

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

Culhane and coauthors make a case that “turnover among the homeless population is significant and that many more people are likely to be homeless over time than at a single point in time.” They demonstrate this point by analyzing shelter entry and reentry data from the Philadelphia and New York City public shelter systems. No one, of course, has ever suggested that annual rates and multiyear rates are *not* greater than point-prevalence estimates. The contribution of the article is that, at least for the population in public shelters in these two cities, the reader is presented with data that illustrate just how much greater. The authors conclude that “the annual rates are three times greater than rates documented by point-prevalence studies.” This is not surprising and is consistent with prior estimated turnover rates in various locales and for the country as a whole. Indeed, the article mentions several prior turnover estimates, which range from 2.3 to 5.8.

Culhane et al. also review a number of recent telephone surveys that indicate that a higher proportion of Americans have experienced homelessness over time than many would deem consistent with the 0.1 to 0.4 percent point-in-time estimates from past enumeration studies. They cite, among others, an article by Link et al. (1993) that reported that 3.2 percent of the U.S. population, which would be about 7 million persons, may have experienced homelessness over a five-year period in the late 1980s. Culhane et al.’s “research question” is whether the high estimates can be reconciled with the low estimates, and they seem to conclude that the multiyear estimates vindicate the claims of homeless advocates that the problem is much bigger than the federal government has until recently been willing to acknowledge.

In spite of the careful couching of their conclusions—acknowledging, for example, that New York City and Philadelphia data cannot be generalized to other cities and not taking issue with the convergence of estimates in point-prevalence studies—the authors nonetheless attribute more policy significance to their findings than is warranted, and sometimes their conclusions overreach the data presented. They strongly imply that the problem of homelessness is much greater than previously assumed by scholars and policy makers because it affects a larger pool of persons than the number at a point in time. From the high turnover they draw conclusions about the characteristics of homeless persons (e.g., that homeless persons who turn over at a higher rate have a greater likelihood of being employed or recently unemployed) without presenting any data on such characteristics. They also infer “structural” economic causes for homelessness simply from the turnover rate, without an adequate discussion of how government shelter policies affect both first-time entries and turnover.

Defining the scope of homelessness

Turnover rates can provide useful information for policy makers but shed little light on the so-called numbers debate because annual and multiyear rates do not define the scope of a social problem as conventionally understood by the public. When scholars argue that the point-prevalence estimate does not define the relevant scope of a problem, the public perceives only that there is an argument about numbers, which in public discussion are generally understood as point-in-time numbers. The media and public appear to have little understanding of what additive longitudinal data mean and little tolerance for such complexity.

What else can be expected? The number of poor people announced every year is a point-in-time estimate, the number of unemployed is at a point in time, the number of persons on welfare is at a point in time, and so on. These socioeconomic indicators are presented to the public with point-in-time data even though there is turnover in the population for each. The dynamics of entries into and exits from these populations are important to understand for designing proper policies for subgroups with different characteristics, but using such dynamics to establish *rates* of a problem is highly misleading because the rates depend on the (arbitrary) period chosen and can be made as high as one pleases. (It is ironic that Culhane et al. assume a static poverty population to establish a “rate” for the number of poor using shelters over a three-year period.)

As an illustration, assume that because of turnover the proportion of unemployed over three years is 18 percent of the labor force and not the 7 percent or so of a point-in-time estimate. Suppose a group of scholars defined the three-year 18 percent rate as the relevant or “real” scope of the problem because 18 percent and not 7 percent of the labor force was “affected” by unemployment and was “at risk.” What would policy makers and the public do with such an assertion? Probably nothing. It is easy to figure out that being *at risk* of unemployment, or *having been* unemployed, is not the same as *being* unemployed. Otherwise, one would have to conclude that point-prevalence estimates were underestimates of any social problem by definition.

The same argument applies to the use of period-prevalence estimates to measure the scope of homelessness. Such an approach overestimates the scope of the problem. Why should someone who was homeless for a day in January or a week in March, and not again that year, be included as part of the homeless problem in December? Yet this is what an annual count does. That formerly homeless persons may still be at risk of homelessness is another issue, but it is misleading to include in the “scope of homelessness” those who were once homeless but no longer are.

