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Shroder, Mark

US Department of Housing and Urban Development, Office  
of Policy Development and Research

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# Housing Subsidies and Work Incentives\*<sup>1</sup>

Mark Shroder, HUD-PD&R, Washington, DC, USA

## Abstract

Low-income housing assistance is part of the welfare state of all developed countries. The rest of the welfare state may cause work disincentives. In theory, housing assistance may also do so, but those disincentives may be blunted by its in-kind character and the way it is rationed. Rationing and selection make the estimation difficult; the most rigorous evidence from the United States suggests a loss of 10 to 20 cents in earnings per dollar of assistance. Less rigorous evidence from Australia suggests negative impacts in public housing but not housing benefit, while in Scandinavia researchers have as yet found no long-term duration of dependency.

## Introduction

In all developed countries, housing assistance is a part of the larger structure of the welfare state, some parts of which reduce labor supply. In this article we discuss whether housing assistance does so as well. Arthur Okun introduced the metaphor of the leaky bucket to describe the common situation in which a dollar taken from the rich delivers less than a dollar's worth of benefit to the poor. One of Okun's four basic leaks is from changes in work effort induced by redistribution. Gary Burtless, reviewing a series of negative income tax (NIT) experiments, found that the implementation of the NIT in the United States would have caused the government to spend almost \$2 in order to increase family incomes by \$1, mostly due to higher transfers inducing reductions in labor supply among the assisted.

Fair assessment of program effects is difficult. Housing assistance programs usually target the most disadvantaged families in society. They were selected for assistance because they needed help. It is easy to confuse the effect of the selection with the effect of the program. We first consider what economic theory predicts, then the methodological barriers to investigation, and finally the evidence relating to labor supply consequences of housing assistance programs.

## Theory: A Neoclassical Hypothesis

In general, a housing assistance program fills the gap between the cost of decent housing and the amount the state expects a low-income household to contribute to its own shelter. In general, the amount of assistance is the higher of zero or

$$A = G - T(Y) \quad (1)$$

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where  $G$  is the amount the state will pay for a household with no income at all,  $Y$  is the household's actual income, and  $T(Y)$  is the contribution function, which usually depends on  $Y$ .

The government typically sets  $G$  in response to housing market conditions and family circumstances, such as number of members in the household. The amount by which  $T(Y)$  rises with a small increase in  $Y$  is the tax rate, or taper – the rate at which housing subsidies fall as income rises. In the United States,  $T(Y)$  is a flat tax, as the tenant is expected to pay 30 cents of every dollar of income towards rent. In many countries the tax rate is graduated. For example, for UK households with less than a minimum income (more or less the basic social welfare payment), the housing program has a tax rate of zero, but above that level benefit is withdrawn at the rate of 65 cents for each additional pound. The French housing tax is a marvel of complexity, varying between 0 and 37 cents on the marginal euro according to a five-factor formula. Most developed countries list the specific features of their programs in the OECD (2007).

Many economists and not a few non-economists instinctively suspect that housing assistance receipt must reduce labor supply. Here we formalize that instinct; let the positive predictions that emerge from the following analysis form the 'neoclassical hypothesis'. Suppose an assisted housing tenant is able to work. Her willingness to sell her time to an employer may change because a housing program has both substitution and income effects.

The substitution effects are straightforward. If she gets no housing assistance, selling an hour of her time will yield her some nominal wage, net of taxes that include the benefit taper rates in other programs. If, however, she receives assistance, her net wage will be reduced by yet another tax, representing the share of her income that she is expected to contribute towards rent. Obviously, this somewhat depresses her willingness to sell her leisure time to a potential employer.

Housing assistance also should have income effects. In principle, admission to one of the deeply targeted programs guarantees the household the use of a standard quality housing unit if the head of household has no income whatever. Even in the absence of a marginal tax rate on wages, the housing guarantee might make the client somewhat less interested in offering up her free time to the market.

