Housing for Health: Impacts of Permanent Supportive Housing

A Critique of "Evaluation of Housing for Health Permanent Supportive Housing Program" conducted by Sarah Hunter, Melody Harvey, Brian Briscombe, and Matthew Cefalu

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Introduction

With California state legislators requesting a state of emergency on homelessness to be declared in 2016,[1] a solution to combat homelessness in the state was needed. A solution proposed in this same year was approved by the Los Angeles County Board of Supervisors, which included expanding services provided by Housing for Health.[2] Housing for Health is a permanent supportive housing program that "[provides] supportive housing to DHS patients with complex medical and behavioral health issues…experiencing homelessness."[3] Permanent supportive housing is a type of intervention that exists beyond Housing for Health, categorized by pairing housing with case management and other types of supportive services.[4]

In addition to permanent supportive housing, Housing for Health offers two other programs: recuperative care and stabilization housing. While recuperative care provides immediate crisis assistance, such as food, medical services, and regular programming, stabilization housing provides temporary, transitional housing for people waiting to receive permanent supportive housing.[5] Although the evaluation conducted by RAND focuses on the permanent supportive housing component, it is unknown in this sample where individuals received the other two components while waiting for permanent supportive housing.[6]

This is an important consideration as the evaluation relies on a pre-post study design to infer impacts of permanent supportive housing. After utilizing DHS services in LA County, a service provider would refer the patient to apply for permanent supportive housing. After an average of 4 months' wait, the client receives intensive case management services (ICMS). After an additional average of 3 months' wait, the client receives permanent supportive housing.[7] This 7-month waiting period is captured in the pre-period where clients may also receive other services, such as recuperative care or stabilization housing.

Study Overview

The authors aim to extrapolate the impact of receiving permanent supportive housing in their evaluation. They explore three research questions: (1) whether "[program] recipients use fewer public services in the year after receipt of housing compared to the year prior," (2) whether there are overall savings to the county, and (3) whether there are changes in health functioning.[8] This critique focuses on the first question, use of public services by program participants in the year

pre- and year post-housing. There were several outcome variables of interest, including the use of medical services (ER visit days, hospitalization days, outpatient visit days), mental health services (inpatient/outpatient service days, crisis stabilization services, substance abuse service days), and general relief & legal services (months of general relief received, days incarcerated, days on probation).[9] The authors find statistically significant decreases in service utilization in several categories, including ER visit days, hospitalization days, outpatient visit days, mental health outpatient services days, and months of general relief received.[10]

This data was obtained from the DHS and Housing for Health. While the authors had access to data from the Enterprise Linkages Project (an integrated data system including administrative data from across the county),[11] they opted to use only DHS data as they felt it was more complete.[12] However, the authors point out significant discrepancies in the DHS data. Specifically, they note that although it was part of the eligibility criteria to have been seen at a DHS facility prior to receipt of housing, one-third of participants did not have any documented use of these services.[13] This influenced their choice not to use the dataset to find a comparison group, as they were not confident the data was accurate. They argue, however, that the data was missing at random and included all participants, regardless of their pre-housing utilization rates, in the pre/post design.[14]

Study Methods

To estimate the impact of receiving permanent supportive housing, the authors used a robust Poisson Generalized Estimating Equation regression. This model is used because it accounts "for the correlation of observations within [program] clients across time."[15] They control for demographics, such as age, gender, and race, whether a client received case management, and use fixed-year effects. The study reports both the simple difference of means, and the regression results which show the estimated effect of permanent supportive housing (represented by the coefficient of interest, θ).

The regression equation used is: $\log\big(E(SERVICEUSE_{it})\big) = \beta_0 + \beta_1 AGE_i + \beta_2 FEMALE_i + \beta_3 RACE_i + \beta_4 year_{it} + \theta PSH_{it} + \phi CM_{it}$, where:

- $SERVICEUSE_{it}$ indicates the expected value of the outcome of interest for the i^{th} client at quarter t.[16]

- $year_{it}$ is "a set of indicators of the calendar year of the i^{th}…patient's t^{th} quarter."[17]
- PSH_{it} is "a dummy variable for treatment, indicating whether the patient [i] received permanent supportive housing from [the program] in quarter t,"[18] and θ "represents the impact of receiving permanent supportive housing."[19]
- CM_{it} is "a dummy variable for the case management component of [the program], indicating whether the patient [i] received case management from [the program] in quarter t,"[20] and ϕ "captures the effect of the case management component of the program."[21]

Critique 1: Case Management

There are two core issues with how case management is used in this evaluation: (1) using it as a control, and (2) contamination of the pre-period with treatment. Regarding using it as a control variable, permanent supportive housing involves intensive case management as part of the program. It is plausible to argue that case management improves the effectiveness of the program. In addition to weekly duties, case managers assist clients in the housing transition, link clients with various medical and support services, and help prevent eviction.[22] By controlling for the receipt of services that impact the outcome variable of interest, the reported impact of the program could be underestimated.

