

## **Comment on Dennis P. Culhane et al.'s "Public Shelter Admission Rates in Philadelphia and New York City: The Implications of Turnover for Sheltered Population Counts"**

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### **Introduction**

Dennis Culhane and his coauthors have made an important contribution to our understanding of contemporary homelessness. Until now attention has been all too exclusively focused on point-prevalence estimates. Their credible calculations of period-prevalence estimates of the sheltered homeless in Philadelphia and New York City tell us that the cumulative impact of homelessness over time is quite high. Their findings may allow homelessness policies to become more carefully and closely tailored to differences among the homeless in their needs for resources and services.

The emphasis in the preceding paragraph should be placed on the adjective "credible": There have been other period-prevalence estimates derived from cross-sectional studies, such as Burt and Cohen's (1989) and my own (Rossi 1989), but they were built on frail foundations and are, therefore, not fully credible. I never felt comfortable with the annual prevalence estimates I calculated for Chicago because they were based on time of entry into homelessness reports from homeless individuals and buttressed by what I believed to be foolishly heroic assumptions. Longitudinal data of reasonable validity were needed, and such data were absent (or at least not known to many homeless researchers). Culhane et al.'s exemplary use of administrative data collected by the large public shelter systems of New York City and Philadelphia goes a long way toward filling in one of the important gaps in our knowledge of the impact over time of homelessness in two major cities of the United States.

It was always clear to scholars with elementary statistical knowledge that period-prevalence homelessness rates, when credibly calculated, would be much larger than point-prevalence rates. That conclusion was inevitable given the highly skewed

distribution of lengths of time homeless found in cross-sectional surveys, as indicated by the usual large differences between mean and median.<sup>1</sup> The problem was the size of that difference. Culhane et al.'s major contribution is to provide credible estimates of period prevalence for a significant component of the homeless population and to show how much larger such estimates were than those of point prevalence.

All that said, there remain serious technical, substantive, and policy issues in Culhane et al.'s approach, as elaborated in the remainder of this comment.

### **The different meanings of point- and period-prevalence measures**

There are no intrinsic contradictions between point- and period-prevalence measures. They are simply measures of different sorts with different meanings and uses. Neither form of measurement can be regarded as the only right way to measure homelessness. To suggest, as Culhane and his coauthors almost do, that period-prevalence measures are somehow superior is misleading.

A point-prevalence measure concerns the size and composition of some phenomenon at a point in time. Accordingly, a point-prevalence measure of homelessness for (typically) "an average day" provides an estimate of the number of homeless persons on that composite day. It is a useful estimate for some purposes. For example, from the perspective of someone planning a shelter system or food kitchens for the homeless, point-prevalence estimates are estimates of the potential demand for shelter and food.

In contrast, a period-prevalence measure of homelessness provides estimates of the number of persons who are ever homeless during the period in question. For example, an annual prevalence measure counts the number of persons ever homeless during a year. It might be regarded as a measure of cumulative impact. Policy makers concerned with the extent to which a given condition affects the total population of an area would find period-prevalence rates of considerable interest, as would those concerned with developing preventive programs.

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<sup>1</sup>In the Chicago study (Rossi 1989), we found the mean number of months homeless to be 21.9 and the median to be 7.6.

Much more information is provided when both measures are available. The point-prevalence measure and the period-prevalence measure converge when the condition being counted is of uniform time duration. If everyone who became homeless over a year remained homeless for at least a year, there would be little difference between the two rates. The rates diverge when the lengths of time homeless vary among the homeless. Accordingly, diverging point- and period-prevalence rates indicate duration differentiation among the homeless, suggesting that relevant policy ought to take such differences into account. More direct evidence of duration differences is provided by the turnover measures also presented by Culhane et al.

The implications for policy of such duration differences are not obvious, as is shown later in this comment.

