
Frustrated Bus Robbers, Liberal Cat-Lovers, and Keynesian Dentists: A Review of Three Books about US Homelessness

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Homelessness Is a Housing Problem: How Structural Factors Explain U.S. Patterns, by Gregg Colburn and Clayton Page Aldern

Homelessness in America: The History and Tragedy of an Intractable Social Problem, by Stephen Eide

San Fransicko: Why Progressives Ruin Cities, by Michael Shellenberger

In the 1970s, buses in Newark started getting robbed frequently. Robbers got on the buses, pulled a knife or a gun on the driver, and demanded that they hand over the cash they had collected from riders. Usually the drivers complied, and then the robbers hopped off the bus and vanished into the surrounding neighbourhood. This was a big problem.

Fortunately, the transit agency solved this problem after a few months. They stopped the drivers from keeping or even touching money. Lockboxes were welded to all the bus floors, and riders deposited their fares directly into the lockbox. If you wanted money, you had to steal the whole bus, and it’s really hard to hide a bus in Newark. The robberies stopped.

I kept thinking about this incident when I was reading these three books. Mainly (because I was riding the buses then), I’m grateful that the bus robbery problem was not subjected to the sort of discussion we often have now, in the age of culture wars, as exemplified by these three books. If it had been, well-educated people would have saturated the discussion, assigning blame everywhere: on lenient judges, weak prosecutors, lazy cops, absent parents, elimination of prayer in schools, Michel Foucault, the end of the draft, Martin Luther King, Malcolm X and Stokely Carmichael, Richard Nixon, Gerald Ford, inequality, capitalism, deregula-

tion, low tariffs, the movement of factories to the South, and in general, the ideas of those of a different political persuasion. Nobody would have let the engineers weld lockboxes to the bus floors.

Each of these books has strong points. In all of them, for instance, the last chapter is sensible and interesting. They all have excellent bibliographies that have helped me find work that I had not known about. But to varying degrees they have all been infected by the culture wars virus.

Homelessness Is a Housing Problem, by Colburn and Aldern (CA)

For me, this book was the most comfortable of the three. It's got economics, it's got housing, and that's what I spend a lot of my time thinking about. The book uses a cross-section of point-in-time (PIT) count data by continuum of care (COC) from the Annual Homeless Assessment Report (AHAR) and finds that housing market variables have higher simple correlations with homelessness than practically anything else, especially measures of pathology. This exercise is intended to convince policymakers and the general public of the proposition in the title.

It didn't convince me, because I believed something like that proposition already (more on this later), and I know that simple correlations have very weak implications; they're what you do in kindergarten. But simple is good, and CA are out to convince policymakers and the general public, not me, and so perhaps this approach is the best way to reach the intended audience. Or perhaps not.

The great strength of this book is its emphasis on aggregate data. Generally, when a policymaker asks about homelessness they really want to know about aggregates. Too often homelessness researchers respond with results about individuals. This disconnect, for instance, is the part of the current problem with Housing First in California and the Pacific Northwest. More on that below.

CA's policy recommendations generally agree with the consensus among US homelessness researchers. One small difficulty in this section is their uncritical acceptance of the wildly enthusiastic interpretation of the reduction in veterans' homelessness after several new programmes were launched in 2009. Many commenters like to believe that these programmes were responsible for almost all of that decline. As I showed in O'Flaherty (2019), this interpretation is implausible. During the eight years that followed that programme announcement, the number of veterans under age 65 declined precipitously as the huge Vietnam cohort, the last cohort of draftees, passed that age. Homelessness appears to be quite rare among Americans over 65, as many safety net benefits, and several

veterans' benefits, kick in around 65. This demographic change appears to account for a large proportion, perhaps a majority, of the 2009-2017 decline in veteran homelessness.

My major difficulty with the book is the title and with the frequent references to housing as the root cause of the homelessness problem. Whenever I see the clauses "x is a y problem," or "y is the root cause of x," I know that the culture wars virus is around and mask up. The other two books are also replete with these clauses. Lockboxes would never have been welded to bus floors if the New Jersey transit agency had started thinking in these terms.

Almost any reasonably complex phenomenon has many (perhaps millions) of but-for causes (the only type of causes I know how to think about). The Newark buses, for instance, were being robbed because they carried US currency, because many firms accept US currency as a means of payment, because the bus drivers were unarmed, because some Newarkers had larceny in their hearts, because some of those Newarkers were not incarcerated, because there were lots of buses running, because deterrence was insufficient, because no earthquakes had shattered Newark and shut down the bus system, because Newark air had oxygen, and so on. Remove any of these but-for causes and robberies fall drastically. Nothing is a root cause; bus robbery is not any x problem. Arguing about these terms is fatuous.

