housing) than giving it to shorter-term users. Families who would leave on their own are less likely to leave via subsidized housing, making it more possible for families who would only leave through such housing to get it. Further, because the value of subsidies decreases as the length-of-stay criterion is raised, this policy would increase the exit rate by discouraging families from remaining sheltered in order to qualify for permanent housing. And, of course, the policy would also discourage families from entering shelters to gain access to housing subsidies.

Policymaking that first utilized this logic and then ignored it occurred in New York City in the late eighties and early nineties. Seeking to decrease its shelter population after years of endless growth, city government began to provide subsidized housing to families sheltered at least one more-or-less continuous year.[32] This reduced the number of sheltered families from a peak of 5,305 in June 1988 to 3,686 in March 1990. Most of the decline was due to an increase in the overall exit rate of the shelter system as thousands of families unlikely to ever leave were given permanent housing. During this time, the monthly average number of entrants declined by only 21 families (2.5 percent) over the monthly average

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permanently leaving shelters, not just ending a particular shelter stay. That is, the episodic leavings of repeat users would be ignored, and total shelter time would be used for making probability estimates. Whether allocating benefits in this way makes practical sense, however, depends on the number of repeat users, how long they stay, the comparative cost of providing services and the chance that service provision will induce entrants, among other things. (See Ellwood 1986).

Longer-term users could have characteristics that differentiate them *at entrance* from the rest of the population, or they could be undifferentiated at entrance and the conditions of being sheltered could cause people to stay sheltered. See Hoch and Slayton (1989) for some arguments as to how shelters encourage dependence. Focusing policy on people who have stayed sheltered a long time is undermined if shelters or being homeless causes people to stay sheltered a long time *and* if ways other than waiting can be found to identify would-be longer-term users.

Last, even if we rely only on length-of-stay, the logic of the argument is that subsidies should be given to those sheltered longest, not to those sheltered a certain length of time. Specifying a time threshold, however, is more easily implemented.

[32] This includes families who left the shelters but returned within thirty days.

for the previous year. These are too few families to account, at best, for more than one-quarter of the population decline.

But beginning in November 1989 and quickening in April 1990, the city changed its policy, cutting the length-of-stay criterion from twelve months to nine and then to six months. Further, the policy was applied in a way that lowered the officially required shelter time by as much as three months. And in order to close certain shelters, the city sometimes ignored the length-of-stay criterion entirely. Over the summer of 1990, families who had recently entered shelters were given subsidized permanent housing.

Table three reports the predictable effects of these policy changes.[33] It shows monthly percent changes from the previous year in the number of entrants and in the percent change in the exit rate of families leaving on their own. When the city most aggressively moved out shorter-term users between April and October 1990, the average number of families entering each month increased 17.2 percent from the previous April through October. This was a sharp change from the previous two years when the average number of entrants over this time had first declined 4 percent and then increased 2.7 percent, and from the year after when it decreased 2.6 percent.

Between June and October of 1990, the average change in the monthly exit rates of families leaving on their own showed a 1.9 percent decline below rate changes for the same period the previous year. (The data do not allow estimates to April 1989). June to October 1991 showed a 0.7 percent average increase in rate changes compared to changes for the same period in 1990.[34]

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[33] Although the length-of-stay criterion was initially lowered to nine months in November 1989, this analysis focuses on the period between April and October 1990 when the shortest length-of-stay criterion was applied. This is consistent with the logic of the argument that the shorter the criterion, the more likely the effects. To show what was happening immediately before April 1990 and after October 1990, the bottom panel in table three reports every month of the time series between November 1989 and October 1991. Data are not available to calculate changes in the overall exit rate before June 1990.

Cragg and O'Flaherty (1999) contend that the effects described here were not caused by these policy changes. They argue a host of economic, shelter and other policy changes caused population growth and, moreover, that greater placements would have resulted in a lower shelter population. There are several important differences that account for the different findings. First, Cragg and O'Flaherty are interested in explaining the three year shelter population trend from 1990 to 1993; we are more interested in the rise immediately surrounding the policy change. Second, their measure of time to permanent housing (queue) is more blunt than our analysis and does not take shelter dynamics as fully take into account. Their measure, for example, incorporates long term shelter users who were not able to be placed, and does not measure our contention that the queue expectation dropped greatly at a specific point in time for a certain potential shelter-using population. Third, their method requires that queue not be endogenous, i.e., that individual would-be shelter user time-to-placement expectations not be generated by the increase in permanent housing placement rates by worried policymakers. Such endogeneity is exactly our contention, rendering the model problematic. Wackstein's (1991) report of policymakers thinking and behavior at this time supports our contention.

