# Is Shared Housing a Way to Reduce Homelessness? The Effect of Household Arrangements on Formerly Homeless People

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## Abstract

Most single adults share housing with other adults, and living alone is considerably more expensive than living with someone else. Yet policies that discourage shared housing for formerly homeless people or people at risk of becoming homeless are common, and those that encourage it are rare. This would be understandable if such housing adversely affected its users in some way. We ask whether shared housing produces adverse effects. Our provisional answer is no. For the most part, whether a person lives alone or shares housing seems to make no difference to the outcomes we studied although shared housing is associated with reduced psychotic symptomology. We use data from ACCESS, a 5-year, 18-site demonstration project with over 6,000 formerly homeless individuals as participants.

## Keywords

homelessness; shared housing; mental health; psychotic symptomology

Most single adults share housing with other adults, and living alone is considerably more expensive than living with someone else. In 2005, of adults under 65 in the household population who were not living with spouses, only 23.8% lived alone (American Community Survey). The cost per person of maintaining the same standard of living is probably between 36% and 47% lower in a two-person household than in a one-person household (Nelson, 1993, Lazear and Michael, 1980). Yet policies that encourage shared housing for formerly homeless people or people at risk of becoming homeless are rare. Indeed, the section 8 housing voucher program, one of the most commonly used vehicles to prevent homelessness, implicitly discourages shared housing.

Neglecting shared housing, or even holding a negative attitude toward it, would be understandable if such housing adversely affected its users in some way: made them more likely to re-enter homelessness, or less well-adjusted to their communities, or noticeably less satisfied with their lives. The goal of this paper is to begin to explore whether shared housing produces any of these deleterious effects. Our provisional answer is no.

Our approach is nonexperimental. We use the well-known ACCESS (Access to Community Care and Effective Services and Supports) data set. ACCESS was a 5-year, 18-site demonstration project that examined the effect of service integration among homeless people with severe mental illness. It provides detailed longitudinal data for at least a year for over 6,000 participants. Mares and Rosenheck (2004) also look at the effect of living arrangements on a number of outcomes for ACCESS participants, but they use a different partition of arrangements.

Our interest is in shared housing, not group homes.

We run OLS regressions to determine whether sharing housing is correlated with subsequent changes in mental health and other outcomes. For the most part, we find no statistically significant differences, and can be confident that any difference is economically small. We find only one significant difference between living alone and sharing, and that significant difference indicates that sharing is better, not worse. Sharing housing is associated with less psychotic symptomology. On the other hand, sharing is also associated unconditionally with greater homelessness several months later, but this association is not significant. People who share housing are more likely to be homeless again in a few months, but the reason appears to be that they have less income or that they live in areas with tighter housing markets (or in ACCESS sites with less effective case managers). If the relationship between shared housing and subsequent homelessness were causal, we would expect to see shared housing worsening some intervening variable like addiction or mental illness, and we do not.

We have also used instrumental variables (IV) to look for any causal relationships. We found none. Unfortunately, our instruments were weak. IV results are available in the working paper version of this paper (He et al., 2008).

The plan of the paper is the following. We begin by showing how our question and results are linked to policy questions about shared housing. Our approach to shared housing is somewhat unorthodox (although entirely orthodox within economics) and needs some explaining. In an ideal world, our paper would be presenting a modest result within a huge literature, but unfortunately most of that huge literature has not been written yet. The next section is a literature review. Section 3 is about methods, and section 4 presents results.

## 1. Policy discussion

We are concerned with two areas of policy: programs specifically targeted to homeless people and people deemed to be at risk of homelessness; and broader design issues in many federal housing assistance, food, and income maintenance programs. In both cases, our aim is to advance the policy discussion, not to resolve it completely. Since most programs targeted specifically at homeless people make generous use of the broader federal programs, design issues in those programs are directly relevant to homelessness programs, and so the dichotomy is less stark than it might appear.

### A. The implicit tax on shared housing

Many federal assistance programs impose a substantial implicit tax on shared housing. Supplemental Security Income (SSI), for instance, reduces the payments an eligible person

receives when she lives with an ineligible person. The food stamp program has a series of complex thresholds and calculations that greatly penalize sharing. Ellen and O'Flaherty (2007) use the example of two poor individuals in New York City in 1998; with plausible income and rent values, they would lose 74% of their combined food stamp benefits if they moved in together.

Section 8—the Housing Choice Voucher Program—also taxes sharing. A household with a section 8 voucher pays 30% of its income in rent, and its landlord receives an administratively determined fair market rent (FMR). The FMR for a household of two is far less than twice the FMR for a household of one. (FMRs are based numbers of bedrooms and local surveys, and so no fixed percentages are available, but in New York the FMR for two has been as little as 10-20% greater than the FMR for one.) Thus the rent subsidy an eligible individual would receive if she shared housing is much smaller than the subsidy she would receive if she lived alone.

Consider, for instance, two individuals in New York City, each with an income of $1000 a month, slightly more than the 2008 poverty guideline. If they lived alone in studio apartments, the fair market rent (FMR) for each would be $1091 a month (FY 2009). With section 8 vouchers, they would each pay $300 a month in rent, and so each would receive a subsidy of $791 a month, for a total subsidy of $1582. Suppose instead that they share a 1-bedroom apartment. The FMR is $1180 a month, and 30% of their combined income is $600 a month. The subsidy drops to $580 a month, a penalty of over $1000 a month for sharing housing. (The situation would be even worse if they had incomes of $1300 a month. Living separately in studio apartments they would each be subsidized $701 a month for a combined $1402. But if they lived together they would exceed the eligibility level and they would receive no subsidy.)