### **Political numbers and scientific numbers**

It is hard to understand why anyone took seriously the early 1980s estimates of homelessness issued by Mitch Snyder and the Community for Creative Non-Violence. These were political numbers cobbled together out of wishes and guesses, bolstered by a few haphazard telephone calls to local informants in some small selection of cities, and designed to make a political point by astounding and amazing our political decision makers and the mass media with the message that “millions” were homeless.

It is not clear whether Snyder’s estimates were of point or period prevalences—or indeed whether Snyder knew the difference between the two kinds of rates.<sup>2</sup> What was clear at the time, however, was that his estimates had no scientific standing. There was no reason to take those estimates seriously because they had not been calculated by methods that had any standing in the social science community. That they were taken seriously indicates the eagerness of homelessness advocacy groups to find some support for their belief that the problem of homelessness was very large.

It is even harder to understand why anyone took Snyder’s estimates then or the guesses of other advocates today seriously, long after all social scientists have rejected the numbers as examples of zealotry. It is very disappointing to see Culhane

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<sup>2</sup>I suspect that Snyder meant to provide a point-prevalence estimate because his attacks on the U.S. Department of Housing and Urban Development (1984) point-prevalence estimates made no mention of the difference between point and period measures.

et al. refer to such political numbers as if they had any standing. Their conclusion that “advocates for the homeless appear to have been correct in insisting that homelessness affects a much larger pool of persons than has been documented by cross-sectional research” clearly confuses the differences between political and scientific numbers and between point and period prevalences. That Snyder estimated several million homeless and that period-prevalence numbers are of the same magnitude as his are no validation of his point-prevalence estimates.

The distinction between political and scientific numbers lies both in the methods by which they are generated and the uses to which they are put. Scientific numbers rest on a foundation of procedures that are accepted widely by the scientific community as yielding valid estimates. Political numbers are ones that are advanced publicly as supporting some set of policy measures, regardless of the means by which they were generated. Clearly, scientific numbers can become political numbers when they are used for political purposes. But political numbers are not automatically scientific, whatever credibility may be given to them in the political arena or by the mass media. The advocates’ estimates were never credible because they were not based on credible estimation procedures. That their point-prevalence estimates made in the early 1980s are of the same magnitude as the period-prevalence estimates developed by Culhane et al. may be only an interesting coincidence.

## **Validity issues**

All research efforts are flawed. The difference between good and poor research largely hinges on whether the flaws are fatal to the major focus of the research. In my view, there are no fatal flaws in the research of Culhane et al., but there are several limitations the reader should bear in mind. In particular, the estimates are of period prevalence for the homeless who use the public shelter systems of New York City and Philadelphia, a coverage that is limited.

First of all, as Culhane and colleagues correctly state, their estimates cover only that portion of the homeless of New York City and Philadelphia who have used the public shelter systems in the years covered by their period-prevalence measures. Two additional components of the total homeless population are not covered: those using only the 15 to 18 percent of beds for the homeless located in private shelters and those who do not use either public or private shelters. On any given night these

omitted components and those using the public shelters are mutually exclusive (or virtually so). Over some period, however, the three components overlap to some unknown extent: Public shelter users may also on occasion use private shelters or sleep in nonshelter public places, and those in private shelters or sleeping on the streets at one time may at other times be counted among one or more of the other components. Whether Culhane et al.'s period-prevalence estimates cover all persons in the two cities who ever became homeless in a period depends on how much mixing among components occurs over time. If every person who becomes homeless enters the public shelter system at least once during a given period, then the period-prevalence estimates based on public shelters will be unbiased. To the extent that there are persons who become homeless in that period but never enter the public shelter system, these estimates are biased downward; that is, they underestimate the homeless count. Given our current knowledge, there is no way to estimate how much bias exists, if any.<sup>3</sup>

Second, Culhane et al.'s turnover calculations measure turnover in the public shelter population, not entries into and exits from the homeless state. Some of the entries into public shelters undoubtedly are from the private shelters or from the unsheltered homeless condition. Similarly, exits from the public shelter system can be entries into one of the other components of the homeless population. To the extent that such exchanges among the three components occur, turnover rates for the public shelter population will be higher than for the overall homeless population. There is abundant evidence (Dennis et al. 1993; Rossi 1989) that such crossovers occur.