(Figure 1 about here.)

Figure 1 gives a static view of leisure–goods tradeoffs, with labor supply ('Hrs') on the X-axis and consumption ('Good') on the Y-axis. The individual has a utility function representing 'tastes and preferences' that govern her choices between consuming more market goods and having more free time, or leisure. Joining combinations of leisure and goods yielding equal amounts of utility forms indifference curves such as  $U$  and  $U'$  in Figure 1; when the agent's budget constraint reflects no housing assistance ('No HA'), her labor supply is  $A$  and her utility is  $U$ . When housing assistance is introduced

('HA'), the slope of the budget constraint becomes less steep because the housing program tax reduces the net wage, and thus the slope; the guaranteed level of housing consumption raises the intercept. The agent's utility rises to  $U$ , but her labor hours fall to  $B$ .

(Figure 2 about here.)

The neoclassical hypothesis, therefore, is that housing assistance programs will depress a head of household's willingness to work in the short run. Although Figure 1 presents the case of a marginal reduction in hours worked – possibly a case of little policy significance – one cannot exclude the possibility that shift in the budget constraint would lead some assistance recipients to a corner solution, that is, having no job. This case is depicted in Figure 2).

An agent with the utility function depicted would work  $C$  hours if unassisted, but zero hours if assisted.

No reasonable housing assistance program can readily escape the alleged negative consequences. To avoid an income effect one would have to stop helping people altogether. A program without a tax or taper on benefits as income grows would assist everyone, whether in need or not, and tax everyone to pay for it. Some housing programs have no taper up to a certain income  $Y^*$ , but provide no assistance at all above  $Y^*$ . These programs have not escaped the dilemma, as the tax rate on  $Y^* + 1$  might be 10,000% or more.

#### Weaknesses of the Neoclassical Hypothesis

The neoclassical hypothesis has two major theoretical weaknesses: treatment of assistance as an income supplement and neglect of selection effects.

*Objection 1: In-kind character of program.* We have, up to this point, treated the assistance as an income supplement rather than as a commodity subsidy. This is a gross oversimplification; the income effect is particularly questionable because true housing assistance is not fungible. Michael Murray demonstrated in 1980 that most commodity subsidy programs induce more work effort than equivalent cash transfer programs, by stimulating the desire to consume more of the subsidized good with additional income. In this view, housing assistance should be modeled as a price cut to a particular good. The higher resulting real income might result in greater consumption of housing, greater consumption of other goods, and/or greater consumption of leisure. The leisure choice is indeterminate without further assumptions about preferences for consumption of other goods, which are likely to be heterogeneous.

The heterogeneity of assisted families can be considerable. Consider four cases:

1. Housing assistance might enable a parent to move away from a gang-infested area, where she need not monitor her teenager as closely. Perhaps that will lead to new employment. Or it might cause her to move to a gang area, where it is easier to use her assistance. If assistance is only available through residence in a housing project with a

gang, it will be not only easier but also necessary. Perhaps that will cause her to quit her job.

2. Assistance might permit a parent to move out of the apartment where she has been living, doubled up, with her sister's family. Perhaps the reduction in background chaos will make job search easier; perhaps the increased privacy will make her feel more comfortable if she stops looking for work.

3. Suppose the head of household is caring for a sick relative, and works only enough to pay the rent. With assistance, she might stop working. But perhaps, with the rent mostly taken care of, she can get out of charity waiting rooms and into a full-time job.

4. A student recipient who is currently working part-time might be encouraged by housing subsidy payments to cut back on work hours so as to attend school full-time. Her schooling might or might not lead to full-time employment later on.

In short, so long as housing assistance is not an unconditional cash grant, the effect of housing assistance on labor supply, as a matter of either theory or practice, is ambiguous. The effect depends on which people are assisted, and how the assistance is delivered.

