

## Introduction

This report contains a critical analysis of “Halfway Home? Residential Housing and Reincarceration” by Logan M Lee<sup>1</sup>.

### Background

*“With nearly two million people behind bars at any given time, the United States has the highest incarceration rate of any country in the world. We spend about \$182 billion every year — not to mention the significant social cost — to lock up nearly 1% of our adult population.”<sup>2</sup>*

In 2015 alone, more than two-thirds of the 640,000 prisoners released from state and federal prisons were expected to be back in prison again within 3 years. (Carson and Anderson 2016; Durose, Cooper, and Snyder 2014). This vicious cycle of recidivism is perpetuated by significant barriers to accessing healthcare, accommodation, employment opportunities, and the absence of a social safety net for a significant subset of these released. Furthermore, a study from Illinois revealed that the average cost per instance of recidivism amounted to \$151,662. (Steinfeld, Powers, and Saltmarsh 2018) which doesn’t entirely capture the perceived costs on society through loss in the workforce, collateral damage, and finally the effect on citizens’ physical and mental well-being.

Halfway homes are a way to potentially break this cycle by enabling access to stable housing in neighborhoods with lower crime. However, the author is skeptical about their efficacy in comparison to measures like parole for three reasons. First, these facilities impose heavy restrictions on residents through stringent supervision, thus mirroring their previous circumstances and potentially subjecting them to additional duress. Second, they also put people with vastly different criminal histories in proximity and an individual who was previously unlikely to commit serious crimes might be negatively influenced. And finally, residential housing is expensive. With daily costs amounting to 14 times that of parole, and Iowa alone spent about 15 million on them in 2018.

### Outcome of Interest

Considering the previous concerns, the author aimed to examine whether halfway housing is a good solution to combat re-incarceration. Thus, the research question explores the causal effect of halfway housing on the likelihood of re-incarceration within three years of release compared to an alternate approach like parole.

This research is based on administrative individual-year-level records for 7,903 incarcerated men who were eligible for halfway housing between 2011 to 2014 coming from the Iowa Department of Corrections (IDOC 2017). It contains demographics, case details, case manager details, residential housing details, and recidivism-related information for all the prisoners in the state. Since a simple OLS regression is marred by selection bias (explored later), the author used a 2SLS

regression approach where the instrument is the propensity of residential housing assignment recommendation by randomly assigned case managers.

The first stage equation below explores the correlation between this instrument and our independent variable (Residential) which is binary w.r.t the assignment to residential housing (or halfway housing). The unit of analysis is individual  $i$ , with case manager  $c$  in prison  $p$  for year  $y$ . He has also included prison-year fixed effects ( $\phi_{py}$ ) and controlled for various prisoner-crime characteristics ( $X'$ ).

$$Residential_{icpy} = \alpha Instrument_{icpy} + X'_{icpy}\beta + \phi_{py} + \epsilon_{icpy}$$

The second stage equation given below regresses this predicted Residential variable to estimate the local average treatment effect of residential housing on our outcome of interest: Recidivism (Recid) which is equal to 1 if the person was re-incarcerated within 3 years.

$$Recid_{ic} = \theta \widehat{Residential_{icpy}} + X'_{icpy}\Gamma + \lambda_{py} + \epsilon_{icpy}$$

The paper found no evidence to show that residential housing reduces re-incarceration. Although there was some evidence to suggest that drug-related offenses were lower (possibly because of the stricter supervision), there was a statistically significant difference of 14.4 ppt in technical violations and 6.9 ppt in violent crime in comparison to people who were assigned to parole.

### Selection Bias in OLS Regression

As discussed earlier, running a simple OLS regression to estimate the causal effect would have been heavily affected by selection bias. This was confirmed by the statistically significant differences between the pre-assignment characteristics of the people assigned to halfway housing and those assigned to parole. Differences in some key characteristics raised concerns since they could potentially bias the OLS estimates in either direction.

First, people assigned to residential housing have committed a higher number of prior crimes, had a longer jail stay period, and had fewer people visiting them during their stay. People who have exhibited violent behavior in the past might not have a stable social or financial space to return to and could fall back into the cycle of committing crime again. This will generate an **upward bias** on the OLS results because the observed value of our outcome variable (recidivism) is higher than the potential true value.

Contrarily, the people assigned to residential housing also displayed lower LSI-R (risk) scores, were older, and had fewer misconducts while they were incarcerated. Thus, one could argue that the members of this group weren't very likely to commit a crime again once they were out, thus

generating a **downward bias** where the observed value is lower than the true impact of residential housing on recidivism.