Notice that this pair would lose money by sharing even if only one of them had a section 8 voucher: sharing would reduce the subsidy from $791 a month to $580 a month.

The larger policy issue with these programs that we begin to address in this paper is whether this tax is justified or good public policy. The case against such a tax is standard, orthodox, and obvious: it distorts decisions and creates deadweight loss. The implicit tax rate on sharing is high and the empirical literature we survey below indicates that household size is responsive to implicit taxes; thus the deadweight loss this tax creates is likely to be considerable.

The case for this tax has never been explicitly articulated to our knowledge, and so is more difficult to piece together. One classical argument for such a tax would be Pigouvian: sharing creates external costs in some way, and so people's decisions to share housing should be discouraged and not fully respected. The external costs could be physical: shared housing might be dirtier or noisier or disease might spread more quickly in shared housing. Or the external costs might be fiscal: sharing might make people more likely to receive transfers or less likely to pay taxes. Finally the "external costs" might be associated with lack of self-control or judgment errors: people might decide to share housing by mistake or without realizing the objectively bad consequences it might have for them or because of urges they cannot always control. Or parents might decide to share housing without adequately considering possible harm to their children. One might describe this third concern as a paternalistic argument against sharing, rather than a Pigouvian one; we are indifferent about the terminology. In any of these cases, a tax on sharing (or sharing by families with children) of some size (not necessarily so large as the taxes now in place) would be good public policy.

These arguments for a tax on sharing, though, require empirical support. This paper looks for some empirical support in a particular setting where we might expect to find some, and finds none. (Claims could also be made that sharing has external benefits—for instance, that living alone is complementary to leisure, and so should be taxed if labor is taxed and leisure cannot

be.) That we have not found evidence, however, does not establish that it is not somewhere. For instance, the results in this paper can say nothing about the effects of sharing on children.

We do not try to figure out how programs could be redesigned with a smaller tax on sharing or none at all. In particular, we do not discuss whether it would be better to keep the current level of assistance for someone living alone and raise the level of assistance to people who share; or to keep the current level of assistance for people who share and drop the level for those who live alone; or to undertake a convex combination of these reforms. These are important questions, but beyond the scope of this paper.

Even though we find no current justification for the tax on sharing, we are not arguing that everyone should live in a big household. Our argument is for less distorted consumer choice unless solid research demonstrates external or paternalistic costs to sharing.

Thus what we reject as an a priori proposition is that programs should be designed so that participants on average are roughly indifferent to the size of the household they live in. Pendakur (1999, p. 2), for instance, writes: "An accurate equivalence scale may allow decision makers to design transfer programs that do not create incentives for program participants to change their household type to increase their level of welfare." This proposition implies that the tax on sharing should be confiscatory—all of the economies of scale from larger household size should inure to the benefit of the government. It is little different from the proposition that taxes on labor should be confiscatory---"…transfer programs that do not create incentives for program participants to change their hours of work to increase their level of welfare."

Of course, if sharing (or working) carries with it external or paternalistic costs that are very large, then the Pendakur proposition might be a good guide to policy—but that is an empirical question, not an a priori one.

## B. The equity argument

An alternative argument for the sharing tax is about equity, not efficiency. The argument is something like this: people sharing housing must be better off than people who are not sharing housing, even if they have the same income, and so anyone concerned with equity should want to help those living alone more than those who are sharing.

This argument invokes interpersonal comparisons of utility in ways that are much stronger than those that modern economists are used to. It is no doubt easy to construct stories about household arrangements in which people share because they have been blessed by otherwise unobservable good luck: they met the right person, they were born with a pleasing personality, they can endure waits for the bathroom and another person's dirty socks with equanimity. If these were the only explanations for why people shared, the equity argument might have some bite.

But it is just as easy to construct stories in which people share because they have been cursed with otherwise unobservable bad luck: they lack the independence or the mechanical dexterity to live alone, their work and social life is so barren that they must seek companionship at home, they are allergic to dogs and cats and so cannot live with superior companions, they never acquired the skill to play the bagpipes or understood the pleasure of reading long novels, they have siblings or parents who need someone to look after them.

People's decisions depend on the difference between the utility they attain by sharing and the utility they attain by living alone, not the absolute level of either, and those decisions are all that we—or governments—can observe. The decisions tell us nothing about attained utility. Some people share because the utility from sharing is high, and others share because the utility

from living alone is low. The same can be said about practically any binary choice that consumers make—whether to buy a compact or an SUV, whether to live in the central city or in a suburb, whether to listen to rap music or classical. Taxing people differently based on unobserved utility parameters is very tricky business.

## 2. Literature review

Four different strands of literature have some relevance to policy questions we are asking. Several of these strands have more apparent than actual relevance. We begin with the more relevant strands.

### A. Policy proposals to reduce the tax on sharing

Several writers have advanced proposals to reduce the tax on sharing by providing assistance to people who are doubled up and to their hosts. Rossi (1989) proposed a program of "Aid to Families with Dependent Adults" to help defray the expenses that hosts incur. Shinn et al. (1991) found that many host families made great efforts to keep doubled-up families from entering shelter, and sought programs to help the hosts in those efforts. Vacha and Martin (1993) surveyed host families and echoed the recommendations of Shinn et al. Wasson and Hill (1998) cited Vacha and Martin's finding that the main problem for hosts was lack of space, particularly crowding of kitchens and bathrooms, and recommended that policies for doubled up families concentrate on lessening the tensions surrounding sharing an apartment —"restaurant vouchers, child care, conflict resolution assistance, mental therapy, and access to bathroom and/or cooking facilities (p. 339)."

