



Columbia University

*Department of Economics
Discussion Paper Series*

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

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Discussion Paper No.: 0809-04

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May 2008

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This draft, May 22, 2008

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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 discourage 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. Indeed, shared housing is associated with reduced psychotic symptomology and it appears that this relationship is causal over some time frames, although the latter result is not robust. We use data from ACCESS, a 5-year, 18-site demonstration project with over 6,000 formerly homeless individuals as participants.

Acknowledgments

Ingrid Ellen provided advice and wisdom on every step of the writing of this paper. Bill McAllister and Ester Fuchs generously made data available to us. We have benefited from helpful comments from Sherry Glied, Carol Caton, Sue Barrow, Dan Herman, and Bill McAllister. Financial support from the Columbia Center for Homelessness Prevention Studies is gratefully acknowledged.

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. The 2005 American Community Survey found that of adults under 65 in the household population who were not living with spouses, only 23.8% lived alone. 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 (the former standard is based on federal poverty guidelines [Nelson, 1993] and the latter on an econometric study [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 first run OLS regressions to determine whether sharing housing is correlated with subsequent changes in mental health and other outcomes. 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).

OLS cannot shed much light on causality, however. It may be that whatever it is that causes sharing—good social support, for instance—is better for mental health than whatever it is that causes living alone—federal subsidies, for instance. Therefore we use instrumental variables to check whether the result on psychosis is causal. Our instrumental variables are the amounts of competitive federal funding for homelessness programs that states received in the years immediately preceding ACCESS, and the number of Section 8 vouchers the jurisdictions received in 1996 (all numbers normalized by the number of poor people). The instrumental variables results indicate that the impact of shared housing on psychotic symptomology may be causal at 12 months, but not at 3 months. The 12-month result, however, is not robust.

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 discussions

Our results are relevant to 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. It is more convenient to discuss the broader design issues first and at greater length because the design principles we are concerned with are more obvious in those programs.

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.

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. 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 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. In this paper we examine empirically whether sharing has particular fiscal and paternalistic costs among a group of people among whom these costs are especially likely to be present. Thus we examine one particular possible justification for the tax on sharing. It is in that sense that we are making a modest contribution to a literature that should be huge.

Many other empirical claims about why people would share housing too much in the absence of this tax could be made, obviously, and we review below some of the literature on this topic. (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.) Many additional papers can and should be written.

We also 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.

Notice that even if we find no 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. Application to homelessness

Since many programs to prevent and deal with homelessness are tied to SSI, food stamps, or section 8, the immediate application of these ideas is obvious. There are also many other ways that an implicit tax on sharing manifests itself in programs for alleviating homelessness. Partly it shows up in programs that do not place people in shared housing, and in homelessness prevention programs that do not give housing

assistance to people who are doubled up; the tax in this case is the full amount of program benefits. The tax also shows up in how housing counselors steer their clients, and whether clients realize rewards, financial or otherwise, when they choose to share.

Again, redesigning the programs that deal with homelessness is beyond this paper's scope.

2. Literature review

Five 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).

B. External and paternalistic costs of shared housing

Since this paper is looking for external and paternalistic costs of shared housing, we review other papers that look for or find such costs.

The greatest number of such papers concentrate on medical costs, and examine the effects of crowded housing, not shared housing per se. Children in crowded conditions are more susceptible to respiratory ailments (Mann et al. 1992), stomach illnesses (Galpin et al. 1992, Mendall and Northfield 1995), and cerebro-spinal diseases (Pierce et al 1995). Children brought up in crowded conditions die younger (Coggon et al. 1993). There is also some evidence of greater morbidity among adults (Mann et al. 1992, Mendall and Northfield 1995).

Crowding also affects development and concentration, especially in children and young adults. See Shapiro (1974), Rodin (1976), Booth and Edwards (1976), Booth and Johnson (1975), Karlin et al. (1979), Nagar (1998) and Epstein (1981). For a judicious review of literature on health and shared housing, see Ahrentzen (2003).