Thus, the overall direction of the bias is unclear from our hypothesis. We can observe from the results that OLS predicts higher recidivism for new crime for the halfway housing cohort but the 2SLS, though lower, fails to present a statistically significant result. However, for technical violations, we observe that the estimated value for 2SLS is higher than that observed in the OLS regression.

	Residential housing (1)	Released to parole (2)	Difference (3)	Balance test (4)
<i>White</i>	0.643 [0.479]	0.711 [0.454]	−0.067 (0.014)	
<i>Black</i>	0.303 [0.460]	0.226 [0.418]	0.077 (0.014)	0.013 (0.008)
<i>Hispanic</i>	0.0336 [0.180]	0.0425 [0.202]	−0.009 (0.004)	0.004 (0.004)
<i>Other Race</i>	0.0206 [0.142]	0.0211 [0.144]	−0.001 (0.003)	−0.012 (0.006)
<i>LSI-R Score</i>	19.33 [7.959]	29.79 [8.039]	−10.46 (0.433)	−0.001 (0.001)
<i>Prior Prison</i>	0.555 [0.497]	0.446 [0.497]	0.109 (0.013)	0.010 (0.007)
<i>Age at Release</i>	36.64 [10.29]	35.58 [10.84]	1.059 (0.287)	0.000 (0.000)
<i>Total Crimes</i>	3.046 [2.308]	2.673 [1.974]	0.373 (0.054)	0.001 (0.001)
<i>Violent Crime</i>	0.198 [0.399]	0.152 [0.359]	0.046 (0.013)	0.007 (0.007)
<i>Drug Crime</i>	0.360 [0.480]	0.382 [0.486]	−0.021 (0.014)	0.001 (0.006)
<i>Property Crime</i>	0.271 [0.444]	0.245 [0.430]	0.026 (0.012)	0.004 (0.005)
<i>Public Order Crime</i>	0.0957 [0.294]	0.162 [0.368]	−0.066 (0.011)	0.001 (0.007)

  

	OLS			2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A. New crime reincarceration</i>						
<i>Residential Housing</i>	−0.014 (0.013)	−0.010 (0.013)	<b>0.014</b> (0.016)	−0.084 (0.059)	−0.085 (0.059)	<b>−0.063</b> (0.053)
Observations	7,309	7,309	7,309	7,309	7,309	7,309
First-stage F-stat				10.4	12	398.9
Dependent variable mean	0.285	0.285	0.285	0.285	0.285	0.285
<i>Panel B. Technical violations</i>						
<i>Residential Housing</i>	0.115 (0.011)	0.122 (0.011)	<b>0.087</b> (0.012)	0.129 (0.057)	0.124 (0.064)	<b>0.144</b> (0.051)
Observations	7,309	7,309	7,309	7,309	7,309	7,309
First-stage F-stat				10.4	12	398.9
Dependent variable mean	0.162	0.162	0.162	0.162	0.162	0.162
Prison-by-case-manager assignment-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Jurisdiction dummies	No	Yes	Yes	No	Yes	Yes
Other controls	No	No	Yes	No	No	Yes

*Notes:* The dependent variable is whether an individual returned to prison because they committed either a new crime (panel A) or technical violation (panel B) within three years of release from incarceration. Residential housing instrumented with the case manager instrument. Standard errors allow for clustering by case manager and are reported in parentheses.

Figure 1: Balance check and OLS results from the paper <sup>3</sup>

## Validity of the Instrumental Variable

To determine whether the results of this analysis are credible, we must check the validity of the instrument used by the author and make sure they meet the assumptions of an instrument variable.

### Relevance

To meet this criterion, the instrument needs to be strongly correlated to assignment to residential housing. Although in this scenario case managers don't assign people to residential housing directly, their recommendation is almost always accepted by the committee in charge of this. This is demonstrated by a very **strong F-Stat of 398.9** in the first stage equation. Thus, this instrument is relevant.

### Independence

To meet this criterion, we need to make sure that the assignment of the case managers is truly random and is not correlated to our outcome of interest which is recidivism. This assumption would be violated if certain case managers were preferred for “harder” cases. If these case managers think that the people assigned to them are likely to commit crimes in the future and always assign them to residential housing; we will face the same problem we faced in the OLS regression and the estimates could potentially have **an upward bias**.

The author perceived this and confirmed that a regression with the case manager instrument and prisoner characteristics variables suggested that the **randomization worked** and only 1 out of the 11 characteristics tested showed a significant difference on a 5% level. Thus, this assumption is also satisfied.