There are generally two different reasons why we may want to draw particular attention to a small subset of causes, rather than all of them: the explanatory reason and the policy reason.

In explanatory investigations, we try to see whether variation in particular causes over a particular stretch of time or space matches variation in the *explanandum*, the effect we are studying. So, for instance, if we try to explain variation in bus robbery in Newark in the 20th century, we can dismiss severe earthquakes as an explanatory cause, because there happened to be no variation in severe earthquakes in Newark in that period. This does not mean that earthquakes could never be an explanatory cause (if we looked at San Francisco in the 20th century, we might find something different), only that in this particular time and place, earthquakes were not an explanatory cause.

In contrast, policy investigations are forward-looking rather than backward-looking, and involve costs and value judgments. The idea is to find the subset of but-for causes that can be manipulated to bring about the desired changes in the *explanandum* at lowest cost, all things considered. So, for instance, we might veto

the idea of shutting down the entire bus system to reduce robberies because of its enormous cost, and so we would not consider the operation of the bus system as a policy cause.

Notice that good explanatory causes are not necessarily good policy causes, and vice versa. When the New Jersey transit agency was thinking about the rise in bus robberies, unwelded lockboxes was not an explanatory cause, but it was an excellent policy cause. This setting leads to a reasonably precise statement of the relationship as we know it now between housing and homelessness, without any appeal to “root causes.”

First, housing market variables, especially measures of rent that people at the margin of homelessness might pay and variables correlated with this, explain a lot of the variation in measured homelessness in the US for the period since around 1975. In this time and place, housing variables are a decent explanatory cause, and we currently know few other variables about which this can be said.

Second, at this point, interventions and policies involving housing subsidies account for almost all the tools for which we have decent evidence of efficacy in reducing homelessness (see Evans et al. (2021) and O’Flaherty (2019) for details). Assertive Community Treatment and Critical Time Intervention have demonstrated some efficacy and don’t necessarily involve housing subsidies directly, but they are difficult to scale without housing subsidies. That doesn’t mean that no one will ever find an efficacious intervention that doesn’t involve housing subsidies. The New Leaf Project in British Columbia, for instance, has some encouraging but preliminary results about unconditional cash transfers (Foundations for Social Change, 2022). But today, for the policy maker who wants something off the shelf to reduce homelessness, the only shelf that has been stocked is labelled “housing subsidies.”

San Fransicko, by Shellenberger

While CA seem to have been infected with a mild case of culture wars that leads them to express accurate statements in sloppy ways, Shellenberger has a much more serious case. Somehow Foucault and Marx end up causing tent cities in San Francisco. An undefined group of people he calls progressives collude to make everything terrible—although, somehow, on their watch the Bay Area has become

the richest large metropolitan area in the US, possibly the world. Reading this book is like being pummelled by a firehose squirting anecdotes, selective quotations, and misinterpretations¹ at you.

But if you ignore these distractions, the book has some interesting propositions. It denies that “housing is the root cause of homelessness,” but as we have seen, this proposition is vacuous to begin with. More seriously, it argues that (1) mental illness and substance misuse are policy causes of homelessness in the sense that certain policies (mainly coercive) directed toward those conditions would reduce both homelessness and those conditions; and (2) these conditions are explanatory causes of homelessness in the US, and in San Francisco in particular. He sometimes seems to imply (3), aside from his recommended policies on mental illness and substance misuse, no other policies or interventions are likely to reduce homelessness substantially. I’m not sure how strongly Shellenberger asserts this last proposition (3), and since it’s known to be false (see Evans et al, 2021 and O’Flaherty, 2019), it’s not worth going into detail about.

Surely, homelessness is correlated with mental illness and substance misuse in California and elsewhere. The best recent study of severe mental illness in California is Caprara et al. (2022). They link records from Los Angeles outreach teams to medical records and find that 17% of unsheltered people had a diagnosis of severe mental illness in the previous five years. Of those, 7% had a diagnosis of a psychotic spectrum disorder (psychotic or delusional disorder, schizo-affective disorder, schizophrenia, schizotypal disorder, bipolar disorder with psychotic symptoms) and 10% had a diagnosis of another severe mental illness. These proportions are well above those observed in the general population, but below those often found with methods that rely on untrained or hasty observations.