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), 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. Persistent unobserved characteristics (small demand for privacy, for instance, or a tight housing market) could be contributing to both being doubled up and being homeless. Most of these papers use samples drawn from some general population, and so any unobserved characteristics associated with both doubling up and homelessness will be relatively rare. Our sample consists entirely of formerly homeless people, and so the unobserved characteristics should be much more prevalent. If persistent unobserved characteristics are responsible for the correlation between doubling up and homelessness, then we should expect a much weaker correlation in our sample than in the samples in which risk factors are normally studied.

Dworsky and Piliavin (2000) look at the probability of re-entering homelessness

for a sample of formerly homeless people. In that regard, this paper is like ours. They find that those who doubled up as their first housing arrangement after homelessness were more likely than those who lived in a house to which they had legal title were more likely to re-enter homelessness, holding many other characteristics constant. There are several important differences between their paper and ours. Their sample is much smaller and geographically homogeneous, and their set of covariates is much less rich. Our sample is less heterogeneous in that all members are mentally ill as well as formerly homeless. They also define housing arrangements differently, with more attention to legal distinctions than to physical ones. Thus they apparently do not count people who share housing but are primary tenants as doubled up, and they even make a distinction between those who live in apartments and those who live alone in SROs.

None of these papers attempts to assess the cost of living alone, so that we could tell whether homelessness could be reduced more for the same expenditure if some people doubled up.

We would like to test for causality, but we have not been able to find instrumental variables that would let us do so. We provide a rough cost-benefit calculation in the conclusion.

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) used 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.

By way of analogy: If asked whether they would prefer a job where they had to work 6 hours a day to the same job where they had to work 8 hours a day and received the same pay, most people would probably choose the 6-hour job. But no one would refer to such “preferences” as an argument for confiscatory tax rates on earned income.

E. Evolving consumer households

Caplan et al. (2006) compared formerly homeless mentally ill people living in group homes with those living in independent apartments. Socially isolated housing was

associated with weakened executive functioning. While this result suggests one way that sharing might help consumers, aside from saving resources, it is not a clean test of shared housing. In the Caplan et al. study, people living in group homes also received more professional support, and so it is not possible to tell whether group living or professional support was responsible for the better executive functioning. In general, the bundling of professional support and residential arrangements that occurs in group homes is the reason why this paper does not try to say anything about them.

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 reported in this paper (except the instrumental variable estimates), 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.

iv. Instrumental variables

Our instrumental variables are the amounts of discretionary Continuum of Care funds that states received from the federal Department of Housing and Urban Development (HUD) in fiscal years 1993, 1994, and 1995; and the number of section 8 vouchers in the relevant city or county in 1996. The former data are available in Barnard-Columbia Center for Urban Policy (1996, table A-2); the latter are available from the Picture of Subsidized Housing in HUDUSER. We normalize by dividing by the simple average of 1990 and 2000 poor populations, and take the natural logarithms of the

resulting ratios. (Poor population in 1990 and poor population in 2000 are strongly correlated, with R^2 above .99 on both city and state levels.) We have also normalized by dividing by population; the results are similar and we do not report them.

For the first set of instrumental variables, HUD made competitive grants under several different categorical programs in these years in attempting to implement the Continuum of Care for homeless persons. The programs involved were Supportive Housing (the largest source of funds), Shelter Plus Care, Section 8 SRO, and Innovative programs (a small amount). Since the majority of these funds were targeted at encouraging formerly homeless people to live alone, we would expect more funding to increase the proportion of people living alone. We expect the funds to be spent for several years after they were awarded. We do not expect these funds to affect directly the mental health of ACCESS participants; that is our exclusion restriction.

As discussed in the policy section and shown in the literature review, section 8 vouchers reduce the relative price of living alone and encourage smaller households, among those who receive them. To the extent that the availability of section 8 vouchers leads to more ACCESS participants securing these vouchers, ACCESS participants would be less likely to share housing (and less likely to be homeless or experience other bad outcomes). On the other hand, Susin (2002) has argued that section 8 vouchers increase the rents that non-recipient poor people pay and provided some empirical support for this position. If this position is correct, and most ACCESS participants do not receive section 8 vouchers, then more section 8 vouchers should raise rents that most ACCESS participants face, and increase the proportion who share housing (and the proportion who are homeless or experience other bad outcomes).

