Translating Research into Homelessness Policy and Practice: One Perspective from the United States

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Like social scientists everywhere, homelessness researchers in the US are usually ignored. Good science that identifies what causes homelessness, sound evaluations which document that certain programs will never work, and even evidence that promising solutions deserve broad replication, are often disregarded. Such wanton indifference for science would constitute malpractice in the field of medicine, but it sometimes passes as acceptable policy in the field of social welfare. Ideology, politics and preservation of the status quo usually prevail. So, what's a well intentioned researcher to do? Persevere and become more tactical. After all, policy failures can't be ignored forever. Like good advocates, researchers too have to be opportunistic. We have to find the right kind of audiences for our work, and perhaps even more importantly, we have to do the kind of work that will get us the right audiences.

Although I know little to nothing about the Australian situation, permit me to venture that some of the translational problems that we encounter in the US have some parallels in your country. First, historically, our methods for studying homelessness have probably been the weakest link in the chain to effective policy intervention. Most homelessness research has been very descriptive in nature and has been based on cross-sectional samples or population surveys. The results of this research have been confusing at best, and easily misinterpreted at worst. For example, the literature on mental illness among the homeless has been widely misrepresented by the popular press as applying to all of "the homeless," when in fact only single adults have been included in the most rigorous epidemiologic studies (persons in families represent 45% of our nightly homeless population). Furthermore, reported prevalence rates are usually interpreted as disproportionately high, when no systematic comparisons to housed poor populations from comparable demographic groups are provided. The cross-sectional nature of the samples is even more self-limiting, because the nature of the sample confounds discerning which factors are associated with becoming versus remaining homeless. In other words, even if disproportionately high rates of mental illness are found, such higher rates may not exist among people who become homeless, but only among people who remain homelessness. In either case, the lack of longitudinal data and comparison groups makes the epidemiological research on mental illness essentially uninterpretable with regard to whether mental disability is or is not a risk factor for becoming or remaining homeless, let alone why.

A second limitation with the policy relevance of our traditional research approach has been the primary subjects of our research: people who are homeless. While studying people who are homeless may be convenient, they (their characteristics, beliefs, attitudes, and behaviours) may not be the oracles of policy relevance we might otherwise presume them to be. Indeed, the most obvious reason that much homelessness research is ignored by policy makers is that most homelessness research doesn't directly examine policy issues. For example, for much of the 1980s homelessness advocates in the US rallied around the slogan that the problem was about "Housing! Housing! Housing!" Yet, the homelessness research literature, which produced several hundred articles by 2000, includes only a handful in which housing is actually studied. Certainly, the psychological or medical training of most of the investigators has had something to do with this bias, but there are likely other factors that have contributed to this narrow focus. Social systems — the mental health system, the housing market, the welfare system, the criminal justice system — are inherently complicated, and in many cases are very difficult to measure. For one, to understand how these systems intersect with homelessness requires knowing as much about these systems and their overall dynamics of utilisation as about the people who become homeless because of them. Yet, most homelessness researchers have limited knowledge of these systems. It also requires access to data which these systems may be less than forthcoming in providing. Further, connecting even a single case of homelessness directly to the policies of these systems is difficult, because policy matters are mediated in complex ways through a variety of social institutions and providers, let alone within the biographies of individuals and families, and usually over a period of time and exposure. In essence, our unit of analysis (people who are homeless already) may well be one of the fundamental reasons that our work has so had so little policy impact.

A third potential consideration is less about methods and measurement, and more about us: perhaps homelessness researchers are a reason that homelessness research has had limited policy impact. As researchers we have been disappointed not only with how little attention is paid to our basic science in this area, but even to the more applied program and policy research. Programs that are found to be of little value continue to be funded, and projects that should be replicated on a larger scale remain only at a demonstration level. This frustrating situation persists because researchers are not usually in positions of influence, and are often well outside the sphere of political power that drives policymaking decisions. Even when researchers are located in government agencies, they are often even more constrained institutionally from promoting their research findings as a basis for policy reform than their academic counterparts. Like our research, we are often not considered when local and federal officials set out to make policy changes.

What's a researcher to do? While there's no simple response that will guarantee that the research community will overcome all of these barriers and get the needed attention of policymakers to their research, a few features of the remedy are clear. First, longitudinal study designs with appropriate comparison groups are needed so that research results address the nature and scale of various risk factors for homelessness, and are readily interpretable by a policy audience with regard to the populations they serve. Second, the policy issues and the social systems that are at the root of homelessness (and/or its resolution) have to be studied directly, not just as they are reflected in the characteristics, opinions and self-reported experiences of homeless people. Third, researchers have to become less insular and more collaborative with non-researchers, especially with public program administrators and other policy and program audiences, so that our research and its benefits are viewed as part of a shared process of continuous, incremental improvement in how society addresses

homelessness. And although there may be many means to achieving these aims, I would propose that the creation and analysis of administrative data, particularly homelessness service system data, and their integration with other service systems' data, holds the greatest promise for achieving the collective goal of these aims: to improve the policy relevance of homelessness research.