*Objection 2: Rationing and selection effects.* Housing assistance in many countries is not an entitlement, and even where it is theoretically an entitlement, as in Scandinavia, many eligible families fail to participate. In the United States, assistance is rationed to income-eligible households according to various local criteria by public housing authorities and project owners. Selection effects might mitigate any depressing effect on earnings and would certainly complicate its estimation. Blackorby and Donaldson first analyzed the selection effect of a 'tagged' good in 1988. They showed that if the government cannot observe potential income, it can help ensure selection of the most needy into assistance by providing it in a form that is unappealing to those with higher potential income.

Many housing projects are unappealing to those who can afford better units. In the United States, housing vouchers are sufficiently unattractive to many landlords that a significant fraction of those who receive vouchers never use them; eligible families with the highest incomes and lowest prospective subsidies are least likely to use their vouchers. Thus, while receipt of assistance may suppress realized income, potential income may suppress receipt of assistance. The practical conditions of assistance may be such that more able workers leave or never enter the programs. A high marginal tax rate like the 65 percent charged to some tenants in the United Kingdom, for example, will on the one hand reduce labor supply from some tenants who remain in the program, but will on the other hand push other tenants out of the program more quickly. The use of long waiting lists or onerous admissions procedures as rationing devices would have a similar impact. People who were able to adjust to the housing market without assistance (e.g., by moving to another jurisdiction, by getting another or better job) would tend to screen themselves out of the program. These forms of screening will

eliminate the least desperate, who may include those most responsive to the potential work disincentives.

### Methodological Issues

Empirical studies in this area might be subject to four different types of bias: bias from reporting error, selection bias, simultaneity bias, and general omitted variable bias.

*Bias from reporting error.* This is potentially fatal to certain types of study. The fact and type of housing assistance receipt are widely misreported by respondents, often in surprising directions. Researchers have to use survey data on assistance status with great caution. For example, in the 1993 American Housing Survey, the population equivalent of 2.235 million respondents informed their interviewers that they lived in public housing, which is explicitly defined in the survey instrument as units owned by a public housing authority. This is almost exactly double the true number. Thus, incorrect identification of assistance status is a threat to the validity of any study relying entirely on survey data.

*Selection bias.* Even if assistance status is correctly reported, selection bias may be difficult to control, for several reasons. If two households look the same, according to the data that we observe, but one has assistance and the other does not, one cannot assume that the assistance causes any observed differences in behavior, because the difference in selection into the assistance program may be related to other behavioral patterns.

First, selection into housing assistance is likely to be highly dependent on locality. Local housing markets dictate the degree of competition for scarce assistance resources. Program eligibility parameters also differ from one market to another, and local administrative preferences and procedures may determine in part which families rise to the top in that competition.

Second, many of the world's housing programs effectively exclude the full-time employed, even those with low wages. In programs that weed out workers with strong labor force attachment, the choices of the assisted may largely reflect the selection regime. Thus, the analyst who starts out arguing, as a normative matter, for treating the current clients of a program differently may end up arguing that the program should serve different clients altogether.

*Simultaneity bias.* This is related to selection bias, but in principle may be easier to control. As noted above, housing assistance may affect labor supply, but, also, employment success affects the use of housing assistance. If one wishes to measure the former but not the latter, one strategy is not to measure them at the same time. Assuming one has the other sources of bias controlled properly, one might model the effect of housing assistance receipt in the first period on employment success in the second.

*General omitted variable bias.* From a very large literature in labor economics, we know that employment varies with participation in other social programs, which have their own incentives and disincentives; with a worker's human capital; with the demands of the local labor market; with the individual's connectedness to social networks; and with sex, race, ethnicity, height, accent, personal appearance, motivation, intelligence, and many other factors. Failure to control for these factors will generally bias measurement of the specific effect of housing assistance. As we saw above, it is improbable that the effects of housing assistance on employment are uniform across different types of recipients. Omitted variable bias will naturally be lower whenever a richer set of relevant explanatory variables is available, but is inescapable in observational data.