Since our instrumental variables are site-specific, we cannot use site dummies in these equations. Hence we use a dummy for those participants in treatment sites in these equations.

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:

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

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.

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:

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

All controls (except year dummies) are measured at baseline. Thus all right-hand side variables are measured before 12-months (except year dummies, again).

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.)

iii. Instrumental variables estimation

Because our OLS results cannot rule out all forms of reverse causality, we use instrumental variables where possible to see which relationships are likely to be causal.

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.)

At 12-months, as we have noted, using only lagged housing arrangements reduces considerably the endogeneity problem. 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.

We also want to use instrumental variables at 12-months to find out whether contemporaneous housing arrangements are having any deleterious effects.

Our first-stage equations are multinomial logit regressions for the odds of each household arrangement, relative to DLA, at 3-months and at 12-months. The asymptotic properties of IV estimators apply only when the first-stage is somewhat stronger than ours. Accordingly, we will use IV estimates only to look at results where OLS was significant; we will not check whether coefficients that were insignificant in OLS become significant.

At 3-months, our first-stage equation will be the same as the OLS regression, except that we use the estimated probabilities of each household arrangement at 3 months as our independent variables instead of dummy variables. Thus we estimate the effect of current household arrangements on current outcomes.

At 12 months, we run two kinds of regressions. First, we repeat our OLS specification for 12 months, where we include 3-month but not 12-month housing arrangements, but with 3-month housing arrangements estimated. Then we include both 3-month and 12-month housing arrangements, with only 12-month housing arrangements estimated:

$$\text{Outcome at 12-months} = \alpha + \beta * \text{outcome at 3-months} + \gamma * \text{actual housing arrangement at 3 months} + \delta * \text{estimated probabilities of housing arrangements at 12 months} + \phi * \text{controls} + \varepsilon.$$

Thus we will see how both current and lagged housing arrangements affect current outcomes.

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. Table 3 shows the result. For each outcome, this table shows the coefficient on sharing.

The outstanding feature of Table 3 is that nothing is significant. There is no support in this table for the hypothesis that sharing housing has adverse effects—indeed, most of the insignificant effects are weakly positive. The table suggests that sharing is associated with more personal safety and social support, higher quality of life, and less psychotic symptomology. We have also run these regressions without year and site dummies; in no case is sharing significantly associated with worse outcomes.

C. OLS results at 12-months

Table 4 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.

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 5 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.

It would be useful to look further into the effects of sharing on homelessness using an IV strategy, but we do not have a good instrumental variable for doing so. The instrumental variables we use cannot support inferences about housing market outcomes like homelessness.

E. Instrumental variables results

First stage results. Our first-stage equations are multinomial logit regressions for the odds of each household arrangement, relative to DLA, at 3-months and at 12-months. These are presented in the appendix. Our greatest interest is in the equations for the PDS

household arrangement. The coefficients on fiscal year 1993 funding level is negative and significant, while the coefficient on the other years' funding levels is sometimes positive, sometimes insignificant and always smaller in absolute value—people in states with more funding are less likely to share housing, relative to living alone. The effect is weaker in the 12-month equations, but since 3-month household arrangements are included as controls in these equations, the weaker effect of funding should not be surprising. Vouchers appear to decrease rather than increase the propensity of people to live alone: the coefficients on vouchers in the equations for all three other household arrangements are positive.

When we run regressions without vouchers, all three funding levels decrease sharing, and when we run regressions without funding levels, the effect of vouchers is often negative. Similarly, when we drop one or more of the funding level variables, the remaining variables become more negative. Thus with vouchers the market effect seems slightly stronger with this group than the direct subsidy effect, and because vouchers are positively correlated with funding levels, the market effect predominates when funding levels are held constant.

Second-stage results. At 3-months, sharing has no significant effect on psychosis; indeed the point estimate is positive. Interestingly, though, sharing significantly reduces depression in the 3-month IV equation. But since the OLS result on depression was not significant, this conclusion does not seem to be robust. Most importantly, there is no IV evidence (or OLS evidence) that sharing adversely affects participants.