¹ For instance, on pp.41-42, he writes, “And those who put many of the stricter drug laws into place did so because they were under pressure to protect African American communities suffering from violence associated with gang warfare over open-air crack markets. Notes Pfaff, ‘when prosecutors weren’t too concerned about drug crimes, they simply ignored the Rockefeller Drug laws...’” In the quoted passage, Pfaff (2017) was emphasising the discretion that prosecutors have in enforcing drug laws and did not state that it was concern for African American victims that made them start doing so. In general, Pfaff is somewhat cynical about prosecutors’ motives, and two of his major proposals are toward raising the influence of urban voters in prosecutorial elections (pp.214-216). Similarly, Shellenberger (p.38) says, “Some long time Housing First advocates suggest the movement has become dogmatic,” and goes on to quote Dennis Culhane. The Culhane quote looks like an attack on the qualifications and mind-set of Housing First advocates. In context, Culhane (2022) said that Housing First is highly successful for the majority of people, but that we shouldn’t be ideological about it in the sense that for some groups of people some other programmes probably work better.

But for Shellenberger's first argument to work, this correlation has to be causal. The best studies—in fact the only serious work I'm aware of on this question—are several papers by Julie Moschion and Jan Van Ours (two of them are also co-authored with Duncan McVicar). These papers use a unique Australian data set, Journeys Home. Since the 1980s at least, researchers have debated how the individual-level association between homelessness and substance use and mental illness (SUMI) worked: did SUMI cause homelessness, or did homelessness cause SUMI? Since no data were around then to shed light on the issue, researchers just yelled at each other. Shellenberger continues this tradition by yelling loudly that SUMI causes homelessness (Eide yells the same thing, but less loudly).

Moschion and Van Ours don't have to yell because they have the right data. Journeys Home is a longitudinal data set that follows over a thousand extremely disadvantaged Australians for about three years in the early teens of this century. It has extremely rich covariates. With Journeys Home, Moschion and Van Ours and co-authors can observe people entering and leaving homelessness, as well as entering and leaving spells of intense SUMI.

In McVicar et al (2015) and Moschion and Van Ours (2015), they examine the relationship between homelessness (using various definitions), and substance use (illicit drugs, cannabis, and alcohol, together and separately). They ask two main questions: Are people who use particular substances more likely than people who do not to transition from non-homelessness to homelessness, holding everything else constant? And are people who are homeless more likely to transition from not using to using particular substances than people who are not, holding everything else constant? A "yes" answer to the first question is a necessary condition for substance use to lead to homelessness (as Shellenberger contends). A "yes" answer to the second question is a necessary condition for homelessness to lead to substance use (as some other authors have contended).

McVicar and Moschion and Van Ours answer no to both questions almost always – alcohol use may lead to a condition somewhat more general US sheltered homelessness, and a smaller though still significant rise in unsheltered homelessness. In general, neither causes the other, although people who tend to use substances tend to become homeless, and vice versa.²

² Two other papers find similar results with a longer time horizon. McVicar and Moschion and Van Ours (2019) find that only daily cannabis use by young men increases the risk of homelessness in the next decade or so—other substances have no effect for men, and none affect women. Moschion and Van Ours (2022) find that only depression among young people increases the likelihood of homelessness over a similar time span; anxiety disorders, bipolar, schizophrenia, and PTSD have no impact.

There is nothing strange about the relationship they found; it just never occurred to people who like to shout about homelessness. As an example of a relationship like the one McVicar and Moschion and Van Ours find, consider the positive correlation in the US between liberal political views and preferring cats to dogs as companion animals (e.g., Ivanski et al., 2021). Almost no one explains this correlation by arguing that one attitude causes the other. Instead, the correlation probably arises because some of the same things that predict preferring cat companionship (being female, living in a small apartment, not seeking predictability or hierarchy, for instance) also predict politically liberal attitudes in the US.

Using a US longitudinal data set, Geller and Curtis (2011) similarly find that substance use does not predict future homelessness in a data set of young urban non-residential fathers, once other controls are added (table 4).

Moschion and Van Ours (2021) applies approximately the same approach as their earlier paper to mental illness and finds a similar answer. Their measure of mental illness every six months is the respondent's answer to the question: "Have you been diagnosed with any of the following conditions by a doctor or a health professional, such as a psychologist or a psychiatrist since [date of last interview]?" The conditions were: depression, anxiety disorder, bipolar affective disorder, schizophrenia, and post-traumatic stress disorder. They found that depression increased transitions to precarious housing, but no condition led to homelessness in the US definition, and the US definition of homelessness led to no psychological condition.

These results are consistent with what randomised control trials (RCTs) generally find. If homelessness does not cause SUMI, then RCTs where the treatment is primarily provision of housing should not usually reduce SUMI. This is a good description of the average results in Housing First RCTs (see Kertesz and Johnson, 2017). If SUMI does not cause homelessness, then interventions that reduce only SUMI will not affect homelessness. This is a good description of the most common outcome of such RCTs (see Rosenheck, 2010; Evans et al., 2021).