Another group of scholars has called for reform of zoning laws and building codes to remove legal impediments to shared housing. See Ahrentzen (2003) and Ritzdorf (1994).

Subsidized roommate matching services have also received some attention. Web technology and the development of algorithms for two-sided matching should be making these services more effective and attractive. However, many legal issues around the Fair Housing Act are still being litigated, and any subsidies would have to be cognizant of this litigation. (See, for instance, *Fair Housing Council of San Fernando Valley vs. Roommates.com LLC*, no. 05-56916, 04-57173, decided by the Ninth Circuit, April 3, 2008.)

### B. External and paternalistic costs of shared housing

A number of papers have looked at some of these possible costs, especially those affecting children.

The greatest number of such papers concentrate on medical costs, and examine the effects of crowded housing, not shared housing per se. Crowding is not the same as sharing, and so these papers are not relevant to our concerns. Empirically, crowding is correlated with household size, probably because when people's income rises they purchase both less crowding and smaller households (just as people with nicer cars tend to own nicer refrigerators). But conceptually, crowding and household size are distinct. Crowding means a large number of people per cubic foot; large household size means a large number of people per housing unit. A larger household that lives in a larger housing unit is not more crowded. Indeed it is easier to accommodate people without crowding if they live in larger households. For instance, to house two individuals living alone in 1000-square-foot units with 8-foot high ceilings requires 6024 square feet of exterior wall, ceiling and floor. To accommodate the same two individuals in a single housing unit, and give each the same 1000 square feet of space with an 8-foot ceiling requires only 5431 square feet of exterior wall, ceiling, and floor. Larger households reduce crowding more cheaply than smaller households.

Another group of papers identify shared housing—"doubling up"—as a risk factor for homelessness. For instance: Weitzman et al. (1990), Caton et al. (1994, 1995), Bassuk et al. (1997), Shinn et al. (1998), Caton et al. (2000), Dworsky and Piliavin (2000), Lehmann et al. (2007). Sharing housing at one time is correlated with homelessness at a slightly later time. This relationship is usually robust when many other independent variables are added, but these papers do not check for causality.

## C. Responsiveness

The policy implications of this paper are relevant only if moderate changes in policies can affect whether or not people share housing. If everyone is inexorably fated either to share housing or not, then the tax on shared housing creates no distortions, and programs that make it easier to share housing will have no effect on homelessness (or anything else).

But substantial evidence indicates that modest changes in policies and programs can trigger substantial changes in sharing because many people are on the margin between sharing and living alone. Ellen and O'Flaherty (2007) survey many of these papers. (Freeman 2005 is a partial exception to this general finding, but Ellen and O'Flaherty discuss the reasons at length.)

For instance, in a time-series analysis, Murray (1999) finds that public housing reduces household size so considerably that demand for private housing does not fall when more public housing is built. He does not find similar responsiveness for subsidy programs targeted to somewhat better off households. Sinai and Waldfogel (2005) look at cross-sectional data and test for causality; they find that project-based and tenant-based subsidies reduce household size, although by smaller amounts than Murray found. Susin (2005) uses propensity score matching to perform a longitudinal analysis of the effect of subsidy receipt. Receiving federal housing subsidies reduced the number of adults in a household.

More recently, Abt Associates et al. (2006) report on a large controlled experiment in which randomly selected welfare families received section 8 vouchers. The vouchers reduced household size dramatically, and virtually eliminated households where the head lived with parents and siblings. (The control mean household had 0.599 adults other than the respondent, and treatment-on-the-treated effect was to reduce adults in the household by 0.766, which was highly significant. See exhibit 3-10.)

None of these papers, however, focuses on formerly homeless people or mentally ill people.

## D. Expressed preferences

A number of papers report on responses of consumers to hypothetical questions about whether they would like to live alone or share housing, without specifying the consumption of other goods in those two situations. In most cases, most consumers say they would prefer to live alone. For instance, interviews in Abt Associates et al. (2006) find voucher recipients happy to be able to set up households independent of their parents or to leave abusive relationships. Tanzman (1993) surveys many papers that examine expressed preferences among mental health consumers.

These opinion questionnaires are not relevant to our question because respondents were not asked what they would be willing to give up in order to live alone. Even if they had been asked this question, average willingness-to-pay is not a useful statistic. What matters is the distribution of the willingness-to-pay in the neighborhood of the difference between the costs of living alone and the costs of sharing. The responsiveness studies tell us this.

## 3. Methods

### A. Data

ACCESS gathered data from nine randomly selected pairs of sites in nine states. In each pair, the experimental site received funds to promote integration of service systems and to support assertive community treatment for clients; the control site received funds only for assertive community treatment. Each site was responsible for recruiting 100 clients per year in four annual cohorts. The first cohort was recruited between May 1994 and July 1995; the fourth was recruited between May 1997 and July 1998.

Clients were eligible if they were homeless, suffered from severe mental illness, and were not involved in ongoing community treatment. Clients who agreed to participate in the program were evaluated with a comprehensive baseline interview and were re-interviewed three months later and twelve months later.

The client-level evaluation of ACCESS is described in greater detail in Rosenheck et al. (2002) and Randolph et al. (2002).

### B. Variables

**i. Household arrangements—**Our key independent variables are those that describe whether a person is living alone or sharing housing. Unfortunately, ACCESS does not provide direct information about this. Accordingly, we construct several variables to indicate whether individuals are certainly living alone or almost certainly sharing at the 3-month and 12-month follow-ups. At each follow-up, we construct four dummy variables representing individuals who are: a) definitely living alone (DLA), b) pretty definitely sharing a place to live (PDS), c) other-negative, and d) all else. These categories are exhaustive and mutually exclusive. Because our goal was to make the DLA and PDS categories include only people living alone or sharing, respectively, it is likely that the "all else" category contains some people who had been living alone or sharing for fairly long periods of time.