Second-stage instrumental variable results for psychosis at 12 months are shown in Table 6. At 12-months, we begin with the same specification we used with OLS.

Because household arrangements are lagged nine months and 3-month outcomes are included as controls, the only reverse causality fear is that families and friends use information unobserved at baseline to anticipate improvements, and select the ACCESS participants more likely to improve to share housing with. When we instrument for 3-month household arrangements, we answer this objection. The coefficient on sharing in the psychosis equation remains negative and significant, and in fact becomes larger: -16.287 (2.02) with the long series of controls and no year or site dummies. However, when we include 3-month income as a control, the coefficient slips below significance (results not shown).

With instrumental variables, we can also enter household arrangements at 12-months as an explanatory variable, provided we instrument for them, along with 3-months housing arrangements. With this specification and the long set of controls, both sharing at 3 months and sharing at 12 months have negative and insignificant coefficients. With the short set of controls, sharing at 3 months has a positive and significant effect on psychosis, while sharing at 12 months has a negative and significant effect, much larger in absolute value.

Because sharing at 3 months is positively correlated with sharing at 12 months, we looked at the sum of coefficients on PDS-3 and PDS-12—what happens to a person who shares at both follow-ups. The point estimate is negative for both sets of controls, but is significant only with the short set.

Our results thus suggest that the effect of sharing on psychosis is causal, but they are not robust.

Sharing had no significant effects on other outcomes. (We also tested for whether

the sum of the coefficients on sharing at 3 months and sharing at 12 months was equal to zero, and we could not reject it for any outcome except psychosis.) We also ran these regressions omitting various funding levels from the first-stage regression, and there were no major differences. Similarly we ran these regressions normalizing funding levels and vouchers by total population instead of poor population; again the major results remained the same.

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, and some of our results suggest that this relationship is causal, although this conclusion is not robust. 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 5 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; this is in the upper range of the empirical findings. 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 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.

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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)

Standard deviations in parentheses.

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,
3-Month Follow-up with Year and Site Dummies**

Outcome: Quality of life (higher is better)

Controls	Coefficient on PDS
Short	0.107 (1.14)
Long	0.107 (.73)

Outcome: Mental health score (lower is better)

Controls	Coefficient on PDS
Short	-0.046 (1.05)
Long	0.024 (.35)

Outcome: Depression (lower is better)

Controls	Coefficient on PDS
Short	0.033 (.27)
Long	0.074 (.38)

Outcome: Alcohol abuse (lower is better)

Controls	Coefficient on PDS
Short	-0.011 (1.20)
Long	0.004 (.27)

Outcome: Drug abuse (lower is better)

Controls	Coefficient on PDS
Short	-0.002 (.53)
Long	0.003 (.42)

Outcome: Psychosis (lower is better)

Controls	Coefficient on PDS
Short	-0.703 (1.70)
Long	-0.122 (.19)

Outcome: Social support (higher is better)

Controls	Coefficient on PDS
Short	0.107 (1.14)
Long	0.107 (.73)

Outcome: Victimization (lower is better)

Controls	Coefficient on PDS
Short	-0.052 (.96)
Long	-0.039 (.43)

Outcome: Personal safety (higher is better)

Controls	Coefficient on PDS
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Short	0.078 (.87)
Long	0.108 (.77)

Outcome: Safety where you live (higher is better)

Controls	Coefficient on PDS
Short	0.019 (0.20)
Long	0.223 (1.50)

Absolute t-values in parentheses. * denotes significant at 10% level.

N varies between 5749 and 6151 with the short set of controls; between 2481 and 2613 with the long set of controls.

Table 4: Effect of Shared Housing on Various Outcomes, 12-Month Follow-up with Year and Site Dummies

Controls	Quality of life (higher is better)	Mental health score (lower is better)
	Coefficient on PDS-3	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)	Alcohol abuse (lower is better)
	Coefficient on PDS-3	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)	Psychosis (lower is better)
	Coefficient on PDS-3	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)	Victimization (lower is better)
	Coefficient on PDS-3	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)	Safety where you live (higher is better)
	Coefficient on PDS-3	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 5a: 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)

* 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.