To be sure, the Moschion and Van Ours results do not rule out the possibility of interventions that reduced homelessness at the same time that they reduced SUMI, even though we know few or none now. An intervention could do this if it ameliorated some condition that was causing both homelessness and SUMI. Such an intervention would be wonderful, but finding it would be hard, since we know very little about what conditions are responsible for the observed correlation between homelessness and SUMI.

By analogy, there probably are some conditions that both make people liberal politically and cat loving (Ivanski et al., 2021 have some tentative leads). Some of them may be easily malleable (others, such as gender, may not be). But we don't

know what they are yet and producing an advertising campaign today that would simultaneously increase votes for liberal candidates and cat adoptions would be considered an extremely difficult task. Practical people usually don't try to do it. A person who was either trying to get liberal candidates elected or a person trying to get cats adopted would not turn to such a campaign as a first resort.

Thus, Shellenberger's first proposition about SUMI as a manipulable cause of homelessness is not likely to be true now or in the foreseeable future. It is not a proposition on which action could be based today.

What about the other proposition, that SUMI is an explanatory cause of homelessness? Shellenberger does not claim to have seen any serious study that shows variation in SUMI is significantly correlated with homelessness across COCs, or within COCs over time, and so it is hard to think of SUMI as an explanatory cause of homelessness. CA show a negative simple correlation between a measure of mental illness and homelessness on a state level, and a positive but tiny correlation with measures of drug use. Fargo et al. (2013), with a much larger multi-variable model, show a small and inconsistent relationship between substance use and homelessness, but use only 2009 PIT count data. Even large and significant positive correlations, of course, would not necessarily be causal. However, since SUMI is usually measured poorly, attenuation bias may affect both these studies, resulting in coefficients that are too close to zero.

So, the best that can be said for the explanatory proposition is that most of the evidence against it is pretty weak. On the other hand, no evidence at all supports it—even though it has been prominent in the media for more than 40 years. Such a proposition should not be the foundation of a long book.

Shellenberger, however, has another story: migration. Large numbers of homeless substance users and severely mentally ill people migrate to San Francisco, in Shellenberger's view, and this is what makes San Francisco's homeless population both large and troublesome.

What do we know about migration? First, net migration in the US either of people experiencing homelessness or people experiencing homelessness with SUMI problems has to add to zero, and so in-migration cannot account for homelessness everywhere. That means Shellenberger has to give up the pretence to generality in his subtitle, *Why Progressives Ruin Cities*. Second, even gross migration on average in the US is a small phenomenon among people experiencing homelessness. Meyer et al. (2021) used a nationally representative sample of sheltered people experiencing homelessness in the American Community Survey between 2011 and 2018 to estimate that 9% moved to a different state in the past year—this is gross migration, not net. Finally, local officials and residents often claim that homeless-

ness in their locality is dominated by in-migrants and almost never say that their localities experience outmigration. It is impossible for all the local commentators to be right.

What about San Francisco, then? In the appendix (available in the Columbia University Academic Commons)³, I look at what is actually known about San Francisco and the surrounding counties. The information is far from definitive, but it suggests quite strongly that net migration is not a major reason for San Francisco's disproportionately large homeless population.

So as a book about homelessness, *San Fransicko* goes nowhere. It does, however, have interesting things to say about mental illness and substance use—mental illness especially. These are serious problems that are associated with a great deal of pain and suffering (whether or not they have an explanatory or policy connection with homelessness), and that's why we should be concerned about them.

Homelessness in America, by Eide

Eide's *Homelessness in America*, on the other hand, is much more carefully argued, much more surprising, and much more willing to follow a line of reasoning to its logical conclusion. For readers of the *European Journal of Homelessness*, this is the book I recommend if you are going to read one of these three. You'll find a higher proportion of propositions you mainly agree with in CA, but these are things you mainly know already. I also recommend reading this book in reverse (by chapter, of course, not by letter). The last few chapters are crisp and clear; the first few are mired in culture wars.

The book's greatest strength is as a warning against hubris in homelessness research. We really don't know a lot of good, cheap strategies for reducing aggregate homelessness, and we do everyone a disservice by claiming that we do or acting as if we do. Keynes (2010 [1932], p.332) said something similar about economists about a century ago: "If economists could manage to get themselves thought of as humble, competent people on a level with dentists, that would be splendid!" Keynes sets the right standard for homelessness researchers, too. Eide shows how homelessness research should be humbler in several areas.