These categories are complex because several different questions contain information about household arrangements, and we want to assign someone to a category only if all information points in the same direction.

"Definitely living alone" at a follow-up point is defined by the following conditions, each of which was met:

1.  To the question about how many days the respondent had been living at various places in the last 60 days, "own place" was recorded at more than 30 days; and

2.  To the question about when the respondent last lived independently, the last date was the interview date, approximately, the habitation was described as "own place," and the number of other people living there was zero; and

3.  To the questions about relationships, all the questions about whether someone lives with the respondent were answered no; and

4.  The interviewer considered the interview reliable.

"Pretty definitely sharing" meant that the following conditions were met:

1.  To the question about how many days the respondent had been living at various places in the last 60 days, "own place" and "someone else's place" sum to 30 days or more; and

2.  The interviewer considered the interview reliable; and

**3.** Either:

    **a.** To the question about when the respondent last lived independently, the last day was the interview date, approximately, and the number of other people living there was greater than zero, and either the place was described as "own place" or "someone else's place," or

    **b.** To the questions about relationships, at least one of the questions about whether some lives with the respondent was answered yes, and condition 2 for DLA was not satisfied.

"Other-negative" meant that out of the previous 60 days, the respondent spent at least 31 days homeless, or at least 31 days institutionalized, or at least 45 days either institutionalized or homeless. "Homeless" means sleeping outdoors (either urban or rural), in an abandoned building or public building, in an automobile, or in a shelter. "Institutions" are halfway houses, residential treatment programs, hospitals, jails, and prisons.

"All else" is the residual category. It includes people who gave inconsistent answers as well as people who lived several places in the months before the interview.

Table 1 shows the distribution of housing arrangements.

This table shows a marked increase over time in the number of respondents in conventional housing, but it also shows a great deal of instability. About half of those who were either DLA or PDS at 3 months were no longer so at 12 months. The large number in the "all-else" category may also indicate instability. Notice also that most of the ACCESS participants who were able to achieve stable conventional housing were living in shared housing.

About three-quarters of participants who were sharing appeared to be sharing with other family members. This was true at both 3-months and 12-months.

**ii. Dependent variables—**Our dependent variables are several measures of well-being that have been previously used in ACCESS studies. The first is a subjective quality-of-life indicator. This is a seven-point scale where higher values indicate greater satisfaction with quality-of-life. The second is a composite mental health score constructed from a series of questions about depression, psychosis, alcohol abuse, and substance abuse. Lower values on this scale indicate better mental health.

In addition to these overall measures, we also look at depression, psychosis, alcohol abuse, and drug abuse separately. For these disorders, we also use measures previously developed and employed. Smaller is better with these measures. The depression score indicates the presence of symptoms of severe depression during the past month, and the psychosis score measures the frequency with which participants have experienced feelings or beliefs indicating symptoms of psychosis during the past month. Both scores omit experiences under the influence of drugs or alcohol. The drug and alcohol abuse measures are scores on addiction severity indexes, each constructed from multiple items on the ACCESS questionnaires.

We also have a measure of social support; for this, higher values are better. This measures the number of family members and friends who would lend the participant $100, give him a ride to an appointment, or provide emotional support.

Finally, we have three measures of safety. The first is a self-report of the number of times the respondent has been a victim of crime in the past two months ("victimization"). Obviously, smaller numbers are better here. We have two subjective measures of safety: global personal

safety ("safe1") and safety where the respondent lives ("safe2"). For both, larger numbers are better.

We also run regressions with the number of days homeless or institutionalized out of the previous sixty as dependent variables.

**iii. Other independent variables—**We run regressions with two sets of controls. The short set includes gender, race, ethnicity (Hispanic or not), and age. The long set contains the short set plus English-speaking ability, years in the city, veteran status, whether the respondent's checks go to a payee, years of education, an indicator of instability in the participant's family of origin, a measure of history of conduct disorder before the age of 15, the natural log of income the past month, the interview observations of psychotic behavior at intake (intob), whether the respondent is now married or was ever married, and whether the respondent has a child or a child present. These variables are all measured at baseline. For 12-month outcomes, we also include the natural log of income at 3 months. We use the short set as well as the long set because the short set allows us to use many more observations.

For some of the regressions where homelessness is the dependent variable, we use indicators of whether the individual previously had a section 8 voucher or was a resident of public housing as control variables.

For all regressions, we include year and site controls. Since ACCESS was designed to use different sites to test different approaches helping formerly homeless people, omitting these controls would lose an important source of variation. Housing markets, too, are likely to be different in the different sites.

## C. Estimating strategy

Our ideal empirical strategy would be to regress outcomes on a weighted sum of past days sharing or living alone, probably with greater weight on more recent days. We do not have the information to carry out this ideal strategy. Our DLA and PDS variables are proxies for these ideal variables: they indicate membership in groups that are highly likely to be living alone or sharing at the time of observation, and that had a considerable amount of time in that status in the recent past. Differences in the coefficients on these variables indicate that sharing affected respondents differently from living alone.

Since our DLA and PDS variables are noisy measures, the coefficients on these variables may suffer from attenuation bias. That is, because our variables do not measure living alone and sharing perfectly, the coefficients on these variables will be biased toward zero, everything else being equal.