#### Empirical Results: United States

In 2002 Shroder summarized nearly two dozen studies with a bearing on the effect of housing assistance on labor supply in the United States, writing that a rather large literature had failed to confirm the neoclassical hypothesis in its purest form, and that "the distribution of results from these empirical studies is consistent with a true housing assistance/short-term employment effect of zero".

That summary is obsolete. Three major reports since 2002 have made it apparent that housing assistance in the United States has a net negative impact on labor supply. However, that impact appears to vary among subgroups, may change over time, and seems rather small relative to the amounts paid out in subsidy.

In 2006 Mills and colleagues reported on a demonstration in which nearly 9000 welfare families with children in six distinct locations were randomly assigned either to receive housing vouchers or not to receive them, at least initially. In principle, use of administrative data and random assignment to treatment group eliminate all of the biases noted above. After allowing for the effects of failure by some members of the treatment group to actually use their vouchers, and for the tendency of some control group members to obtain vouchers with the passage of time, the Mills group reports a negative impact on earnings of about \$960 per household actually using a voucher in the first 18 months after random assignment, but the effect is not statistically significant in the following 24 months. During the period covered, the direct cost to the government of a voucher to a welfare family for 18 months probably averaged about \$9000, so the measured loss to earnings is in the neighborhood of 10% of the subsidy outlay.

In 2008 Jacob and Ludwig reported on the experience of roughly 42,000 families in Chicago, again assigned by lottery either to receive or not receive vouchers. The Chicago sample is much more diverse in some ways than the Mills sample, containing families who never received welfare or had no children, but the danger of generalization from Chicago to the nation is an offsetting limitation. They find a negative earnings impact of about \$328 per quarter, with negative effects tending, if anything, to increase over time. As average subsidy outlays in Chicago in this period were roughly \$1700 per quarter, this would be consistent with measured loss of earnings in the neighborhood of 20% of the subsidy outlay.

Also in 2008, Carlson and coworkers exploited administrative data from the Wisconsin welfare and food stamps programs to track the employment experience of nearly 13 000 self-reported voucher recipients, who applied at least once for either the welfare or food stamps programs, and created a comparison group of 30 or so non-recipients for every recipient, using the propensity score method. They find a reduction in earnings of \$858 in the first year of voucher receipt, relative to the comparison group, but just \$277 in the fifth year following first receipt. The former number would be consistent with measured loss of earnings in the neighborhood of 15 percent of subsidy outlay. Because most families with children do not use the voucher for more than 4 years, the fifth-year estimate is consistent with no permanent impacts of the voucher on the career path of earnings once a family has relinquished it.

Together, these studies seem to establish that the US voucher program has a negative impact on the earnings of assisted families. The 'leakage from the bucket' seems to be in the range of 10 to 20 percent of subsidy outlays. Evidence on the growth or diminution of this leakage over time is inconsistent.

The best evidence available on the relative impacts of different forms of assistance does not indicate any difference between the impact of vouchers and of public housing. In 2003 Orr and colleagues reported on the experience of about 4600 tenants of high-poverty public housing projects in five cities who applied for vouchers in a special demonstration and were randomly assigned to regular vouchers, vouchers only usable in low-poverty neighborhoods, or to nonvoucher control status. Roughly 5 years after random assignment, there was no significant difference in earnings among the three groups.

#### Empirical Results Elsewhere

Empirical literature measuring the direct labor supply effects of housing assistance outside the United States is scattered and surprisingly sparse. Evidence from social experiments like those reviewed above is nonexistent outside the United States. This is not the case for other types of social interventions. The author has found significant empirical literatures only for Australia and Scandinavia. Work disincentives have been discussed elsewhere, but so far as I can tell the effects have not been measured. We omit discussion of the very complex results of Bingley and Walker (2001) in the United Kingdom.