Third, it appears likely that the compositions of the three homeless population components differ. In both the Chicago (Rossi 1989) and Washington, D.C. (Dennis et al. 1993) studies, the street homeless were more likely to be older, suffered more often from substance abuse and mental illness, and were likely to have been homeless for longer periods. It may be that the street homeless are persons more likely to be excluded from the shelter system.<sup>4</sup> I have no knowledge of the characteristics of New York

<sup>3</sup> Some fragmentary knowledge does exist. For example, the Washington, D.C., Metropolitan Area Study (Dennis et al. 1993) estimated that 14 to 17 percent of the homeless in that area had never used shelters up to the time they were interviewed.

<sup>4</sup> This point has more weight in discussing shelter systems that have the ability to deny entry to the substance abusers and mentally ill and hence is not applicable to New York City, whose public shelter system is required to admit all who apply.

City's and Philadelphia's private shelters and their admission criteria, but it seems likely to me that they occupy specialized niches and hence have clients with special characteristics. In any event, descriptions of the composition of the homeless population based on the component using public shelters may also be biased to the extent that the other components differ.

Fourth, every administrative record system makes errors in entering data. In addition, there may be errors in the data because some of those seeking to enter the system may give wrong information, for whatever reason. I believe that Culhane and colleagues have done their best to eliminate errors that are detectable, especially those that can be found as near matches across records, but I am sure that there are additional errors that cannot be easily detected by machine methods, as the authors undoubtedly would admit. The point is that there remain errors in the data consisting of individuals whose separate entries into the system are counted incorrectly as the entries of different persons and that such errors bias estimates upward by overcounting. Such errors might be so rare that they can be safely ignored, but then again they might be quite numerous.

Fifth, there is the issue of generalizability. Philadelphia and New York City are unique in being the only major cities with extensive public shelter systems. As Culhane has argued in another article (Culhane 1992), shelter administrative policies, such as rules concerning length of stay, can affect shelter caseloads and the mix of shelter clients. Shelter system capacity and occupancy rates vary from city to city (Burt 1992). The point is that, at this time, we cannot assert that the processes shown for these two cities are mirrored more or less faithfully in other places. My own best guess is that with minor variations from place to place we will find that period-prevalence rates are much greater than point-prevalence rates and that the homeless with disabilities will be found quite universally to be the long-term homeless.

The import of the five validity issues discussed above is that there is a band of uncertainty of unknown size around the estimates of Culhane et al. Furthermore, the band of uncertainty becomes larger if the estimates are viewed as applying to other cities. On the one hand, the coverage restriction suggests that the estimates are biased downward by an amount that depends on the extent to which the uncovered components of the homeless population cross over into the shelter population. On the other hand, crossovers among homeless population components and data errors may exaggerate the size of period-prevalence

estimates and of turnover measures. In addition, the possibly unique qualities of the two cities studied add uncertainties to the estimates.

The limitations enumerated above do not seriously affect the main points made in the article. If the band of uncertainty were to be reduced, it is highly likely that the corrections would not affect the magnitude of the rates presented by the authors.

### Calculating rates

The classical definition of a prevalence rate is the number of persons ever in some condition over a given period divided by the number of persons exposed to the risk of being in that condition. Accordingly, prevalence rates are affected by both the numerator and the denominator used in the calculations. Although it is not clearly stated in their article, Culhane et al. apparently used the total 1990 census populations of Philadelphia and New York City as their denominators,<sup>5</sup> thereby implicitly defining those populations as being at risk.