**i. Estimation at 3 months—**At the 3-month follow-up, this strategy is straightforward. We regress outcomes on household arrangements, baseline values of the outcome, and four different sets of controls. Thus at 3-months our basic equation is:

$$\text{Outcome at 3months} = \alpha + \beta * \text{outcome at baseline} + \gamma * \text{housing arrangement at 3 months} + \delta * \text{controls} + \varepsilon.$$

For controls, we use either the long or short series. Except for year dummies, all controls are measured at baseline. In all these equations, DLA is the omitted category. Thus the coefficient for sharing is the difference between the effect of sharing and the effect of living alone.

We also estimate an equation for differences between 3-month and baseline outcomes.

At 3-months, the problems of endogeneity with OLS are obvious, since we are measuring the effects of contemporaneous housing arrangements on contemporaneous outcomes. (For some outcomes, like personal safety, endogeneity may be less of a concern than for others, like social support.)

**ii. Estimation at 12-months**—At 12-months, we look only at 3-month household arrangements as an explanatory variable. Thus our OLS estimating equations are:

$$\text{Outcome at 12months} = \alpha + \beta * \text{outcome at 3months} + \gamma * \text{housing arrangement at 3 months} + \delta * \text{controls} + \varepsilon.$$

All controls (except year dummies) are measured at baseline. Thus all right-hand side variables are measured before 12-months (except year dummies, again). (As with 3-months, we also estimate an equation where the difference between 12-month and 3-month outcomes is the dependent variable.)

This specification considerably reduces the problems of endogeneity. We are asking how 3-month housing arrangements affect 12-month outcomes, holding 3-month outcomes constant. If, for instance, better contemporaneous social support leads to more contemporaneous shared housing, and that is the only relationship between social support and sharing, then sharing will show no effect on social support in the estimating equation. Better social support at 12-months, the dependent variable, will be correlated with sharing at 12-months, but sharing at 12-months is not an explanatory variable. If social support at 12-months is correlated with social support at 3-months, then sharing at 3-months will be correlated with social support at 12-months through social support at 3-months—but our equation controls for social support at 3-months. Thus this estimating equation avoids the obvious endogeneity problems.

(We cannot use this specification at 3-months because everyone is homeless at baseline.)

Still, subtle kinds of endogeneity could arise. For instance, if individuals or their families or their caregivers can forecast reductions in psychotic symptomology nine months in advance (beyond standard mean reversion), then families may welcome individuals at 3-months in anticipation of improvement. If forecasts are more accurate than random guesses around mean reversion, then reductions psychotic symptomology will be correlated with sharing.

## 4. Results

### A. Baseline conditions

At intake, those who go on to share housing do not appear to be significantly different in any observable dimension from those who go on to live alone. Table 2 summarizes the data.

Table 2 shows that on most variables people who will live alone have somewhat better observable baseline characteristics than those who will share housing, but the differences are small and always far from significant. The one exception is social support: those who will share housing score higher on this variable than those who will live alone. The difference is still not significant at conventional levels. This result is not surprising, since social support makes it easier for people to find others with whom to share housing.

Although participants who would be living alone at 12 months had lower income at baseline than participants who would be sharing at 12 months, the relationship was reversed for income recorded at 12 months. At 12-months, moreover, participants living alone had higher current income than participants sharing.

### B. OLS results at 3 months

At 3 months, household arrangements appear to make no difference to outcomes. No household arrangement variables in any of the standard regressions were significant. Sharing was associated with more personal safety and social support, higher quality of life, and less psychotic symptomology, but the relationships were not significant. Results are in He et al. 2008. When differences were estimated, the results were much the same, although sharing housing was associated significantly with a greater decrease in psychotic symptomology.

### C. OLS results at 12-months

Table 3 presents the results for the 12-month follow-up. The lagged value of the dependent variable is the value at 3-months.

As with the 3-month follow-up results, no significant coefficient indicates that living alone is associated with better outcomes than sharing, and only for quality of life, victimization, and drug abuse do small, insignificant coefficients suggest that living alone might be better than sharing. In contrast with 3-months, however, sharing is significantly better than living alone for psychosis, when the long set of controls is used. Since the 12-month follow-up includes more people in conventional housing, and because it often reflects more experience with either living alone or sharing, it should not be surprising that the effects are larger and easier to discern.

Why is the effect of sharing on psychosis significant when the long set of controls is used, and insignificant when the short set is used? The variables in the long set of controls but not in the short that are significant in the psychosis equation are English-speaking ability, education, and a dummy for having a representative payee. Holding 3-month psychotic symptomology score constant, those with poor English-speaking ability, low education, and a representative payee have a higher expected psychosis score. Controlling for these variables makes the effect of sharing housing significant.

The picture is essentially the same when we run regressions in differences, instead of holding 3-month outcomes constant on the right-hand side. The effect of sharing on psychotic symptomology, however, falls from statistical significance.

For these results about not statistically significant benefits from living alone to have policy relevance, several other conditions must be met. Essentially, it must have been a reasonable expectation that if benefits existed they would show up in the data we have. The results would not be meaningful if there were little variation in outcomes or changes in outcomes over this period, or if the coefficients had very large standard errors relative to the variation in outcomes, or if twelve months were too short a period to expect to see changes in the outcome variables.

To address the first two issues, table 4 shows the 95 percent confidence intervals on the coefficients of PDS-3 in table 3 (with the long set of controls). The bounds of these confidence intervals are expressed as ratios to the standard error in the change in the outcome variable between 3-months and 12-months. For most of the outcomes (psychosis being the major exception), the 95 percent confidence interval is about a fifth of a standard error on either side of zero. The variation induced by housing arrangements is confidently a small proportion of the variation in outcomes, and it is highly unlikely that living alone produced any noteworthy benefits.