*Australia.* Australia combines an entitlement housing allowance program with a rationed public housing program. The cash assistance (income support) programs, such as disability pension, unemployment benefit, and so on, are the passports to eligibility for the housing allowance program. The housing allowance is withdrawn once entitlement to the underlying pension or allowance is zero, and the taper rate is the same as that applying to the pension or allowance. This arrangement avoids adding to the multiple stacking program, but extends the income range over which high effective marginal tax rates can persist. The entitlement program simply adds an



additional guarantee to households already receiving cash assistance; the cash assistance programs have their own tapers and the entitlement program does not add to them.

The extant literature is primarily concerned with labor supply impacts of the public housing program, where the basic tax rate is 25%. Hulse and Randolph write that both entitlement and public housing program survey respondents are well aware of work disincentives in their respective programs, but that only public housing tenants considered them serious concerns. Whelan (2004) finds some evidence that housing assistance reduces the likelihood that assisted tenants have any employment (like the corner solution in Figure 2) but no evidence that it reduces the choice of hours worked (like the marginal case in Figure 1). In his view the programs do not have a “sizeable or substantial impact on labor market activity” because receipt is conditional on participation in other government programs, which have their own, more serious, work disincentive effects.

(Table 1 about here.)

*Scandinavia.* Table 1 is adapted from Nordvik and Ahren (2005). It shows the percentage of households receiving housing allowances and the percentage of GDP paid out in housing allowances in the four Scandinavian nations in 2002, and we have added the United States as a reference point.

Allowances are an entitlement in Scandinavia, but differences among these four countries are large. Finland, for example, gave allowances to one-fifth of its population but in doing so redistributed only one part in 800 of its national income. This implies a very low guarantee, a very low taper, and therefore a very low impact on labor supply; housing problems would need to be widespread but very shallow to justify this program. The other three countries were redistributing about one part in 150 of national income, with very different participation rates: 6% in Norway, 21% in Denmark, and 36% in Sweden. These figures imply high guarantees in all three countries, with a high taper in Norway and lower tapers in the other two. In Sweden, for example, the tax rate is graduated but never exceeds 20%.

We have found no analyses of impact on short-term labor supply, but several on the related question of longer-term dependence on subsidy. If assistance keeps people out of the labor market, absence from the labor market keeps them poor, and poverty keeps them in assistance, then we should expect to see duration dependence in the data, that is, households would be more likely to stay in assistance if they are already assisted.

To the extent that the neoclassical hypothesis applies anywhere in Scandinavia, it must apply with greatest force in Norway, but Nordvik and Ahren reported no evidence of a dependency culture in a data set comprising all assistance recipients with children. Attrition from the program is high and they find no sign of duration dependence.

If Norway with its high guarantee and high taper generates no signs of duration dependence, it should not be surprising that Sweden with its somewhat lower guarantee and relatively low (20%) taper does not either. Chen (2006) found no indication of duration dependence and nothing to indicate that welfare traps should be a serious concern for the Swedish housing allowance system.

### Conclusion

Housing subsidies, like other parts of the welfare state, might discourage clients from pursuing earned income. Current literature appears to indicate that the effects of housing assistance on contemporaneous labor supply are modestly negative. Findings on long-term effects on economic self-sufficiency are not entirely consistent, but appear modestly negative at worst and negligible at best. The modest size and heterogeneity of the labor-supply response may cause a lack of robustness in findings from studies based on observational data. Controlled experiments outside the United States would therefore be highly desirable.