Table 5b: 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.

**Table 6: Effect of Shared Housing on Psychotic Symptomology
12-month follow-up with Treatment Site Dummy
Instrumental Variable Results**

A. With long set of controls		
	No 12-month housing arrangement	With 12-month housing arrangement
Coeff of PDS-3	-16.287* (2.02)	-0.879 (0.54)
Coeff of PDS-12	na	-1.999 (0.46)
Sum of coefficients	na	-2.878 p=.335
B. With short set of controls		
	No 12-month housing arrangement	With 12-month housing arrangement
Coeff of PDS-3	-2.534 (0.46)	3.141* (2.39)
Coeff of PDS-12	na	-8.295** (2.78)
Sum of coefficients	na	-5.154 p=.046

*Significant at 10% level. **Significant at 5% level. N= 2248 for long set of controls; 5322 for short set of controls. P-value for sum of coefficients is for the null-hypothesis that the sum of coefficients is zero. 3-month income not included in long set of controls.

Appendix 1: First-Stage of IV Regression
3-Months Living Arrangement
Multinomial Logit Regression with Short Set of Controls
Omitted Group = DLA (Definitely Living Along)

	<u>Regression 1</u>			<u>Regression 2</u>		
	PDS	Other- bad	All else	PDS	Other-bad	All else
Treatment Site Dummies	0.149 (1.16)	0.202 (1.82)	-0.116 (1.02)	na	na	na
Baseline psychosis	0.016 (2.36)*	0.016 (2.68)**	0.021 (3.31)**	0.017 (2.48)*	0.018 (2.88)**	0.020 (3.24)**
vouchers	1.002 (4.56)**	0.183 (0.94)	-0.565 (2.85)**	0.914 (4.42)**	0.098 (0.53)	-0.549 (2.90)**
f1993	-0.777 (5.17)**	-0.844 (6.08)**	-0.436 (3.04)**	-0.746 (5.09)**	-0.805 (5.97)**	-0.451 (3.24)**
f1994	0.226 (1.29)	0.374 (2.40)*	0.067 (0.42)	0.209 (1.20)	0.355 (2.29)*	0.070 (0.44)
f1995	0.102 (1.25)	0.584 (7.96)**	0.528 (7.08)**	0.106 (1.30)	0.581 (7.91)**	0.538 (7.19)**
male	-0.805 (6.53)**	0.297 (2.71)**	-0.032 (0.29)	-0.808 (6.55)**	0.293 (2.68)**	-0.029 (0.26)
black	0.075 (0.58)	0.073 (0.64)	-0.088 (0.76)	0.075 (0.57)	0.071 (0.62)	-0.091 (0.78)
hispanic	-0.080 (0.29)	-0.266 (1.10)	-0.365 (1.47)	-0.083 (0.30)	-0.268 (1.11)	-0.365 (1.47)
age	-0.043 (6.68)	-0.002 (0.31)	-0.013 (2.21)*	-0.043 (6.71)**	-0.002 (0.36)	-0.012 (2.17)*
Constant	-0.007 (0.01)	0.469 (0.66)	3.597 (4.94)**	0.339 (0.44)	0.836 (1.25)	3.484 (5.06)**
Observations	6145			6145		

Absolute value of z statistics in parentheses

* significant at 5%; ** significant at 1%

The table reports Multinomial (polytomous) logistic regression results. The coefficients should be interpreted as the effect of a variable on one housing arrangement (PDS, other-bad or all else) relative to the reference group, DLA.