[34] Although the analysis does not examine other possible explanations for these changes, the more obvious ones appear remote. No other major policy change nor external event occurred in the spring of 1990 or had occurred in the recent past. For the number of entrants, systematic longer-term behavior consistent with changes in entrants seems unlikely. For example, the monthly percentage change in the number of entrants over the twelve month periods were:

April 1988 - March 1989:	- 5.2%
April 1989 - March 1990:	+ 4.6
April 1990 - March 1991:	+12.1
April 1991 - March 1992:	- 0.7

In the three years prior to April 1990, the yearly average number of entrants varied less than three percent from the three year average of 826. Between April 1990 and March 1991, the average number of entrants was 935 and stayed at that level through March of 1992. The time series for all years shows seasonal increases in July and August. The exit rate time series does not go back far enough to comment on systematic long-term behavior as an explanation.

Two additional pieces of evidence support the proposed explanation. Using an informal study her office

Table four compares the effects of these policies on shelter population size. It shows how much lower the monthly population would have been between April 1990 and March 1991 if (a) the number of entrants had stayed at the same monthly levels as April 1989 through March 1990, (b) the exit rate of families leaving on their own had remained the same as the average rate between June 1989 and March 1990, and (c) both conditions had existed. By March 1991, the shelter population would have been 714 (entrants effect) or 672 (exit rate effect) less than it actually was.

Importantly, the analysis also shows changing the exit rate has a much greater impact than changing the number of entrants. The 12.1 percent increase in the total number of entrants over this period led to a shelter population that was 19.1 percent greater than if this increase had not occurred. But the decrease of 1.8 percent in the average exit rate made the shelter population 17.8 percent larger than if this slow down had not occurred. A small change in the exit rate goes a long way. The importance of improving exit rates to reduce population size was also found in simulations of foster care populations. Increasing the chance of a child being discharged by 10 percent cut the foster care census by 8.4 percent over the base scenario. By contrast, preventing 4.5 percent of placements decreased the number of children in foster care by less than 1 percent, compared with the base scenario (Wulczyn 1990a, 31; Wulczyn 2003).[35]

Clearly, in homelessness and in foster care, as in welfare, reducing population size means paying attention to how people come and go, but especially to those who do not leave. The analysis further suggests the limited utility of prevention to lower population size, due likely to the kinds of people being prevented. For example, using the New York City data, if prevention programs randomly deter entrance, half those deterred would have left shelters on their own within three months and, thus, not contributed much to population size.

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conducted, the head of the Mayor's Office for Homeless and Single Room Occupancy Housing for New York City testified that the parents of entering families reported that they had learned they could get subsidized apartments by entering a shelter (Wackstein 1991). And the experience of the New York City family and adult shelters has been that large, dramatic shifts in the rate at which the population grew or declined were always occasioned by policy changes, such as lowering the length-of-stay criteria, not by external events or long-term systemic forces. For example, the population in New York's single adult shelters fell an average of 7 percent in the six months following April, 1989, after rising by an average of 6 percent the previous five months. This change, like the one recorded in the analysis of family shelters, occurred too dramatically for longer-time factors, such as economic or housing changes. The most likely cause was a policy implemented in April that initiated assessment and assignment to shelter and so limited shelter users' ability to access shelters when and as they wished. (All comparisons are to the same month in the previous year. Data limits prevent comparisons before November 1988.)

[35] As Wulczyn (2003 and private communication) points out, it is not always the case that exit rate change will be more effective for reducing the population. In particular, when the ratio of the population at the beginning of a time period (e.g., a year) to the number of new entrants over a similarly measured time period (e.g., a year) is below 1.0 and the exit rate of would-be entrants to a caseload is not "high", preventing new entrants can be more effective. The work has not yet been done to specify more precisely what "high" means.

5. Conclusions

To explain the utility of caseload dynamics for policymaking around prevention and resource allocation, we have focused on a common goal of policymakers and public administrators: reducing caseload size. In practice, we could argue with such a singular focus and yet observe the policy utility of caseload dynamics.