The second issue is contamination of the pre-period with treatment. As previously mentioned, while clients are waiting to receive permanent supportive housing, they receive case management services. To demonstrate the problem, imagine this case: you are homeless, and you've just had a crisis which warranted a trip to the emergency room. You wait 4 months and finally receive a case manager. You had no awareness of services you could use to help maintain your condition, but your case manager is able to link you with these services, and you receive the services. Then, after 3 months go by, your condition has resolved because of the additional medical care and case management services. Your wait is up to receive housing, and all that service utilization is captured in the pre-period.

The problem is multifaceted: first, receiving case management could inflate pre-period service utilization. Due to receiving case management, clients may utilize more services during this period than they would have otherwise. Consequently, the pre-period may no longer

accurately represent an untreated baseline, harming the ability to disentangle the effects of permanent supportive housing from the effects of earlier case management interventions. It could be the case that crisis stabilization on behalf of the case manager worked so well that it influences the decline in services seen in the post-period, more so than what can just be attributed to receiving housing.

Critique 2: Missing Data

While the authors acknowledge the likelihood of missing DHS data for existing participants, I argue that the problem of missing data is larger than that. The authors note that only one in three eligible patients that were referred to Housing for Health successfully enrolled in the program.[23] This is plausible evidence that the program is systematically missing enrollment for a specific type of client. A likely cause is that the most unstable, vulnerable individuals may not make it through the 7-month waiting period to receive housing. This could be caused by many factors, such as instability, higher mortality rates, or difficulty maintaining contact.

If individuals who do not make it through the waiting period are systematically higher utilizers of services on average, this could bias the results towards zero, meaning the observed effect of the program is understated. This aligns with existing literature: the literature review conducted by the authors found that "programs targeting sicker populations demonstrated larger cost offsets"—cost offsets derived from the overall decrease in service utilization. This indicates that capturing more of the eligible population could have shown greater impacts of the program. This is shown in Figure 1.

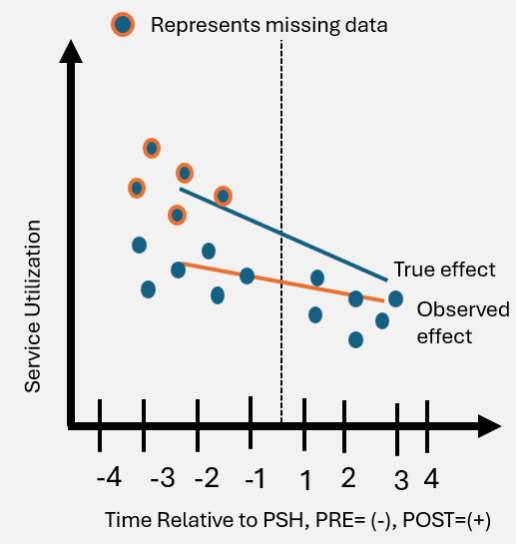

Figure 1: How Would Missing Data Bias the Results?

Critique 3: Regression to the Mean

There is a key assumption lingering in this evaluation design that is unlikely to hold. The assumption is that, absent of the treatment, service utilization is expected to stay the same. This

assumption underpins the entire design in the sense that the authors are attributing all decreases in service utilization in the post-period only to housing receipt. However, someone experiencing a crisis is likely to see a drop in serve utilization after the crisis, a common phenomenon known as regression to the mean. This problem is compounded by the earlier discussion of case management occurring during the pre-period. These services are specifically geared to alleviate a crisis, and receiving this type of intensive care in the pre-period could also influence a quicker return to baseline.