Appendix 2: First-Stage of IV Regression
3-Months Living Arrangement
Multinomial Logit Regression with Long Set of Controls
Omitted Group = DLA (Definitely Living Along)

	Regression 1			Regression 2		
	PDS	Other-bad	All else	PDS	Other-bad	All else
Treatment Site Dummies	0.004 (0.02)	0.268 (1.42)	-0.178 (0.92)	na	na	na
Baseline psychosis	0.006 (0.55)	0.007 (0.63)	0.009 (0.85)	0.006 (0.55)	0.008 (0.74)	0.008 (0.77)
voucher	1.085 (3.10)**	0.373 (1.14)	-0.146 (0.44)	1.090 (3.32)**	0.261 (0.84)	-0.080 (0.25)
f1993	-0.658 (2.77)**	-0.846 (3.74)**	-0.491 (2.12)*	-0.654 (2.82)**	-0.803 (3.63)**	-0.522 (2.32)**
f1994	0.384 (1.36)	0.469 (1.77)	0.464 (1.71)	0.380 (1.35)	0.448 (1.69)	0.478 (1.77)
f1995	0.027 (0.21)	0.544 (4.46)**	0.278 (2.30)*	0.027 (0.21)	0.543 (4.46)**	0.283 (2.33)**
male	-0.213 (0.94)	0.466 (2.21)*	0.304 (1.42)	-0.211 (0.93)	0.470 (2.23)**	0.303 (1.42)
black	0.241 (1.11)	0.200 (0.98)	0.111 (0.54)	0.240 (1.10)	0.201 (0.99)	0.110 (0.53)
hispanic	-0.377 (0.74)	-0.361 (0.76)	-0.303 (0.63)	-0.383 (0.75)	-0.384 (0.81)	-0.292 (0.61)
age	-0.067 (4.57)**	-0.038 (2.77)**	-0.052 (3.79)**	-0.067 (4.56)**	-0.038 (2.81)**	-0.052 (3.76)**
english	-0.320 (0.66)	-0.241 (0.53)	-0.375 (0.82)	-0.324 (0.67)	-0.282 (0.62)	-0.348 (0.76)
yearcity	0.016 (2.39)*	0.016 (2.63)**	0.016 (2.56)**	0.016 (2.40)**	0.017 (2.75)**	0.016 (2.49)**
veteran	-0.287 (1.04)	-0.005 (0.02)	-0.268 (1.06)	-0.284 (1.03)	0.004 (0.02)	-0.277 (1.10)
payee	-0.118 (0.36)	0.021 (0.07)	0.127 (0.42)	-0.118 (0.36)	0.002 (0.01)	0.140 (0.46)
edu	-0.004 (0.10)	-0.041 (1.03)	-0.005 (0.12)	-0.004 (0.10)	-0.042 (1.08)	-0.003 (0.09)
finstab	-0.020 (0.46)	-0.031 (0.76)	0.010 (0.25)	-0.020 (0.46)	-0.030 (0.73)	0.010 (0.23)
sociop	0.017 (0.35)	0.031 (0.68)	-0.008 (0.17)	0.017 (0.35)	0.030 (0.66)	-0.007 (0.16)
lninc	0.055 (1.35)	0.018 (0.48)	0.037 (0.96)	0.054 (1.35)	0.019 (0.50)	0.036 (0.95)
incarc	-0.013 (0.06)	0.172 (0.83)	0.091 (0.43)	-0.011 (0.05)	0.163 (0.78)	0.100 (0.48)
intob	-0.001 (0.06)	0.015 (1.21)	0.012 (0.96)	-0.001 (0.06)	0.016 (1.24)	0.012 (0.92)
nowmarried	1.189 (2.79)**	0.341 (0.81)	0.237 (0.55)	1.187 (2.78)**	0.318 (0.76)	0.252 (0.59)
evermarried	0.257 (1.11)	0.009 (0.04)	0.121 (0.55)	0.257 (1.11)	0.016 (0.07)	0.116 (0.53)
child	-0.146 (2.68)**	-0.059 (1.37)	-0.056 (1.24)	-0.145 (2.67)**	-0.059 (1.36)	-0.055 (1.23)

childwithu	0.623 (4.23)**	0.258 (1.74)	0.177 (1.17)	0.623 (4.23)**	0.261 (1.76)	0.173 (1.15)
Constant	0.102 (0.06)	1.215 (0.83)	3.097 (2.08)*	0.085 (0.06)	1.756 (1.27)	2.764 (1.96)*
Observations	2612			2612		

Absolute value of z statistics in parentheses

* significant at 5%; ** significant at 1%

The table reports Multinomial (polytomous) logistic regression results. The coefficients should be interpreted as the effect of a variable on one housing arrangement (PDS, other-bad or all else) relative to the reference group, DLA.