For instance, he does a good job in raising questions about assessment practices that try to maximise some aggregate characteristic (like homelessness reduction) when deciding who out of a group of eligible people will receive a benefit and who will not. These assessment practices are formally similar to the algorithms now common in criminal justice to decide questions like pretrial detention and parole.

³ <https://doi.org/10.7916/3043-h542>

For those algorithms, concerns have been raised about whether they contain proper safeguards to prevent racial or gender discrimination (see Yang and Dobbie, 2020, for instance). These concerns also apply to algorithms in the homeless assessment systems. Eide emphasises a broader set of safeguards that are needed—those of fairness. Fairness—part of which is non-discrimination—has taken a back seat to optimisation in many cases, and Eide is right to point out that it shouldn't. Durlauf (2006), Harcourt (2007), and several other authors have made similar points about profiling activities.

Eide also provides an important service by pointing out some problems with popular views of Housing First. On pages 57-58, for instance, he shows that claims that Housing First saves taxpayers money are weak, often relying on both the economic mistake of using average costs rather than marginal, and the philosophical mistake of leaving out benefits to participants and implicitly acting as if programmes for poor people are worthwhile only if they make rich people richer. He also points out the difficulties of believing that Housing First actually does reduce PIT counts. However, his allusion to the rise in homelessness in New York City between 2011 and 2016 is somewhat inappropriate, as this change was driven by a large reduction in the rate at which homeless families received subsidised housing (see, O'Flaherty, 2019).

The question of the effect of Housing First on PIT counts is an important one—since mayors and other policymakers are extremely interested in the PIT count—that has often been misunderstood. The evidence usually cited for Housing First is on the individual level; there has never been an RCT where communities were randomly chosen to implement Housing First and compared with communities randomly chosen not to implement Housing First.

At the moment, Corinth (2017) is the best study of the effect of Housing First, or something like it, on PIT homelessness. This is a panel regression study with AHAR data for period 2007-2014. He finds that on average 100 additional Permanent Supportive Housing (PSH) beds reduce the PIT count by about 10, but the standard error of the estimate is large—anything between 0 and 20 is possible. The major problem with this estimate is that the independent variable is PSH, not Housing First. Many PSH beds are Housing First, but we don't really know whether the marginal PSH bed during this period adhered to Housing First protocols. But I think Corinth's range is plausible.⁴

⁴ Evans et al. (2019) use a shift-share instrument involving veteran population across continua-of-care to study the effect of award of HUD-VASH vouchers, but this instrument violates the exclusion criterion. Funding from several other programmes to combat veteran homelessness, especially Social Services for Veteran Families, is likely to be positively correlated with this instrument as well.

The plausibility is based on a close reading of two well-known RCTs: the HUD-VASH study of PSH (Rosenheck et al., 2003) and the At Home-Chez Soi (AHCS) study of Housing First (Goering et al., 2014). In both studies, the primary emphasis was on days housed—the proportion of days stably housed in AHCS, and simply the proportion of days housed in HUD-VASH. The proportion of days homeless, however, is not just the complement of days housed in either study: in both studies there is a third category, days institutionalised, and in AHCS there is a fourth, days temporarily housed. Table 1, from O’Flaherty 2022, shows the basic results of the two RCTs. AHCS, which was conducted in Canada, did not report days homeless directly; I put that number together using the US definition.

Table 1: Proportion of Days in Various Housing Statuses
Average days over length of the experiment

| Status | Treatment | Control | Difference |
|--------------------------------|-------------------------|---------|------------|
| | <i>At Home/Chez Soi</i> | | |
| Emergency shelter | 6% | 16% | -10% |
| Street | 3 | 8 | -5 |
| Subtotal, homeless (US) | 9 | 24 | -15 |
| Temporarily housed | 12 | 33 | -21 |
| Institutions | 9 | 11 | -2 |
| Stably housed | 73 | 32 | +41 |
| | HUD-VASH experiment | | |
| Homeless | 14.5% | 22.7% | -8.2% |
| Institutions | 19.2 | 24.0 | -4.8 |
| Housed | 66.0 | 52.9 | +13.1 |

Notes: At Home/Chez Soi. Derived from text in Goering et al. (2014) on p.17 (days stably housed) and p.18 (days not stably housed). The sum of the categories for the treatment group is only 97%; the text does not explain why. This includes both treatment arms (Assertive Community Treatment and Intensive Case Management), and all five sites. It covers all days for 24 months of the intervention.

HUD-VASH experiment. Derived from table 2 (p.945) in Rosenheck et al. (2003). “Treatment” is considered “Group 1: HUD-VASH” and “control” is “Group 3: Standard Care.” The original table reports average days out of 90; I divided by 0.9 to get percentages. In the original table, both groups in fact sum to 89.69 instead of 90. Participants were interviewed at six months, one year, 18 months, two years, and three years, and asked about housing in the previous 90 days. Numbers in table are averages over all these interviews. “Homeless” category in includes sleeping in “a substandard single-room occupancy hotel” (p.942).