Was our study period to expect to see changes in outcomes? It was clearly long enough for factors other than household arrangements to produce changes. The consensus in psychiatry is that a study period of this length is not too short. ACCESS was designed and funded in order to observe whether system integration could produce variation in these outcomes. The

conclusions of ACCESS on system integration have been published in leading psychiatry journals and widely accepted.

## D. 12-month outcomes: homelessness and institutionalization

At 12-months, we can also look at the effect of previous spells of sharing and living alone on homelessness and institutionalization. (No one is sharing or living alone at baseline, and PDS and DLA by construction preclude contemporaneous homelessness, and so the effect at 12-months of housing arrangements at 3-months is the only one we can measure.) Thus our dependent variables are the number of days homeless in the previous 60 days at the 12-month follow-up, and the number of days institutionalized. Our explanatory variables are household arrangements at 3 months, and the usual sets of controls.

Also included as controls in some specifications are the number of days homeless and institutionalized in the 60 days preceding the 3-month follow-up—essentially the lagged value of the dependent variable. We do not include these in all specifications because they are collinear with some of the household arrangement variables, especially bad-other. For future reference, we want to see how the bad-other outcome at 3-months affects homelessness and institutionalization at 12-months.

Table 5a presents the results. The first column indicates whether or not controls for days institutionalized at 3 months (I3) and days homeless at 3 months (H3) were included. Sharing at 3-months appears to be associated with more homelessness and more days in institutions at 12-months, but the association is not significant. Point estimates suggest that shared housing at 3 months is associated with 1.5 days more homelessness in the 60 days between month 10 and month 12, and about 1.0-1.2 more days of residence in institutional settings. The mean number of days homeless at 12-months is 10.0 with a standard deviation of 20.4; and the mean number of days in institutions is 8.6 with a standard deviation of 18.9. Thus the point estimates in both cases are less than a tenth of a standard deviation.

The regression results are roughly congruent with the unconditional differences in mean days homeless and institutionalized. Participants who were sharing at 3-months had on average 5.4 days homeless and 5.0 days in institutions at the 12-month follow-up; those who were living alone had 4.0 days homeless and 3.7 days in institutions.

In contrast to the results for sharing, participants with "bad-other" housing arrangements at 3-months had many more days of homelessness and of institutional residence. This is most clear when the lagged dependent variables are omitted from the specification, since these are positively correlated with this housing arrangement.

Part of the reason for the association, albeit insignificant, between sharing and homelessness, is that those who live alone are more likely to receive housing subsidies. When we control for receipt of section 8 vouchers and living in public housing at 3 months, the coefficients on sharing fall by about a quarter in all specifications of the homelessness equation. (The coefficients on sharing in the institutionalization equations fall by a somewhat smaller fraction.)

Having a section 8 voucher at 3 months has a large and significant negative impact on the number of days homeless at 12 months (it also reduces institutionalization, but not significantly). The coefficient is about three times as large in absolute value as the coefficient on sharing in the homelessness equations.

The important policy question is how to interpret this correlation. One possibility is that something occurs in the process of sharing housing that disposes people who do it to become

homeless—they might lose cognitive or social skills, for instance, or their medical problems might worsen, or they might pick up bad habits. In other words, the correlation might reflect causality.

Another possibility is that the correlation arises purely from selection: people who happen to be at risk of becoming homeless (because they are poor, for instance) also happen often to share housing when they are not homeless. In this case, there is no reason for policies to discourage shared housing.

Indeed, it is not impossible that a risk-factor in this sense should be encouraged. Consider, by way of analogy, visits to cardiologists. People who visit cardiologists are more likely to die of heart disease than people in the general population who do not visit cardiologists, and so visits to cardiologists are a risk-factor for heart disease deaths. But because the correlation is not causal and arises instead from attempts at treatment, policies that encourage visits to cardiologists may reduce heart disease deaths.

Thus by itself the finding that shared housing is a risk-factor for homelessness tells us nothing about whether shared housing should be discouraged, encouraged, or neither.

Economic theory, however, suggests a selection interpretation of the correlation. Sharing housing and being homeless are both varieties of housing consumption. Consumption of any good at time $t$ is usually a good predictor of consumption of the same good at time $(t+1)$. If you bought a lot of books in 20010, you are likely to buy a lot of books in 2011. There are two chief reasons for this correlation: persistent unobserved tastes (some people like books a lot throughout their lives), and consumption-smoothing (it is better to have a steady consumption of reading materials, rather than too few in some years and too many in others).

Thus those who consume the smallest quantity of housing at time $t$ are most likely to be homeless at time $(t+1)$. Since shared housing represents a small quantity of housing, usually, people who share housing are more likely to be homeless in the future than those who do not, For a rigorous statement of this relationship, see O'Flaherty 2009.

Our data also let us test the causal interpretation of this correlation more directly. If the correlation between current shared housing and future homelessness is causal, then shared housing should worsen some intervening variable that leads to future homelessness—variables like addiction or mental illness or social isolation. We have seen that this is not the case.

Obviously we have not ruled out all possible intervening variables, but if sharing does cause homelessness, the mechanism by which it does so is not obvious.

## 5. Conclusion

Shared housing does not appear to affect its users adversely in most of the dimensions we examined. On the contrary, in some dimensions it appears to help. Sharing at 3 months is associated with less psychotic symptomology at 12 months. The idea that sharing reduces psychotic symptomology has intuitive appeal. Isolation leads people to dwell on their own internal thinking, while having contact with others fosters a focus on the interpersonal socialized world.

For the most part, whether a person lives alone or shares housing seems to make no difference to the outcomes we studied. Of course, because we could not measure perfectly whether someone was living alone or sharing, our results may be biased toward a finding of "no difference." But most of our insignificant results indicated that sharing was associated with

better outcomes than living alone, not the opposite. Attenuation bias may be keeping us from concluding that sharing helps.