As a disclaimer, I should note that I am not a disinterested or unbiased observer regarding this approach. Administrative data on shelter use and its integration with other administrative databases has been at the core of my professional career as a researcher in this area for fifteen years. I have also been a public advocate for the implementation and use of administrative data for homelessness research in the United States, going so far as to have tried (and failed) to create and distribute a particular software application in the mid-1990s, and later working with the US Congress to draft legislation in 2000 making such information systems a requirement for cities receiving federal homelessness funds (nearly all of them). That said, forgive me if I make my case once more here in the context of this discussion on the relevance of homelessness research to public policy, and in considering how the Australian situation could likewise benefit from this approach.

First, administrative data that track shelter admissions and discharges are longitudinal by nature. By capturing dates of entry and exit, they provide the documentary film version of homeless system dynamics, as compared to the snapshot portrayal of cross-sectional studies and censuses. The quantity and quality of data they provide is correspondingly that much richer. Researchers can thus readily document not only how many are people are in homeless programs on a given day, but how many move through the system over the course of a year — a basic fact that more compellingly and dramatically illustrates the extent of the phenomena than the single day census approach. Combined with other population data, rates of homelessness by various subgroups can be compared, and the basic population-based risk factors described. Furthermore, how these counts and rates change over time can be easily documented from year to year, measuring how society is faring overall in addressing the problem. Analyses of subpopulation dynamics are also possible, enabling researchers to document who is among the short-term homeless, versus repeated or long-term stayers, and the proportion of resources they consume respectively. This in turn can be used to target people prospectively based on determined risk factors for interventions of various size, scope and expense — all based on knowledge of what the anticipated costs of various persons would be absent such an intervention. This kind of population segmentation is essential if policymakers are to design and implement programs that can reduce the incidence or severity of homelessness in a cost effective manner.

Second, administrative data make possible the interagency dialogue that is so critical to the success of any efforts to reduce homelessness. The homelessness system is a very small actor in the larger stage of social welfare provision. Compared to the mental health, criminal justice, income maintenance, child welfare, or school systems with which the homeless system interfaces, we are but a mouse in room of elephants. Yet, even small changes in these larger systems have major impacts on the homeless system, and, indeed, as we increasingly realise, the homeless clients in turn have significant impacts on these other social welfare systems as well. These critical interdependencies are invisible, or limited to anecdote, without data integration projects that systematically investigate these associations. Merging administrative records on shelter use with prison, foster care and psychiatric discharge records can be used to document not only how many people become homeless (and when) from among these systems' institutional discharges, but we can compare those who become homeless and those who do not and potentially identify prospectively who should be targeted (and where) with alternative interventions. And if these systems are not sufficiently motivated based on their own missions to do so, such data integration projects can also document the cost of homeless clients on these systems when they end up re-hospitalised, rearrested, re-incarcerated and even having children who end up in the child welfare system, as a result, at least in part, of the failure of these systems to effectively provide for the transitions of their former clients to the community. In effect, the homeless system, through periodic data integration efforts, can become the accountability system for the mainstream social welfare institutions whose improved operation is essential to the prevention and eradication of homelessness. Indeed, while these systems may be able to ignore research projects which find samples of homeless people who self-report their prior experiences in their care, they are less able to ignore results based on their own data, which document the actual number and the impacts of homelessness on their own systems, budgets and programs. And while the experiences of individual homeless people form the basis of these analyses, it is in fact the systems that are actually studied, as they are mediated through the institutional experiences of their clients.

Third, integrated administrative database research puts homelessness researchers at the table of the decision-makers and policy audiences we seek to influence. By necessity, research of this type requires partnership. Data exchange agreements and protocols are required to engage in this type of research, and agency directors will need to be involved in understanding the nature and purpose of this research. Researchers won't be able to insulate themselves with samples of their own choosing and control; they will have to commit to be part of a process internal to the agency, and even between public agencies, where there is a deliberate attempt to understand these interagency phenomena and for a explicit policy purpose. Getting access to these agencies, their key decision-makers, database administrators and the like is not easy or guaranteed. Researchers have to form partnerships with these organisations and their staff (and maybe even with the people who can get them to the table in the first place), and develop relationships that will make these kind of projects possible. Doing so requires some political deft. But, the payoff is potentially significant, and usually worth the trouble. Thus, to the extent that researchers seek to influence the policymaking process, they will have to be willing to shape the way they do business and to match their research questions with the policy imperatives of those decision makers.

In conclusion, if researchers studying homelessness seek to have their work be more relevant to public policy, they have to do more policy relevant work. While this may seem self evident and tautological, my intent is distinguish research that may have some policy relevance by virtue of its subject matter alone, with research that is policy relevant because it is based on the very substance — and data — of what policymakers effect and their agencies do. My limited experience in the US is not that homelessness research in general is not relevant to public policy, but that its translational utility is often lost or at least not obvious to those who make public policy. Policymakers are concerned principally with the costs of their programs, and with the mechanisms and levers of influence they control. If our research does not connect to those concerns, or does so only indirectly, our influence on the policymaking process will be limited accordingly. There are no doubt several ways by which homelessness research can be modified and conducted in ways that improve on its policy connections, and I have examined and championed only one here. I have encouraged its adoption here because I believe that by establishing administrative data that track homelessness program utilisation, researchers and administrators are investing in an approach that has promise for institutionalising such policy relevance, and does not leave the issue of relevance to the approaches and interests of individual researchers. As social scientists, we usually share a belief in the value of data, and in the role of social structures in influencing social life. In my opinion, the implementation and use of administrative data on homelessness and for the purposes described above actualises those beliefs. ■

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