There are several issues concerning the choice of the total population of the two cities as the denominator: First, there is the question of whether the homeless in the public shelter system are from New York City or from some larger area—say, the New York City metropolitan area. This issue does not arise for Philadelphia because admission to that city's shelter system is contingent on showing evidence of previous residence in that city. To the extent that the catchment area of the New York City shelter system is larger than the city proper, prevalence rates may be somewhat overstated.

Second, there is the issue of whether individuals or some other population unit, such as households or families, is at risk. In the case of single persons, who ordinarily occupy their own dwelling units, using the individual person as the unit is quite appropriate, but for others, particularly families, who jointly occupy dwelling units, it may be more appropriate to consider families as the population units at risk. Because a child has no risk of becoming homeless that is separate from the risk of his or her family unit, prevalence rates can vary considerably. Another way of stating this issue is to consider whether a family consisting of

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<sup>5</sup> When calculating prevalence rates for a subgroup (e.g., African Americans), the denominator is restricted to the total city population of that group.

a mother and three children who move into a shelter should be considered a single instance of homelessness or four instances.

It is not clear what the implication may be of using one base or another in calculating prevalence rates. I believe that a more appropriate unit would be households, counting single unattached persons as separate households. Using households as the unit of analysis would decrease both the numerator and denominator of prevalence rates, and therefore it is unclear whether rates would be higher or lower as a result.

### **Policy implications**

Several features of Culhane et al.'s findings appear to have important implications for homelessness policy. First, the period-prevalence rates show that homelessness, when measured over significant periods, affects more people than the "average day" counts suggest. Second, the findings indicate that there may be two subgroups within the homeless population: persons who experience short-term episodes and persons who are homeless for long periods. Third, as indicated in cross-sectional studies, the long-term homeless include significantly more of the homeless who are afflicted by substance abuse and mental illness. I will take up each of these points.

If there is some flagging of interest and decline in sympathy for the homeless, as some claim, these new numbers might support a reversal in those trends. These numbers suggest that the impact of homelessness over the past few years has been larger than commonly believed. In addition, the short-term homeless may resemble more closely those who generate the most public sympathy. Public sympathy is generally higher for those who precipitously become unemployed and thus homeless after being "just a paycheck away from homelessness" for some time.

In the recently released report, *Priority: Home! The Federal Plan to Break the Cycle of Homelessness* (U.S. Department of Housing and Urban Development 1994), the new period-prevalence numbers generated by Culhane and coauthors<sup>6</sup> provide key support for the administration's arguments for increased appropriations for homeless programs. It is also rumored that President Clinton has found the new numbers persuasive and supports augmenting funds for such programs. In short, it appears that the period-

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<sup>6</sup> Bruce Link's sample-survey-based period-prevalence estimates (Link et al. 1993) are also cited. (See next section for a discussion of Link's approach.)

prevalence estimates have already entered the policy arena and are being taken seriously by policy makers.

As a long-time advocate of applied social research, I am gratified to observe scientific numbers becoming political numbers—without, I hope, losing too much of their science cachet. In that last connection, I would like to see some muting of the message implicit in Culhane et al.'s article that the period-prevalence numbers are somehow better than the point-prevalence ones. Indeed, if the homeless policies advocated by this administration are designed to bolster the homeless shelter system, then the point-prevalence estimates might be better for planning purposes.

A suggestion made by Culhane et al. is that the existence of a very large number of persons who are homeless for only short periods supports the development of programs aimed at preventing short-term stays in the shelter system. There is much merit to such a suggestion, providing that the programs can be targeted with some precision at those who are at high risk of short-term homelessness.

Targeting programs is a critical aspect of program efficiency. A precisely targeted program is one that reaches all persons who should receive it and no one else. For example, Social Security old age benefits are fairly well targeted, reaching almost all eligible persons who are above certain age thresholds and few, if any, persons below those age thresholds. In contrast, the *Sesame Street* television program, designed to stimulate the cognitive abilities of disadvantaged children, also reaches many advantaged children and thus is poorly targeted. Of course, the targeting imprecision of *Sesame Street* is not of much concern because per capita costs of the program are small and achieving more precision likely would be very expensive and might be technically impossible.