Even a finding of no difference on these dimensions, moreover, is support for a policy that does not tax sharing, because the tax on sharing dissuades some people from living in ways that allows more resources to be devoted to other uses. Since deadweight loss is a bad thing, only positive findings of external and paternalistic costs justify the tax on sharing.

The cost advantages of sharing are apparent even with the result most adverse to sharing—that sharing at 3-months raises days homeless (although insignificantly) at 12-months. Use the first line in table 5a to be precise. In this equation, sharing rather than living alone increased days homeless by about 1.5. The same equations indicated that bad-other status at 3-months increased days homeless at 12-months by 7.5 days, a result that was highly significant.

Consider a back-of-the-envelope calculation. Suppose that sharing uses one-third less resources per person than living alone; the empirical findings suggests that this is a conservative view of the cost-effectiveness of sharing. Shift one person from living alone to sharing. This frees up one-third of the resources needed for a person to live alone, which is one-half of the resources needed for a person to share. Take these resources and use them to move a half-person from bad-other to sharing.

Moving the person from living alone to sharing increases expected days homeless by 1.5 (even though the coefficient is insignificant). On the other hand, moving a half person from bad-other to sharing reduces days homeless by

$$.5(7.5 - 1.5)=3.0.$$

The net effect is a decrease of 1.5 in expected days homeless. This calculation is conservative, but it is also crude, and meant only as a rough illustration.

Of course, "moving people" is only an expression we are using a metaphor, not as a policy recommendation. The policies we are considering are reductions in the tax on sharing—ceasing to try to dissuade people who want to share and reap the financial rewards of doing so. The literature review indicates that such policies will almost certainly increase the number of people who share.

The primary non-obvious weakness of this paper is that we have no information about how sharing housing affected the people with whom ACCESS participants lived. But since those people consented to the sharing voluntarily, even if they later regretted doing so, there is no reason to think they suffered huge losses that were not somehow compensated.

This is important because household size is an extremely powerful variable. The American Community Survey reported an average household size of 2.60 persons in 2005, with a 90% confidence interval from 2.59 to 2.61. With 111 million households in the United States, an increase in average household size smaller than .01—smaller than the American Community Survey could detect—would be sufficient to eliminate homelessness. That is why thinking about the tax on sharing is important.

## Acknowledgments

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**Table 1**

Housing Arrangements of ACCESS Respondents

|  | At 3 months | At 12 months | At 3 and 12 months |
|---|---|---|---|
| Definitely living alone | 423 | 1268 | 200 |
| Pretty definitely sharing | 881 | 1513 | 480 |
| Other-negative | 2829 | 1652 |  |
| All-else | 2024 | 1341 |  |
| Total | 6157 | 5774 |  |

**Table 2**

Baseline Characteristics of Respondents By Subsequent Household Arrangements

**A. Baseline characteristics by household arrangement at 3-months**

| Baseline | DLA | PDS | Other-bad | All else |
|---|---|---|---|---|
| Interview obs. of psychotic behavior | 9.37 (7.04) | 8.68 (7.01) | 11.40 (6.52) | 10.70 (7.61) |
| Quality of life | 3.28 (1.69) | 3.12 (1.71) | 3.18 (1.67) | 3.34 (1.76) |
| Mental health score | -.054 (.77) | .126 (.75) | -.009 (.83) | .014 (.81) |
| Depression | 3.15 (1.95) | 3.48 (1.84) | 3.15 (2.00) | 3.20 (1.95) |
| Alcohol abuse | .112 (.182) | .128 (.201) | .146 (.216) | .140 (.207) |
| Drug abuse | .048 (.09) | .067 (.11) | .072 (.12) | .064 (.11) |
| Psychosis | 9.84 (9.15) | 11.20 (9.09) | 11.37 (9.36) | 11.34 (9.29) |
| Personal safety | 4.09 (1.62) | 4.01 (1.62) | 4.01 (1.66) | 4.09 (1.65) |
| Safety where you live | 4.24 (1.72) | 4.23 (1.74) | 4.06 (1.76) | 4.15 (1.78) |
| Victimization | .72 (1.04) | .77 (1.11) | .75 (1.12) | .79 (1.14) |
| Social support | 1.89 (1.97) | 2.42 (2.34) | 1.63 (2.03) | 1.87 (2.18) |
| Ln(income + 1) | 4.64 (2.44) | 4.55 (2.38) | 4.31 (2.46) | 4.48 (2.42) |

B. Baseline characteristics by household arrangements at 12-months

| Baseline | DLA | PDS | Other-bad | All else |
|---|---|---|---|---|
| Interview obs. of psychotic behavior | 9.85 (7.46) | 9.00 (7.19) | 11.96 (8.60) | 11.68 (8.12) |
| Quality of life | 3.18 (1.73) | 3.08 (1.68) | 3.30 (1.73) | 3.46 (1.73) |
| Mental health score | .000 (.79) | .116 (.75) | -.005 (.85) | -.113 (.85) |
| Depression | 3.25 (1.94) | 3.50 (1.83) | 3.09 (2.02) | 2.86 (2.04) |
| Alcohol abuse | .126 (.196) | .136 (.209) | .167 (.232) | .124 (.189) |
| Drug abuse | .052 (.10) | .072 (.12) | .079 (.12) | .062 (.11) |
| Psychosis | 10.58 (9.13) | 11.10 (9.12) | 11.77 (9.46) | 11.13 (9.37) |
| Personal safety | 4.11 (1.58) | 4.02 (1.61) | 4.01 (1.72) | 4.02 (1.68) |
| Safety where you live | 4.18 (1.73) | 4.13 (1.76) | 3.99 (1.79) | 4.23 (1.76) |
| Victimization | .70 (1.05) | .80 (1.16) | .83 (1.18) | .73 (1.07) |
| Social support | 1.86 (2.05) | 2.18 (2.26) | 1.62 (2.04) | 1.80 (2.29) |
| Ln(income + 1) | 4.44 (2.44) | 4.57 (2.34) | 4.40 (2.43) | 4.38 (2.47) |

Standard deviations in parentheses.