Appendix 3: First-Stage of IV Regression
12-Months Living Arrangement
Multinomial Logit Regression with Short Set of Controls
Omitted Group = DLA (Definitely Living Along)

	<u>Regression 1</u>			<u>Regression 2</u>		
	PDS	Other-bad	All else	PDS	Other-bad	All else
Treatment Site Dummies	0.230 (2.57)**	-0.123 (1.45)	-0.146 (1.70)	na	na	na
3-month psychosis	0.001 (0.21)	0.026 (5.27)**	0.017 (3.35)**	0.001 (0.21)	0.026 (5.25)**	0.017 (3.32)**
voucher	0.156 (1.09)	-0.515 (3.78)**	-1.364 (9.54)**	0.040 (0.30)	-0.484 (3.66)**	-1.329 (9.55)**
f1993	-0.361 (3.71)**	-0.503 (4.99)**	-0.059 (0.55)	-0.329 (3.41)**	-0.519 (5.21)**	-0.086 (0.81)
f1994	0.132 (1.07)	0.495 (3.97)**	0.158 (1.22)	0.110 (0.89)	0.507 (4.08)**	0.172 (1.34)
f1995	0.040 (0.64)	0.435 (6.05)**	0.415 (6.11)**	0.043 (0.69)	0.439 (6.09)**	0.428 (6.30)**
pds_3m	2.714 (14.10)**	1.952 (8.46)**	0.982 (4.64)**	2.718 (14.13)**	1.947 (8.44)**	0.979 (4.63)**
other_3m	1.048 (6.36)	1.142 (5.96)**	1.044 (6.85)**	1.043 (6.34)**	1.145 (5.98)**	1.051 (6.89)**
badother_3m	1.042 (6.33)**	2.220 (11.97)**	0.947 (6.20)**	1.057 (6.43)**	2.213 (11.93)**	0.942 (6.17)**
male	-0.558 (6.48)**	0.431 (4.88)**	0.037 (0.43)	-0.560 (6.50)	0.431 (4.87)**	0.038 (0.44)
black	0.176 (1.97)*	0.229 (2.65)**	0.114 (1.30)	0.178 (1.99)*	0.230 (2.65)**	0.111 (1.26)
hispanic	0.237 (1.08)	0.115 (0.55)	0.055 (0.26)	0.229 (1.05)	0.120 (0.57)	0.058 (0.27)
age	-0.039 (8.41)**	-0.015 (3.47)**	-0.008 (1.86)	-0.039 (8.48)**	-0.015 (3.43)**	-0.008 (1.79)
Constant	0.382 (0.66)	-0.804 (1.44)	2.860 (5.10)**	0.902 (1.67)	-0.987 (1.84)	2.648 (4.92)**
Observations	5325			5325		

Absolute value of z statistics in parentheses

* significant at 5%; ** significant at 1%

The table reports Multinomial (polytomous) logistic regression results. The coefficients should be interpreted as the effect of a variable on one housing arrangement (PDS, other-bad or all else) relative to the reference group, DLA.

Appendix 4: First-Stage of IV Regression
12-Months Living Arrangement
Multinomial Logit Regression with Long Set of Controls
Omitted Group = DLA (Definitely Living Along)