Unfortunately, it is not at all clear whether adequate targeting can be accomplished for programs directed at those who are at high risk of homelessness. Because we do not have enough scientific knowledge about this group, it is difficult to devise a targeting procedure. For example, we know (by definition) that those who become homeless do so because they have lost access to a conventional dwelling unit: We know that they were among the housed. Accordingly, one preventive strategy would be directed at some subgroup in the housed population. We can also surmise that they are poor; otherwise, most would simply find another

dwelling unit after losing access to a current one. But the population of poor persons is very large, numbering in the millions.<sup>7</sup> A program directed at the poor, of whom only a very small proportion ever become homeless over the period of a year, cannot be efficiently targeted.

If a preventive program is to be directed toward those who are likely to lose access to a conventional dwelling, the problem is how to identify those with a high probability of losing their homes. As far as I know, there is no easy way to target those highly vulnerable to homelessness. For example, using the housing courts to intervene before evictions might seem sensible, except that few of those who become homeless do so through formal eviction. Most either leave a leasehold voluntarily or leave an informal shared housing arrangement with kin or friends.

In short, before a preventive program can be designed it is necessary to think through what should constitute the target population and how to reach that population efficiently. Preventive programs are likely to be expensive; hence precise targeting is critical to keep costs within acceptable limits.

In addition, there is the critical question of what the preventive intervention should be. Such an intervention might be directed at stabilizing the existing housing arrangements of those at risk of short-term homelessness, perhaps through a rent subsidy program or through the direct provision of housing, as in the subsidization of single-room-occupancy residences (SROs). Another kind of intervention might be directed at income support for the very poor in the expectation that solving their income deficits would also solve their housing problems. Alternatively, the program might be aimed at providing housing search support.

An alternative to a preventive program would rely on the shelter system to provide the targeting and would be directed toward moving the newly homeless back into the conventional housing market as quickly as possible. This strategy is based on the idea that targeting people before they become homeless is so close to impossible that we would be better off waiting until the short-term homeless come into the system and then helping them to leave. The targeting cannot be precise because some of the persons entering the system are destined to become chronic

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<sup>7</sup> For example, in 1989 there were about 14 million unattached single persons and single parents whose income was at or below the poverty level, including about 6.4 million whose income was 50 percent or less of the poverty level.

homeless and it may be difficult to identify the short-term homeless at entry. Of course, it may turn out that the best distinguishing characteristic between long-term and short-term homelessness is the ability of the homeless person to benefit from programs aimed at accelerating the exit from homelessness. Furthermore, not all persons who become homeless enter the shelter system: Those who sleep only in public places will be missed.

An “accelerated leaving program” might provide persons entering the shelter system with a long-term low-interest loan (or even a gift) covering the first two months’ rent and security deposit. Provision of this assistance would be contingent on the homeless person finding a place to rent within four weeks. Another version of this program might rely on longer term housing subsidies in the form of vouchers.

A major drawback to programs targeted to persons first entering a shelter system is that such programs may entice persons to enter the system to receive benefits otherwise unavailable. Culhane et al. cite anecdotal evidence that some families in New York City “become” homeless in order to receive housing vouchers more easily.

Another set of policy implications centers on what policies ought to be formed concerning the long-term homeless. If preventive programs or accelerated leaving programs are successful, the shelter systems of our local community increasingly will become shelters for the long-term homeless. Accordingly, the shelters will also become increasingly filled with those with the serious disabilities of mental illness, alcoholism, and severe drug abuse, to the extent that shelters will admit such persons.<sup>8</sup> Shelters are not ordinarily equipped to provide substance abuse treatment or therapy for the mentally ill. Moving such persons into appropriate treatment facilities presents a severe problem for policy formation. Such policies might require changes in legislation concerning an individual’s right to refuse treatment. At the least, the capacities of substance abuse and mental illness facilities may have to be expanded.