Both RCTs show robust increases in days housed, and AHCS shows an almost spectacular increase in days stably housed. This is very impressive and is a solid reason to believe that Housing First helps its participants. But the decrease in proportion of days homeless—15% for AHCS and 8% for HUD-VASH—is well less than 100%. A decrease of 100%, of course, would be impossible, because the control group was homeless only 24% of the time in AHCS and 22.7% of the time in HUD-VASH. To a first approximation, if actual programmes operated precisely like these RCTs, we would be looking for a decrease on average of eight to 15 for each 100 Housing First or PSH beds.

But actual programmes do not operate like RCTs. Participants start at various times, and when a participant leaves a new one usually replaces them. The homelessness-reducing effect of a Housing First bed depends on the length of time its occupant has been in the programme, and so the average reduction depends on the length of time the average participant has been in the programme. I experimented with various scenarios of programme expansions and exit rates and found that most of them resulted in the average participant being in the programme a length of time that was more homelessness-reducing than the average length of time recorded in the RCTs. Most of the values for the average PIT count reduction for 100 beds end up being in the range of 10 to 20 in these scenarios. That is why I find the Corinth results plausible—their overlap with the range that RCTs would lead you to expect to see (O’Flaherty, 2022).

There are two reactions to this conclusion. The first is the one that Eide would emphasise: as humble competent dentists, homelessness researchers should be modest about what we claim. A hundred PSH beds will reduce the PIT count by 10 or maybe 20 if all goes well over the next few years; it will not remove 100 people from the streets. Over-promising is dangerous.

The second reaction is one that Eide misses. There isn’t much that is better than this, as far as we know (see, Ellen and O’Flaherty, 2010, p.12, note 3 and Corinth, 2017, p.79). The responsible policy at this time is to rely heavily on Housing First—not because it’s spectacular at reducing the PIT count, but because nothing else is better.

To be sure, Eide seems to recognise this (p.63):

Resistance to Housing First can tempt people into promoting mediocre programs or programs they know very little about... Any temporary intervention risks seeing its gains diminish over time. That goes double for the hardest cases. Even well-run programs will often fail. Programs that do succeed often do so based on certain conditions, which limit the ability to scale them.... We should explore alternatives to Housing First—it does not enjoy the funding or the reputation it now enjoys—while also being humble about what to expect from them.

I agree with most of this (except for the word “funding”). But what is supposed to happen while this long exploration is going on? Competent dentists don’t tell patients to go away and come back in a decade in the hope that better treatments will be discovered.

Eide also does a useful service in pointing out that moral hazard permeates many programmes and policies for homelessness, and that moral hazard is costly. Promising housing subsidies to people experiencing homelessness can encourage people to enter homelessness or discourage them from leaving it, Housing Choice

vouchers can discourage people from working or sharing households, clean needles can encourage substance misuse, and so on. Advocates sometimes act as if these possibilities are not real, and Eide is correct to say that they are real.

Moral hazard, however, is like COVID: although it's everywhere and cannot be ignored, the sensible reaction to its presence is not to despair, but to measure, mitigate, and turn to science to figure out how to deal with it.

Consider structure fires, for instance. For most American households and businesses, a structure fire will trigger a generous pay-out from their fire insurance company, and a speedy and free response by their local fire department. The latter is in fact an entitlement in most cities. Moral hazard is obviously present: people take fewer precautions than they would if they had to bear the full cost of their careless activities (again I failed this year to check the batteries on my smoke detectors during fire prevention week).

Yet by and large, Americans have not given up on fire insurance and fire departments, and households and businesses continue to use structures that might burn. The chief policy responses to the dangers of structure fires have been fire codes and the chief private response has been more fire resistant (and expensive) building materials and techniques (the two responses have complemented each other as the possibility of code upgrades probably spurred investment in fire resistance research). The result has been a large decrease in per capita structure fires and deaths from structure fires in the last century or so, and general contentment with the current system. There is no great demand that households and businesses bear more of the costs of their carelessness. I'm not sure the current system is optimal, but I doubt that it's terrible.

Sometimes people have just decided to accept moral hazard without doing much about it. Medical care is an example. Medical care has been replete with moral hazard for centuries but seems not to have found it a large enough problem to require much action in most cases. In an article about ringside doctors at Mixed Martial Arts (MMA) contests and their concerns about facilitating bad behaviour, one doctor, an emergency specialist, compared ringside physicians to pulmonologists who take care of smokers even though they disapprove of smoking, and noted, "We're ER docs. We would have nothing to do all day if it wasn't for bad behavior" (Whang, 2022).