This problem is demonstrated graphically in Figure 2. The assumption of the study is indicated by the implied difference-in-differences counterfactual (DiD CF), shown as a dashed orange line. This demonstrates what the study design implies would have been the case had the intervention not occurred, and the treatment effect observed in the study is the distance between the solid blue treatment line and the DiD CF line. However, the true counterfactual, or what would have happened absent of the treatment, is likely somewhere in the middle—service use probably saw a greater decline than it would have seen because of the program, but overall, the effect is likely overstated. The true impact of the program is more likely the smaller distance between the true counterfactual estimation and the treatment line.

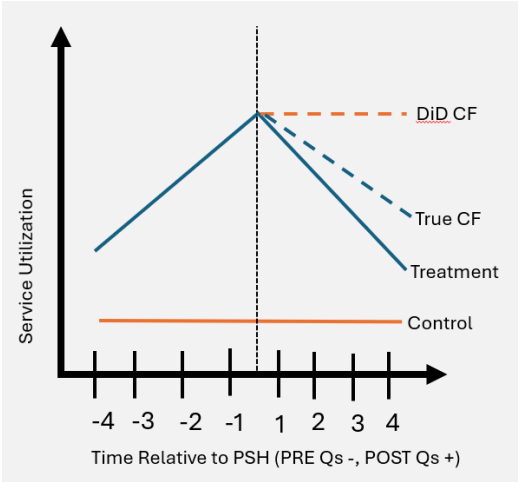

Figure 2: How Would Regression to the Mean Bias the Results?

Conclusion

While the results of this study are promising, they warrant a better evaluation design to properly assess the impact of case management, housing receipt, and how their interaction specifically improves outcomes. While controlling for case management and missing data could cause the overall effect of the program to be understated, I argue that regression to the mean is a more significant driver of bias, likely leading to the program effects being overstated. These combined issues create uncertainty not just about the magnitude, but also the direction of the bias, causing difficulties interpreting whether the program's effect is larger or smaller than reported.

Most importantly, the foundations of this study pose significant difficulties interpreting the results. There is certainly selection bias, evidenced by only one in three eligible participants successfully enrolling.[24] While what exactly is different about the population able to successfully enroll compared to those who did not is unknown, it is plausible that they are systematically different in a way that imposes bias. However, what makes the effects of the program most difficult to disentangle is the existence of treatment in the pre-period. Not only do participants receive case management in the pre-period, but they also might receive other support services, such as interim housing or recuperative care.

Without an untreated baseline for comparison, it becomes impossible to clearly isolate the effects of permanent supportive housing from the effects of earlier interventions or natural recovery processes. While the intent and goals of Housing for Health are commendable, to be used as the basis for a policy decision, future evaluations are needed that address these design limitations. Specifically, using stronger comparison groups, better accounting for interim services received, explicitly modeling regression to the mean, and assessing case management as an integral component of permanent supportive housing, would all help to more accurately capture the program's true impact.

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[1] Hunter, Sarah B., Melody Harvey, Brian Briscombe, and Matthew Cefalu. *Evaluation of Housing for Health Permanent Supportive Housing Program*: RAND Corporation, December 2017, pg. 1.
[2] Los Angeles General Medical Center. *Housing for Health.* Retrieved from https://dhs.lacounty.gov/lageneral/our-services/housing/
[3] Hunter, Sarah B., Melody Harvey, Brian Briscombe, and Matthew Cefalu. *Evaluation of Housing for Health Permanent Supportive Housing Program*: RAND Corporation, December 2017, pg. iii.
[4] National Alliance to End Homelessness. April, 2023. *Permanent Supportive Housing.* Retrieved from https://endhomelessness.org/resources/toolkits-and-training-materials/permanent-supportive-housing/
[5] Hunter, Sarah B., Melody Harvey, Brian Briscombe, and Matthew Cefalu. *Evaluation of Housing for Health Permanent Supportive Housing Program*: RAND Corporation, December 2017, pg. 6.
[6] Ibid, pg. 7.
[7] Ibid, pg. 33.
[8] Ibid, pg. 9.
[9] Ibid, pg. 39.
[10] Ibid, pg. 39.
[11] Ibid, pg. 12.
[12] Ibid, pg. 13.
[13] Ibid, pg. 20.
[14] Ibid, pg. 20.
[15] Ibid, pg. 17.
[16] Ibid, pg. 16.
[17] Ibid, pg. 17.
[18] Ibid, pg. 16.
[19] Ibid, pg. 17.
[20] Ibid, pg. 16.
[21] Ibid, pg. 17.
[22] Ibid, pg. 24.

[23] Ibid, pg. 21.
[24] Ibid, pg. 21.