Culhane et al.’s findings demonstrate the need to consider length of time homeless in the policy development process. However,

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<sup>8</sup> With the major exception of the public shelter system of New York City, most shelters attempt to exclude the obviously alcoholic, drug abusers, and mentally ill. In practice these policies mostly mean that persons who seek admission are excluded if at the time they appear to be under the influence of alcohol or drugs or are acting bizarrely.

major issues related to program design and targeting remain unresolved.

### **Implications for further research**

Both Philadelphia and New York City were forced into running municipal shelters by consent decrees arising out of class action suits that affirmed the right of the homeless to shelter. It is doubtful that any additional local political jurisdictions will attempt to set up and run extensive municipal shelter systems. Accordingly, it seems highly unlikely that computerized shelter databases will exist in numerous other places because shelters in other localities are not likely to develop centralized management information systems. The prospects for a national data system based on data derived from shelter admissions are therefore dim. If a national data system is desired, we should probably look elsewhere.

Another approach, mentioned briefly in the article, is worth considering as an alternative to obtain national measures of period prevalence of homelessness. Using sample surveys of households, the approach simply asks probability samples of persons about past experiences with homelessness. Asking about episodes of homelessness last year provides the base numbers for an annual prevalence rate, and asking about past homeless episodes over longer periods provides data for prevalence rates for longer periods.

Bruce Link has used this approach to calculate national lifetime and five-year period-prevalence rates (Link et al. 1993). Using "random digit dialing" methods, some 1,500 households were contacted by phone. Link's approach had the advantage of covering all places in the United States, including rural areas, and covering all components of homelessness, including those using any kind of homeless shelter and the street homeless. It had the disadvantage of not covering persons living in households without telephones.<sup>9</sup> There also may be problems with the accuracy of recall<sup>10</sup> and with how respondents interpreted homeless

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<sup>9</sup> In particular, this means that those who were homeless at the time of the interview were not covered, nor were those who had been homeless in the period under study but had either died or emigrated before the interview period.

<sup>10</sup> Survey researchers have noted that respondents, when asked to recall past events, tend to "telescope" time, reporting events as having occurred more recently than they did.

episodes. But the major advantage of the household survey approach is that it does not rely on the existence of a well-run administrative record system and hence can provide period-prevalence estimates for the country as a whole and for all localities and regions. Of course, the sample survey approach can be used on the local level as well, providing period-prevalence rates for, say, Chicago or Los Angeles.

I believe that it is worthwhile investing in the refinement of the sample survey approach. There are several obvious ways future researchers may improve on Link's approach. First, area probability sample designs, along with the use of face-to-face interviewing, can cover all households, including those without telephones and those living in SROs and other group quarters. Second, methods development research can be used to improve instrumentation so as to provide acceptably accurate retrospective recall data. In addition, such development work can standardize the meaning of homelessness to ensure that respondents all report their bouts of homelessness in the same way.

I believe that the way to obtain national period-prevalence estimates is to develop technically improved retrospective recall measures based on national sample surveys. Indeed, a few questions added to ongoing large-scale periodic surveys, such as the Current Population Survey, would provide detailed population breakdowns and eventually a time series in which changes in the size and composition of the homeless population could be monitored.

## **Conclusion**

Culhane and colleagues clearly have made an important and dramatic contribution to our further understanding of the nature of contemporary homelessness. At any point in time homelessness is a rare condition, but over time the cumulative homeless experience affects significant proportions of the population, as much as 3 percent over a five-year period for New York City residents. These estimates have been dramatic enough that they have already influenced the policy-making process. However, as an approach to obtaining period-prevalence estimates for the country as a whole, a strategy based on public shelter databases has poor prospects. A more promising approach is to use sample-survey-based retrospective reports on past homeless experiences, which are more easily implemented and generalized to the nation as a whole.

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