### Exclusion

To meet this criterion, we need to ensure that the instrument chosen doesn’t affect recidivism through any other means than assignment to residential housing. A possible threat to this assumption could be the beneficial influence of a good case manager who would help in better rehabilitation of the individual and turn, reduce chances of recidivism. The author posits that this scenario is unlikely because each manager is assigned to around 100 cases at a time and meets the assigned people once in 6 months. This role has a high turnover rate. Thus, the managers have very limited time to personally influence the prisoners. The author even does a placebo analysis on the impact of case managers on the people who were ineligible for halfway housing to confirm that there were **no noticeable differences**.

## Other Notable Strengths

### Monotonicity

A common pitfall for instruments that use the stringent judge technique is that they fail to be consistent.<sup>4</sup> In our scenario, this would mean that “the case managers assigning a higher fraction of their charges to residential housing should do so for all prisoners they interact with not only those displaying a particular set of characteristics.”<sup>5</sup> The author tests for this assumption by using the first stage equation and comparing the propensity of the case manager to recommend people to residential housing based on different individual characteristics v/s all their overall propensity for recommending the treatment. He found **no significant difference** other than violent crime on a 5% level, thus upholding the monotonicity assumption.

TABLE 3—FIRST STAGE

	All (1)	Prior prison (2)	Violent crime (3)	Drug crime (4)	Property crime (5)	Non-White (6)	Age ≤ 34 (7)	Incarceration ≥ 15 months (8)
<i>Case Manager Instrument</i>	0.817 (0.059)	0.752 (0.078)	0.576 (0.095)	0.976 (0.079)	0.905 (0.078)	0.609 (0.131)	0.775 (0.072)	0.804 (0.061)
Observations	7,309	3,518	1,207	2,720	1,843	2,261	3,773	3,603
First-stage <i>F</i> -stat	398.892	311.736	64.962	119.263	157.841	116.413	164.515	202.852
Ratio		0.920	0.705	1.194	1.108	0.746	0.949	0.984
Difference		-0.065 (0.098)	-0.241 (0.112)	0.159 (0.098)	0.088 (0.098)	-0.208 (0.144)	-0.042 (0.093)	-0.013 (0.085)

Notes: Results based on the first stage of a two-stage least squares regression as described in equation (1). In columns 2–8, the sample is restricted as indicated in the column header. Standard errors allow for clustering by case manager and are reported in parentheses. Difference standard errors are calculated simply as  $\sqrt{(SE_{b_{all}})^2 + (SE_{b_{subgroup}})^2}$  (Clogg, Petkova, and Haritou 1995).

Figure 2: Monotonicity check from the paper<sup>6</sup>

### Robustness

To make the results even more robust, the author dropped the case managers who had handled fewer than 50 cases in this duration, which didn't affect the overall sample size too much. This was done to ensure enough data was available to gauge the case managers' propensity for recommending halfway housing.

### Fixed Effects

The regression compares individuals in the same prison, in the same year with the same case manager with robust controls, and still works enough data. I believe this granularity helps in eliminating a lot of potential omitted variable biases that could have plagued an analysis like this, such as differences in jail stay duration, age range, prison environment, etc.

## Conclusion

I believe the author did a great job of addressing threats to causality while writing this paper. In his conclusion, he emphasizes that for people who are on the margin of being assigned to either parole or residential housing, parole seems to be a more effective way to reduce re-incarceration within 3 years, this effect is even more pronounced when you reduce the period of re-incarceration. I agree with this notion since it seems to be effective not only in terms of our outcome of interest but is also cost-effective and poses a lower infringement on the person's autonomy. I also agree that while eliminating halfway housing is not the goal, a more judicious assignment of this treatment in Iowa might be beneficial.

## References

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<sup>1</sup> Logan M Lee (2023). " Halfway Home? Residential Housing and Reincarceration." *AMERICAN ECONOMIC JOURNAL: APPLIED ECONOMICS* VOL. 15, NO. 3, JULY 2023, pp. 117-149.

<https://pubs.aeaweb.org/doi/pdfplus/10.1257/app.20200150>.

<sup>2</sup> <https://www.prisonpolicy.org/profiles/US.html>

<sup>3</sup> Ibid pg 126,136

<sup>4</sup> <https://blogs.worldbank.org/impactevaluations/judge-leniency-iv-designs-now-not-just-crime-studies>

<sup>5</sup> Ibid, pg 130

<sup>6</sup> Ibid pg,129