In contrast, corporations face a great deal of moral hazard, too, but great efforts have been made to mitigate it. The corporate form of business organisation presents large opportunities for moral hazard, particularly from limited liability and the separation of ownership and control. Adam Smith (2007 [1776], book V, chapter 1, part III, article 1) was convinced that joint-stock companies (what we now call public corporations) were horrible organisations that should almost never be used:

This total exemption from trouble and from risk, beyond a limited sum, encourages many people to become adventurers in joint stock companies, who would, upon no account, hazard their fortunes in a private co-partnery. Such companies, therefore, commonly draw to themselves much greater stocks than any private co-partnery can boast of.... The directors of such companies,... , being the managers of other people's money than of their own, it cannot well be expected that they should watch over it with the same anxious vigilance with which the partners in a private co-partnery frequently watch over their own. Like the stewards of a rich man, they are apt to consider attention to small matters as not for their master's honor, and very easily give themselves a dispensation from having it. Negligence and profusion, therefore, must always prevail, more or less, in the management of the affairs of such a company.

That negligence and profusion don't always prevail in the management of today's huge joint stock companies in developed countries is probably due to the invention of many ways to combat moral hazard—auditing, chancery courts, and investor protection agencies, for instance. This has taken many years and large investments. Even today, a significant proportion of the research in business schools and law schools is devoted to moral hazard in corporations. People didn't throw up their hands when they realised that joint stock companies come with moral hazard.

Nor should people throw up their hands at the realisation that dealing with homelessness involves moral hazard. The first step is measurement—which occasions of moral hazard are big, and which are small. With Housing Choice Vouchers, for instance, it appears that the work disincentive effects are pretty small, and the household-sharing disincentives are pretty big. In the Welfare-to-Work Voucher Experiment, a large experiment in the early 20th century, a hundred vouchers to welfare (Temporary Assistance to Needy Families) and welfare-eligible families reduced employment by three or four in the first 18 months, and close to nothing after that (Mills et al., 2006, p.99, exhibit 4.8, TOT), but also reduced the number of families that experienced homelessness in a year by about nine (p.139, exhibit 5.3). That might be worth it. But the same hundred vouchers also reduced the number of multi-generational families by about 20 (p.76, exhibit 3.10). All of these effects are likely to depend on the particular structure and rules of the US Housing Choice Voucher programme, which is quite different from anything in Europe or Oceania. The programme could (and probably should) be redesigned to reduce moral hazard, especially in sharing of houses.

Offsetting these three great strengths, the book has three drawbacks, one of which I think I understand now.

The first drawback is Eide's tendency to write gratuitous anti-woke comments for which he has no evidence, and which do nothing to advance his arguments. As an example (p.116): "Homelessness policymakers minimise the mental health dimension of homelessness so as to avoid qualifying the idea of homelessness as a housing problem and, thereby, undermine support for more subsidised housing." No citation.

I don't see how Eide could have obtained this knowledge. I've spent a lot of time with homelessness policymakers—probably a lot more than Eide has—and I really don't know what makes them tick. The proposition is not necessary for his argument; the motivations of people he disagrees with are irrelevant to whether he is right or not. And it hurts his ability to convince people who believe something different (or are homelessness policymakers).⁵

The good news about this drawback is that you don't have to let it annoy you: just tell yourself that Eide conceives of his audience as conservative and has to throw them some red meat every once in a while to make sure they remember he's on their team. I don't know whether that's true or not (I'm not going to do what I just criticised Eide for doing). But if you keep telling yourself that that might be his motive, you can ignore the snippets and appreciate the book.

The second drawback is Eide's misunderstanding of deinstitutionalisation, especially in the 1970s. His approach is too narrow in some ways, and too broad in others. It is too narrow in paying attention only to mental hospitals, while mentally ill people are also institutionalised in nursing homes, prisons, and jails. It is too broad in that it includes elderly people, who are at little danger of homelessness. If you correct for both shortcomings, you find that the working age (18-64) population of institutionalised mentally ill people in the US rose from about 350 000 in 1975 to over 400 000 in 1983 and rose a little more to 1990 (see O'Flaherty, 1996, chapter 12). These numbers are standardised to the 1990 population. If you don't correct for population growth, the increase would be bigger. In short, deinstitutionalisation of working age mentally ill people was over by around 1975; homelessness in the US first rose at the same time that the institutionalised mentally ill population was rising faster than population.