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Figure 1: Static Neoclassical Analysis of Labor Supply Effects of Housing

Assistance Program

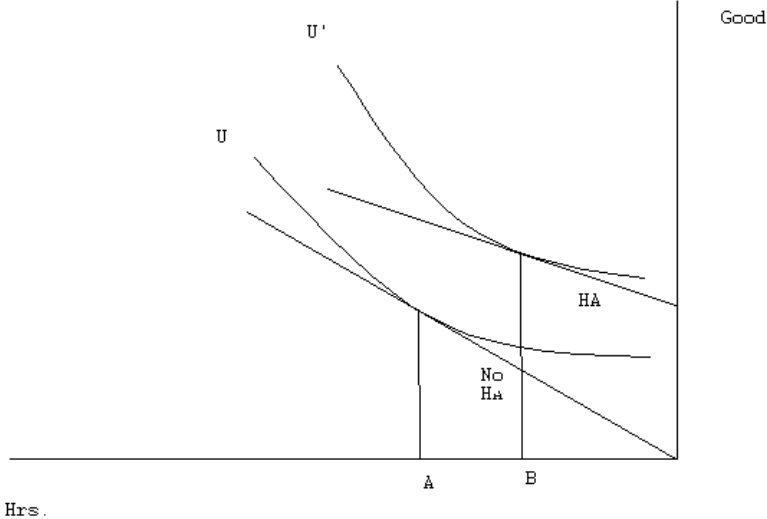
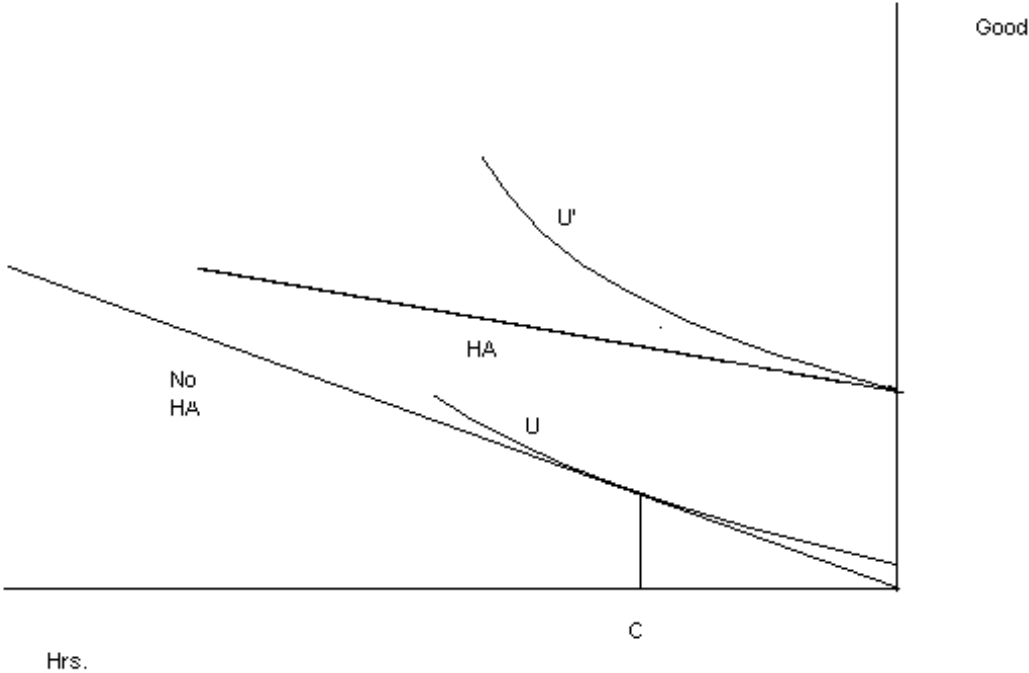


Figure 2: Static Neoclassical Analysis with Corner Solution



**Table 1: Households Receiving Housing Allowances as Percent of All Households and Allowances as Percent of GDP in Four Scandinavian Nations and the USA, 2002**

	<b>Denmark</b>	<b>Finland</b>	<b>Norway</b>	<b>Sweden</b>	<b>USA</b>
Allowance Recipients as Pct. of All Households	21%	20%	6%	36%	4%
Allowances as Pct. of GDP	0.64%	0.12%	0.61%	0.69%	0.21%

Source: Scandinavian data from Nordvik and Ahren (2005), US from author.