First, caseloads are a narrow construction of a problem and are more about the fiscal and organizational needs of government and other organizations than about the lives of actual people. For example, we can consider individuals homeless even if they are not living in a shelter, but rather on the street or doubled- and tripled-up in the home of a friend or relative. Making policy for such conceptual caseloads, however, does not exclude our need to pay attention to caseload dynamics. If we can think of individuals in a certain status as a caseload, we can use dynamics to better utilize resources to reduce caseload size, that is, to reduce the problem more broadly conceived. Thinking of problems as caseloads points out the importance of measurement and its intimate relationship with theory, and how both result from political constructions of social reality.

Second, even if we only consider programmatic caseloads, such as shelter users, we might want to expand rather than contract them. This seems peculiar. Don’t social welfare caseloads indicate people suffering from some problems that they, and we, would rather they not have? Put another way, we might think of caseloads as gathering together people who suffer a common condition—being poor, having no home, having abusive parents, having no work and so forth. That is, caseloads make public the personal problems people face. In this sense, caseloads are ways to transform individual problems into public issues and, as such, represent particular constructions of those individual problems. We have argued, for example, family homelessness can be understood almost entirely as a result of the existence of family shelters (Berlin & McAllister 1994). We do not argue shelters cause their problems, but that these families may not be known to the government and thus addressed by public policy except for the existence of shelters and that these problems become understood as “homelessness” because of the criteria for entering a particular caseload, the shelters. Thus, in transforming personal, individual problems into public caseloads, we might want to expand government caseloads as a way to address these individuals' problems. Understanding dynamics can here be used to wisely expand caseloads so that size does not overwhelm resources able to be committed and, perhaps, to identify those individuals and families most needing assistance.

Third, dynamics helps illuminate policy goal-setting. It discloses, for example, that a strong conflict can exist between reducing the population of a shelter and giving immediate, significant, long-term help to shelter entrants. Choosing caseload reduction can effectively mean telling mothers who have children but no place to live that we are not very interested in their problem. This should be understood as a painful choice, and should lead us to be generally suspicious about caseload reduction for its own sake. In this way, policies in tune to dynamics may be relatively inexpensive stopgap measures that try to manage a situation rather than resolve a problem. They make clear what we are not willing to do to respond to the social and economic ills underlying these problems.

Fourth, the relationship between prevention and resource allocation may be used effectively to make policies. The analysis presented in this paper, for example, has ignored the issue of multiple entrants onto caseloads—families, for example, who leave shelters even within the first several months only to return many times, perhaps becoming long-term continuous users or long-term chronic users. Policies targeting such multiple entrants and providing them with aftercare services is one example of taking the logic of prevention to reduce the exit rate and, thereby, more generally respond to homelessness, (though our current ability to target or to provide effective services is unknown).

Fifth, although our analysis strongly questions the current utility of prevention, it does not argue useful prevention is impossible. Rather, the analysis observes that, in practice, prevention is much more complicated than we may like to think. We simply do not know enough about either who is likely to enter a shelter and what services work to for prevention to have a large effect on the numbers entering a caseload. (See also Shinn & Baumohl 1998.) Moreover, given scarce resources, if prevention is to affect caseload size, we really need to identify the population of would-be long-term shelter users and to provide services before their problems become so severe that typical preventive efforts would fail. These are obviously calls for more and different research, but there is simply no way around it if we want prevention to live up to its aphoristic claims.

And last, because prevention cannot now work very well to reduce caseloads, justifications for such programs make unsupportable prevention claims. This is the sort of thing that gives social welfare initiatives a bad name. When prevention efforts fail to reduce caseloads, it becomes more difficult to generate support for other kinds of programs thought useful for addressing a problem. Better to base arguments for programs on their actual impacts. If we cannot prevent caseload entry but can help people in other ways through programs we currently label “preventive”, we should still carry out those programs but measure their effectiveness and justify their existence by something other than caseload reduction.

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Table 1. Exit rates and homeless spell distributions: New York City family shelters, 1988-89

		Completed spell distributions	
Length of Stay	**Exit Rate**	**Entering Families**	**% Shelter Pop. at Point-in-Time**
1-30	0.305	30.5%	5.2%
31-60	0.184	12.8%	2.0%
61-90	0.125	7.1%	1.2%
91-120	0.105	5.2%	1.0%
121-150	0.045	2.0%	0.3%
151-180	0.031	1.3%	0.2%
181-210	0.051	2.1%	0.3%
211-240	0.072	2.8%	0.5%
241-270	0.055	2.4%	0.3%
271-300	0.035	1.2%	0.2%
301-330	0.012	0.4%	0.2%
331-360	0.058	1.9%	0.2%
>360*	0.039	30.0%	88.5%
Totals		100.0%	100.0%

Source: Human Resources Administration, City of New York.