Eide's figures hide this by using the 1970-1980 decade instead of cutting at 1975 and including the elderly. The decrease in mental hospital population in the 1970s appears to be disproportionately elderly. For instance, in New York State, the elderly accounted for 55% of the total reduction of the mental health population

⁵ Another similar example is the adjective "legacy" attached to Catholic Worker houses on p.51. When Dorothy Day died in 1980 there were about 100 Catholic Worker houses. About 60-70 of them appear to have gone under (McKanan, 2008). But the 2022 directory of communities (<https://www.catholicworker.org/communities/directory-picker.html>) lists 187. The Catholic Worker seems to be doing better than the Catholic Church.

between 1973 and 1990, even though they accounted for only 46% of the 1973 population (for details and sources, see O’Flaherty, 1996, chapter 12 and appendix). Deinstitutionalisation is as much of a myth about this period as Ronald Reagan.

The final drawback is Eide’s insistence that homelessness as currently defined in the US is either a “somewhat artificial” (p.1) or “artificial” (p.150) concept. How can it make sense, he argues (p.4) to attach the same term to “[s]omeone evicted due to a short-term bout with unemployment, [a] thirty-year-old man with schizophrenia who has lived on the streets almost his whole adult life[,] [a] young man, who, on a lark, moved to the streets of San Francisco and has a family in the Midwest who would willingly take him back[,] [a] single mother living in a welfare hotel with two children”?

The objection misses a fundamental principle of modern science: all concepts are artificial (except possibly atomic numbers and some other concepts in physics). The philosopher of science Philip S. Kitcher writes (2007, p.299):

I find it hard to envisage nature as prescribing the forms our language should take, as coming nicely organized with fence-posts that our concepts must respect... We make conceptual progress by devising concepts that prove useful for us, with our particular capacities and limitations, to deploy in answering the questions that matter to us, and we should recognize that those questions are historically contingent and culturally variable.... {T}here is a nondenumerable infinity of possible accurate maps we could draw for our planet, and the boundaries they introduce depend on our evolving purposes.

Thus, the four individuals that Eide describes have the same word attached to them if they all lack access to certain capabilities in the ETHOS typology. Similarly, if all of them had a cavity in the lateral incisor of the upper jaw a dentist would find it useful to group them together by that characteristic. Both groupings are “artificial,” like every other grouping science uses (with a few exceptions).

Eide also seems offended by the fact that we use the word “homeless” in a way different from the way it was used in 1960. But we use the words “icon,” “telephone,” and “marriage” in considerably different ways from those of 1960. Back then, only monks and prisoners had cell numbers and Pluto was considered a planet. When the world changes, the taxonomies that are useful change and so do our words.

Eide’s essentialism has consequences for his arguments, which are sometimes led astray by it. Thus, when speaking about Housing First he wants us to believe that it’s horrible when (p.52):

A program that shows no promise whatsoever at addressing behavioral and unemployment challenges will keep being funded as long as it's hitting its housing metrics. Excellence in homeless services consists of keeping as many people stably housed for as long as possible.

If those researching homelessness are following Keynes' advice and striving to be like competent dentists, this description of a horrible programme would read:

A program that shows no promise whatsoever at addressing behavioral and unemployment challenges will keep being funded as long as it's hitting its *dental* metrics. Excellence in *dental* services consists of keeping as many people *with well-functioning teeth and gums* for as long as possible.

Phrased in these terms, what Eide decries makes perfectly good sense. No one would object to it; this is what we in fact expect from dentistry. No one should object to the same statement about homeless services, *mutatis mutandis*.

Conclusion

While Eide is incorrect in asserting that the current concept of homelessness is illegitimate because it is artificial or different from the 1960s usage, proponents of the term still need to defend it by showing that it's useful.

In a scientific sense, that has been obvious for at least a decade or so, as old questions have been tentatively resolved, new questions have been presented, new data sets have been developed, new methods have been employed, and new scholars—both already established and not—have turned their attention to the field. Those who are mired in 20th century culture wars are missing the excitement. The case can also be made—much more tentatively, of course—that the research that has relied on this concept has inspired reforms and innovations that have improved people's lives (although not necessarily as much as has sometimes been claimed). Finland has almost ended homelessness (Allen et al., 2020) and most of the US experienced a long decline in homelessness before the pandemic. Innovations like Housing First and 100 000 Homes appeared to have made an impact (Leopold and Ho, 2015), and US veterans' homelessness has probably declined, even correcting for demographic change.

Modern homelessness research was born amid the culture wars of the 1980s and 1990s. The wars have always been present. But we shouldn't be obsessed by them.



Acknowledgements

I am grateful to Julie Moschion for wisdom and comments and to Dennis Culhane for personal communications.

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