**Table 3**

Effect of Shared Housing on Various Outcomes, 12-Month Follow-up with Year and Site Dummies

| Controls | Quality of life (higher is better) Coefficient on PDS-3 | Mental health score (lower is better) Coefficient on PDS-3 |
|---|---|---|
| Long | -0.088 (0.55) | -0.060 (0.79) |
| Short | -0.011 (0.11) | 0.004 (0.08) |

| Controls | Depression (lower is better) Coefficient on PDS-3 | Alcohol abuse (lower is better) Coefficient on PDS-3 |
|---|---|---|
| Long | -0.068 (0.34) | -0.008 (0.51) |
| Short | -0.006 (0.05) | -0.007 (0.80) |

| Controls | Drug abuse (lower is better) Coefficient on PDS-3 | Psychosis (lower is better) Coefficient on PDS-3 |
|---|---|---|
| Long | 0.003 (0.30) | -1.422[*] (2.08) |
| Short | 0.005 (0.94) | -0.230 (0.52) |

| Controls | Social support (higher is better) Coefficient on PDS-3 | Victimization (lower is better) Coefficient on PDS-3 |
|---|---|---|
| Long | 0.050 (0.22) | 0.025 (0.31) |
| Short | 0.093 (0.71) | 0.034 (0.67) |

| Controls | Personal safety (higher is better) Coefficient on PDS-3 | Safety where you live (higher isbetter) Coefficient on PDS-3 |
|---|---|---|
| Long | 0.096 (0.66) | 0.040 (0.25) |
| Short | 0.052 (0.57) | 0.002 (0.02) |

Absolute t-values in parentheses.

[*] denotes significant at 10% level. N varies between 2123 and 2247 for long set of controls; between 4993 and 5330 for short set of controls. Long set of controls includes 3-month income.

**Table 4**

**Confidence Intervals on the Effect of Sharing Relative to Standard Error of the Change in Outcomes**

Confidence intervals from long equations in table 3 divided by the standard error of the change in outcomes between 3- and 12-months

|  | Lower bound | Upper bound |
|---|---|---|
| *Variables where higher is better* | | |
| Quality of life | -.217 | .122 |
| Social support | -.175 | .219 |
| Personal safety | -.105 | .212 |
| Safety where you live | -.138 | .179 |
| *Variables where lower is better* | | |
| Mental health score | -.260 | .111 |
| Depression | -.197 | .139 |
| Alcohol abuse | -.225 | .132 |
| Drug abuse | -.184 | .250 |
| Psychosis | -.356 | -.011 |
| Victimization | -.124 | .170 |

## Table 5

**a: Effects of 3-month housing arrangements on 12-month homelessness and institutionalization All results with year and site dummies**

**Outcome: Days homeless in the previous 60 days**

| Including I3, H3 | Controls | Coefficient on PDS-3 | On bad-other-3 |
|---|---|---|---|
| No | Long | 1.522 (0.85) | 7.481[**] (4.44) |
| Yes | Long | 1.533 (0.86) | 2.188 (0.86) |
| No | Short | 1.961 (1.58) | 8.651[***] (7.88) |
| Yes | Short | 2.103 (1.71) | 1.199 (0.67) |

| Outcome: Days institutionalized in the previous 60 days | | | |
|---|---|---|---|
| Including I3, H3 | Controls | Coefficient on PDS-3 | On bad-other-3 |
| No | Long | 1.136 (0.63) | 5.879[**] (3.44) |
| Yes | Long | 1.032 (0.58) | 3.529 (1.38) |
| No | Short | 1.045 (0.89) | 6.973[***] (6.70) |
| Yes | Short | 0.937 (0.82) | 1.889 (1.13) |

**b: Effects of 3-month housing arrangements on 12-month homelessness and institutionalization All results with year and site dummies and Controls for subsidized housing at 3 months**

"Subsidized housing" means section 8 or public housing Outcome: Days homeless in the previous 60 days

| Including I3, H3 | Controls | Coefficient on PDS-3 | On bad-other-3 |
|---|---|---|---|
| No | Long | 1.167 (0.65) | 6.871[**] (4.05) |
| Yes | Long | 1.153 (0.65) | 1.603 (0.63) |
| No | Short | 1.516 (1.22) | 7.943[***] (7.17) |
| Yes | Short | 1.646 (1.33) | 0.647 (0.36) |

| Outcome: Days institutionalized in the previous 60 days | | | |
|---|---|---|---|
| Including I3, H3 | Controls | Coefficient on PDS-3 | On bad-other-3 |
| No | Long | 0.794 (0.46) | 4.648[**] (2.82) |
| Yes | Long | 0.771 (0.45) | 2.598 (1.06) |
| No | Short | 0.876 (0.74) | 6.495[***] (6.19) |
| Yes | Short | 0.841 (0.73) | 1.741 (1.04) |

[*] Denotes significant at 10% level.

[**] Denotes significant at 5% level.

[***] Denotes significant at 1% level. N= 2247 for long set of controls; 5330 for short set of controls. Long set of controls includes 3-month income.