	<u>Regression 1</u>			<u>Regression 2</u>		
	PDS	Other-bad	All else	PDS	Other-bad	All else
Treatment Site Dummies	0.389 (2.92)**	0.140 (1.01)	0.100 (0.69)	na	na	na
3-month psychosis	-0.012 (1.47)	0.027 (3.42)**	0.010 (1.20)	-0.012 (1.48)	0.027 (3.43)**	0.010 (1.20)
voucher	0.220 (1.02)	-0.463 (2.10)*	-1.212 (5.02)**	0.005 (0.02)	-0.558 (2.62)**	-1.299 (5.53)**
f1993	-0.435 (2.99)**	-0.697 (4.36)**	-0.094 (0.53)	-0.374 (2.63)**	-0.665 (4.24)**	-0.074 (0.43)
f1994	0.292 (1.57)	0.819 (3.95)**	0.330 (1.52)	0.248 (1.34)	0.794 (3.86)**	0.315 (1.46)
f1995	0.063 (0.70)	0.426 (4.11)**	0.393 (3.69)**	0.074 (0.82)	0.432 (4.15)**	0.404 (3.78)**
male	-0.464 (3.19)**	0.322 (2.11)*	0.014 (0.09)	-0.463 (3.19)**	0.321 (2.11)*	0.013 (0.08)
black	0.248 (1.79)	0.236 (1.61)	0.199 (1.28)	0.257 (1.86)	0.240 (1.64)	0.201 (1.30)
hispanic	0.354 (1.00)	0.600 (1.69)	0.385 (1.00)	0.336 (0.95)	0.593 (1.66)	0.383 (0.99)
age	-0.048 (4.92)**	-0.023 (2.32)*	-0.026 (2.46)*	-0.048 (4.98)**	-0.024 (2.32)*	-0.026 (2.45)**
english	0.288 (0.98)	0.346 (1.15)	0.300 (0.93)	0.243 (0.83)	0.331 (1.10)	0.293 (0.91)
yearcity	-0.001 (0.24)	0.003 (0.67)	0.001 (0.30)	0.000 (0.02)	0.003 (0.75)	0.002 (0.34)
veteran	-0.100 (0.55)	0.145 (0.82)	-0.266 (1.35)	-0.082 (0.46)	0.154 (0.87)	-0.261 (1.32)
payee	-0.110 (0.54)	-0.025 (0.12)	0.171 (0.81)	-0.137 (0.67)	-0.035 (0.17)	0.166 (0.78)
edu	0.028 (0.99)	-0.019 (0.66)	0.019 (0.63)	0.026 (0.90)	-0.020 (0.70)	0.019 (0.62)
finstab	0.008 (0.29)	0.001 (0.03)	-0.017 (0.54)	0.010 (0.37)	0.002 (0.06)	-0.016 (0.51)
sociop	-0.014 (0.45)	-0.016 (0.51)	-0.010 (0.28)	-0.014 (0.44)	-0.016 (0.51)	-0.010 (0.29)
lninc	0.012 (0.45)	-0.029 (1.01)	-0.038 (1.26)	0.011 (0.42)	-0.030 (1.03)	-0.039 (1.29)
incarc	0.031 (0.21)	0.369 (2.31)*	0.253 (1.55)	0.022 (0.15)	0.367 (2.30)*	0.251 (1.53)
intob	-0.016 (1.80)	0.008 (0.93)	0.001 (0.09)	-0.015 (1.77)	0.008 (0.96)	0.001 (0.07)
nowmarried	0.748 (2.52)*	0.127 (0.40)	0.562 (1.73)	0.729 (2.46)*	0.120 (0.38)	0.557 (1.72)
evermarried	-0.102 (0.68)	-0.307 (1.98)*	-0.020 (0.12)	-0.082 (0.56)	-0.299 (1.94)	-0.015 (0.09)
child	-0.016 (0.40)	0.041 (1.02)	0.033 (0.77)	-0.017 (0.43)	0.041 (1.03)	0.033 (0.79)

childwithu	0.733 (6.30)**	0.255 (1.88)	0.144 (1.00)	0.730 (6.29)**	0.253 (1.87)	0.141 (0.98)
Constant	1.113 (1.12)	0.424 (0.41)	2.990 (2.76)**	2.101 (2.24)*	0.823 (0.84)	3.322 (3.18)*
Observations	2425			2425		

Absolute value of z statistics in parentheses

* significant at 5%; ** significant at 1%

The table reports Multinomial (polytomous) logistic regression results. The coefficients should be interpreted as the effect of a variable on one housing arrangement (PDS, other-bad or all else) relative to the reference group, DLA.