Similarly, the arguments over characteristics of a subject population can rage on not because people disagree on the facts, but because they are looking at different things. Take the welfare population. Some choose to look at welfare recipients over time (a higher proportion will be short-term recipients who are not as dependent on public assistance) rather than at a point in time (a higher proportion will be long-term recipients who are more dependent). Both perspectives have policy relevance, but at least nobody is arguing about how many welfare recipients there are, because the point-prevalence estimate defines the scope of the problem.

So why are the homelessness numbers so different? The reason for the muddle lies in the contentious history of the numbers debate (Kondratas 1991b). For a long time there was no agreement on a reasonable point-prevalence range, nor are such data collected and reported regularly. In recent years, however, a consensus has developed among scholars and policy makers about a reasonable point-prevalence range, and it corresponds to hundreds of thousands of homeless. Since this consensus has not adequately filtered down to the general public, any suggestion that point-prevalence estimates are not the relevant way to look at the problem, or are misleading, is bound to lead to confusion.

Scholars, of course, are not necessarily to blame for misuse and misinterpretation of their study results, but since Culhane et al. undertook their research for its policy relevance in defining the scope of the problem, they cannot ignore the potential real-life impact of their work. Like the Link et al. paper (1993), this article practically begs to be misinterpreted. For example, how would the average person, whose eyes would glaze over at the tables and charts, interpret the following conclusion?

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 demonstrated in this study is the magnitude of turnover in the sheltered population. It is this high rate of turnover that accounts for much higher rates of homelessness over time than at a single point in time.

This conclusion is less a research finding than a tautology, since turnover accounts for much higher rates of everything over time.

Moreover, saying that advocates for the homeless were right is wrong, because the article misstates what the argument was about in the 1980s. Policy makers were not denying that there was a much larger pool affected by homelessness than were actually homeless. Advocates, however, *were* insisting that the number of homeless at a given point in time was grossly underestimated, and they were wrong. The fudging as to what they “really meant” came later and is apparently continuing. For example, estimates of 2 to 3 million homeless persons nationally, which were portrayed as point-in-time estimates when they were put forward in the early 1980s, have more recently been portrayed as annual estimates.

This is not just an academic point. A recent case of the misuse of social research is illustrative. The figure of 7 million homeless over five years previously referenced recently found its way into the Clinton administration’s plan to end homelessness (U.S. Department of Housing and Urban Development 1994). A draft was leaked to the *New York Times*, which ran a story about how the Clinton administration “says the problem is ‘far larger than commonly thought,’” and “endorses recent estimates that as many as seven million Americans were homeless at some point in the 1980s” (DeParle 1994). So far, so good, but the number was contrasted with the upper limit of the point-in-time estimate used by the Bush administration (600,000). This article resulted in a spate of derivative stories across the country.

I was asked to comment on the *New York Times* story by several print and television journalists, none of whom had actually seen the administration's draft report, let alone the paper from which it obtained its figure of 7 million. Nonetheless, most of them duly went on to "report" that the administration was claiming 7 million homeless, and that Republican administrations had never acknowledged the scope of the problem. In vain did I, having seen the draft, try to persuade them that the administration made no such claim.¹ My contribution on one TV segment was reduced to an incomprehensible, dull sound bite about multiyear figures not being the same thing as point-in-time counts, which sounded like bureaucratic evasion, while advocates on the same segment professed themselves to be thrilled that the federal government finally recognized the true scope of the problem. The misinformation spread across the country. An editorial in the *Nashville Tennessean* (1994), for example, summed it up as might have been predicted: "The government report says as many as 7 million people *are* homeless in this country. Previous estimates had stood at only 600,000" (my emphasis).

The media, of course, are far more interested in sensationalizing a story and creating conflict than in adequately researching and communicating reasonable interpretations of the facts. But if scholars use expansive measures of homelessness, knowing full well that no other social indicator is measured or interpreted like that, what can one expect from the press?