Note: Rates are estimated for a shelter system in which all families leave only on their own and are based on a composite of data from families entering in June 1988 and February 1989.

* Assumes steady state value of 0.039, based on the average exit probability of the first 5 thirty day periods after 360 days. At this rate, no shelter stay would last more than eight years.

Table 2. Effects of Allocating Resources to Time-Differentiated Users

	A. No Allocation			**B. Allocation to New Entrants**			**C. Allocating to Long-termers**		
Length of stay	***Month of Shelter Operation*** **13**	**14**	**15**	***Month of Shelter Operation*** **13**	**14**	**15**	***Month of Shelter Operation*** **13**	**14**	**15**
1-30	69	69	69	49	49	49	69	69	69
31-60	57	57	57	57	40	40	57	57	57
61-90	50	50	50	50	50	35	50	50	50
91-120	44	44	44	44	44	44	44	44	44
121-150	42	42	42	42	42	42	42	42	42
151-180	41	41	41	41	41	41	41	41	41
181-210	39	39	39	39	39	39	39	39	39
211-240	36	36	36	36	36	36	36	36	36
241-270	34	34	34	34	34	34	34	34	34
271-300	33	33	33	33	33	33	33	33	33
301-330	32	32	32	32	32	32	32	32	32
331-360	31	31	31	31	31	31	31	31	31
>360*	30	60	90	30	60	90	0	0	0
Population	538	568	598	518	531	546	508	508	508

Source: Human Resources Administration, City of New York

Note: Table assumes 100 families enter each month. Exit rates are from New York City's family shelters. These are composite estimates for families who entered in February 1989 and in June 1988. Rates are probabilities that families would leave on their own.

*Assumes no families leave after 360 days.

Table 3. Percent changes for number of entrants and exit rate

(a) Average monthly percent change

Number of entrants		Exit rate*	
Apr - Oct 1988	-4.0%	Jun - Oct 1990	-1.9%
Apr - Oct 1989	2.7%	Jun - Oct 1991	0.7%
Apr - Oct 1990	17.2%		
Apr - Oct 1991	-2.6%		

(b) Monthly percent change

Month	Entrants	Exit rate	Month	Entrants	Exit rate
Nov 1989	1.1%	n.a.	Nov 1990	0.1%	-1.3%
Dec	25.2%	n.a.	Dec	-7.6%	-1.2%
Jan	0.7%	n.a.	Jan	18.8%	-2.8%
Feb	9.4%	n.a.	Feb	2.0%	-1.1%
Mar 1990	-1.0%	n.a.	Mar 1991	12.1%	-3.3%
Apr	17.7%	n.a.	Apr	4.9%	-0.6%
May	24.4%	n.a.	May	2.0%	-0.9%
Jun	6.4%	-1.6%	Jun	0.0%	0.7%
Jul	20.8%	-1.2%	Jul	4.3%	0.8%
Aug	32.9%	-2.0%	Aug	-10.8%	-0.3%
Sept	7.6%	-1.5%	Sept	-9.7%	0.9%
Oct 1990	10.5%	-3.2%	Oct 1991	-9.1%	1.2%

Source: Human Resources Administration, City of New York

n.a.: not available

* Exit rate is for all families leaving on their own. It does not include those leaving to city provided housing.

Table 4. Shelter population effects of changes in number of entrants and exit rates: April 1990 - March 1991

Effects	Apr	May	Jun	Jul	Aug	Sept	Oct	Nov	Dec	Jan	Feb	Mar
Entrants	-94	-209	-223	-336	-513	-572	-605	-600	-566	-668	-650	-714
Exit rate	57	0	-58	-99	-164	-213	-329	-362	-393	-489	-589	-672
Entrants & exit rate	-40	-207	-277	-424	-658	-708	-849	-844	-807	-997	-1075	-1213
Actual census	3545	3440	3196	3223	3453	3640	3785	3833	3860	4120	4244	4450

Source: Human Resources Administration, City of New York

Note: The number of entrants for each month was held to the same number for that month in the prior year, i.e., April 1989 to March 1990. The exit rate was held to the average monthly rate between June 1989 and March 1990 and is the rate at which families left on their own.

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