Policy implications of turnover

Culhane et al. acknowledge that cross-sectional estimates are useful for "immediate planning needs (i.e., planning shelter capacity)" but inadequate for explaining the "magnitude of the problem over time." Actually, the only way to evaluate the magnitude of the problem over time is not to determine annual and multiyear rates but to do point-in-time estimates or counts by consistent methods at regular intervals to see whether the problem is increasing or decreasing and whether and how the population is changing.

The future research avenues proposed by Culhane et al.—looking at shelter utilization data over time to determine the characteristics of the homeless—would simply increase the ratio of

¹The administration, however, *was* highly irresponsible in the draft report in using such a number without providing any context whatever for understanding it, and in not evaluating critically the survey questionnaire on which it was based.

short-term homeless to long-term homeless in the period under study and lead to debates about who the homeless really are like those that currently take place about who typical welfare recipients are. Such research would, in short, provide the basis for a different *profile* of the homeless population, not a different *magnitude* as the ordinary person understands the term.

Culhane et al. believe that analysis of shelter utilization data over time would decrease the proportion of homeless people with severe personal problems. This suggests that for some of the homeless (even most, if one looks at a long enough period), homelessness may be episodic and more related to a crisis or “structural” economic factors than to chronic personal problems, mental illness, or substance abuse. This idea is plausible, and the authors cite some supporting evidence from other studies, but their own findings neither confirm nor contradict such a view, since they present no data on the relevant characteristics of the shelter users they tracked.

This is not the only one of Culhane et al.’s conclusions that is based on the literature they reviewed, or simply on a hunch, rather than on the data they present. They also state, for example, that “employed and recently unemployed people...may turn over at a higher rate, meaning that their proportionate representation has been significantly underestimated.” Note the transition from “may” to “has been”; it is not atypical of the authors’ tendency to overstate their case.

What is disappointing about the way Culhane et al. present their data is that much space is devoted to description but little to analysis. There are interesting bits of information throughout that the reader may wish the authors had evaluated. For example, table 3 in the article seems to indicate that singles are a higher proportion of the three-year population in Philadelphia and New York City than of the point-in-time population, implying more first-time entries of singles than of families over the three years. As the authors explain, this finding suggests a higher turnover among single adults than families. This difference is intriguing because it indicates a larger potential pool of homeless persons (those at risk) among singles than families. If one relies on cross-sectional analyses of the characteristics of the homeless, such a finding is counterintuitive, because such studies indicate that the singles are the more troubled population and therefore more likely than families to be long-term homeless. On the basis of this cross-sectional research, one would expect more turnover among families. Had the authors collected data on the relevant characteristics of the shelter users they

surveyed, they might have come up with some evidence for their thesis of underestimates of episodic, structural homelessness among singles.

The scope of their findings, however, is limited only to what they call “magnitude,” not characteristics. They do find overrepresentation of minorities in the homeless population, but this is already known from cross-sectional analysis. New York City has an atypical (in fact, unique) percentage of families in public shelters compared with other cities, but because the number is so large, some description beyond racial identification might have been helpful in analyzing turnover data. How many families have two parents? How many are on welfare? Is there any difference in the rapidity of turnover based on any of these characteristics?

There are other problems beyond the researchers’ control in comparing the two cities and drawing nationally applicable conclusions. The two cities measure readmissions differently, for one thing, and data are available for overlapping but different time frames. It is not only research design but also the limitations of the data, in other words, that leave many unanswered questions. But it is also the unanswered questions that limit the relevance of turnover data for policy makers.

Policy directions

In spite of the acknowledged limitations of their data, Culhane et al. move from that basis to critiquing recent local and national homeless policies and recommending changes in focus. They raise many legitimate points, but most seem based more on their collective experience than on the actual findings of their study. Further, the “recent reform proposals” they question—transitional housing and continuum of care—are hardly new approaches. From 1989, the Bush administration emphasized the need to move from an emergency shelter mode to expanding transitional and permanent supportive housing and better local and intergovernmental coordination (shelter plus care, from outreach to permanent housing); the history of its budget requests and legislative proposals bears this out (Kondratas 1991a). By 1991–92, this approach was formalized in a Federal Plan to Help End Homelessness, which also stressed prevention strategies and expanded access to mainstream housing and welfare programs for the homeless (Dolbeare 1991).

The old federal plan, of course, has been consigned to bureaucratic oblivion, while the new one is not yet operational. But the Clinton administration's budget proposals and rhetoric do not seem to be offering policy prescriptions or breakthroughs that are inconsistent with the Bush administration's plan and budget requests in the essentials. The Clinton administration's continuum-of-care approach is a new label on the old shelter-plus-care philosophy. (Whether the sketchy plans of action proposed in budget documents and the federal plan are adequate and consistent with the current administration's stated goals is an open question.)

Culhane et al. feel that their data show that most homeless persons are not chronically homeless (although that is a debatable conclusion), and so they do not think the homeless should be forced or enticed into a continuum-of-care system. Such a view is based on a misunderstanding of what a continuum-of-care approach is all about. In some ways, "continuum of care" is simply a fancy term for coordination of services. It starts with analysis and diagnosis of a person's needs. It does not force everyone along each step of the same continuum. No one is forced or enticed to accept services they do not need; the real danger is that coordination breaks down or services are not available.

Nonetheless, Culhane et al. correctly see the dangers of developing a separate system of services for the homeless and expanding it, rather than changing misguided policies and concentrating on reforming and repairing the safety net of mainstream programs. But that die was cast when the McKinney Act was passed in 1987.² The authors are correct to fear increased admissions and longer stays in shelters because of the lure of being offered priority for public housing and Section 8, because it has already happened (Ellickson 1990). Expanding services in a homeless minisystem is also likely to result in more dumping of clients on the shelter system by other agencies in the mainstream system. That, too, is happening. (Expanding the definition of homelessness would increase this effect.)

Culhane et al. also have a point when they say that prevention of homelessness, by shoring up the institutions of family and community and fixing the holes in mainstream programs and policies that allow people to drop into homelessness in the first place, would be superior to creating an ever-expanding (even if improving) parallel system of services exclusively for the homeless. But this policy would be the best regardless of the scope of

²I had suggested in 1985 that a separate homeless system was not the way to go (Kondratas 1985).

the problem and the turnover rate. Prevention is always better than cure.

Conclusions

Culhane et al. are correct in assuming policy makers would benefit from better data on the dynamics of homelessness, since documenting entries into and exits from homelessness over time would provide insight into the causes of homelessness and what works in ending it for what kinds of persons. Their policy discussion reflects the frustration that providers of homeless services have long felt in being part of a parallel housing, health, and welfare system for the homeless (with all the attendant costs and inefficiencies this implies) when the mainstream housing, health, and welfare systems, with far greater resources, should be serving homeless persons as well.

They are on shakier ground, however, in claiming that shelter turnover data suggest that the problem of homelessness has been underestimated. By implying that an annual (or biennial or triennial) count is a better measure of the scope of homelessness than a point-in-time estimate, the authors in effect expand the definition of homelessness to include those who may have been homeless but no longer are. Such an approach overestimates the problem and is misleading. If this approach causes the average person to assume greater numbers of homeless persons than there are (emphasis on the present tense), pressure may increase on policy makers to expand the separate homeless service systems the authors decry, rather than integrate at-risk populations (and the homeless) into mainstream systems.

In a footnote, Culhane et al. explain that differences in rates of shelter turnover between New York City and Philadelphia “probably reflect differences in local shelter policies (which in turn influence admission and stay patterns) as much as variations in local conditions that produce homelessness.” This offhand observation seems to undermine their entire thesis. If this is the case, it is hard to see why they conclude that the results of their study indicate that the “scope of the homelessness problem,” however defined, should be reconsidered at all. Perhaps what should be studied first is how shelter policies contribute to, alleviate, or reduce homelessness.

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