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Are public housing projects good for kids?[☆]

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Abstract

One goal of federal housing policy is to improve the prospects of children in poor families. This paper examines the effect of public housing participation on housing quality and educational attainment. Using the SIPP, we show that living in projects is associated with more negative outcomes for children, although this appears to be due to unobserved heterogeneity. We control for the endogeneity of project participation using TSIV techniques which combine information on project participation from the CPS with information on outcomes from the Census. We find that project households are less likely to suffer from overcrowding or live in high-density complexes. Project children are less likely to have been held back. Thus, our results run counter to the stereotype that housing projects harm children.

Keywords: Public housing; Children; Instrumental variables; Welfare; Education

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1. Introduction

Since 1937, the federal government has subsidized the housing costs of some low-income families, with the stated goal of improving the quality of housing

[☆]The STATA programs and data used in this study may be obtained from the authors.

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inhabited by the poor. Given that poor families with children make up 60 percent of the public housing caseload (most of the rest are households headed by the elderly and/or disabled), it is clear that a second important goal is to improve the life-chances of recipient children.

The real costs of this assistance (in 1996 dollars) have grown steadily over time, from $7.3 billion in 1977 to $26 billion in 1996. The number of households assisted has also risen from approximately 3.2 million in 1977 to 5.7 million in 1996, and annual outlays per unit have approximately doubled over the same period to $5480 (Committee on Ways and Means, 1996). However, public dissatisfaction with large public housing projects has remained high. More than 25 years ago, Henry Aaron wrote that ''Over the years public housing has acquired a vile image — highrise concrete monoliths in great impersonal cities, cut off from surrounding neighborhoods by grass or cement deserts best avoided after dark .... This image suggests that any benefits inhabitants derive from physical housing amenities are offset by the squalid surroundings'' (Aaron, 1972, p. 108). Many would argue that, if anything, the situation has worsened, as horrifying stories about large projects such as the Robert Taylor Homes or Cabrini Green in Chicago routinely appear in the national news.

As a result, the character of low-income housing aid has changed dramatically over time,[1] as money has been diverted away from 'project-based' aid toward 'household-based' aid given in the form of certificates and vouchers that can be applied toward rents in the existing private housing market.[2] Moreover, since 1982, appropriations for new construction of public housing projects have fallen sharply (Committee on Ways and Means, 1996).[3] And in 1995, the Department of Housing and Urban Development (HUD) put forth a plan that would have eventually replaced all 'project-based' assistance with housing certificates provided directly to individual households (Government Accounting Office, 1995).[4]

[1]There are other reasons for the shift in the composition of public housing from projects to vouchers. Apgar (1990) and Olsen (1983) point out that it is typically cheaper to house a family in existing housing than to construct new housing, so that more families can be served for the same budget outlay. Olsen (1983) and Olsen and Barton (1983) also argue that, in addition to being more efficient, an entitlement program of housing allowances would be more equitable than the current system in which some households receive benefits and other similar households do not. Finally, programs using existing housing do not crowd out private construction of low-rent housing as public construction projects might (Murray, 1983).

[2]In 1977, only 8 percent of assisted renters received vouchers or certificates compared to 28 percent in 1996.

[3]Note that even though Congress essentially stopped funding the new construction of large public housing projects in the early 1980s, many families continue to live in existing projects. Thus, there can be long lags between changes in public housing policy and actual changes in the composition of the caseload.

[4]Specifically, instead of giving money to local housing authorities, HUD would issue certificates to all current public housing project residents. These residents would then be able to choose to stay in their current units or move elsewhere.

The aim of voucher/certificate programs is to assist families without consigning them to the projects. But newspaper accounts not withstanding, there is little evidence that projects actually harm children. Basic economics suggests that families would not move into public housing projects unless they were better in at least some respects than the alternatives they faced. Aaron's intriguing hypothesis is that families in projects tradeoff physical housing amenities and reductions in rental payments against neighborhood characteristics that are bad for their children. But many projects and project neighborhoods may actually be superior to the housing and neighborhoods that families would have occupied in the absence of assistance. And reductions in rental payments may or may not be spent on goods and services beneficial to children. Thus, it is important to look directly at the effects of housing assistance on housing quality and on child well-being.

We first use data from the 1992 and 1993 waves of the Survey of Income and Program Participation (SIPP) to show that living in a project is associated with poorer outcomes, a finding which provides a baseline for our subsequent analyses. However, we find that this sample is too small to yield reliable estimates using instrumental variables techniques. We turn instead to the two-sample instrumental variable (TSIV) technique developed by Angrist and Krueger (1992, 1995) to combine information on the probability of living in a project obtained from the 1990 to 1995 waves of the March Current Population Survey, with information on outcomes obtained from the 1990 Census. The instrument common to both samples is an indicator equal to one if the household is entitled to a larger housing unit in a project because of the sex composition of the children in the household. Families entitled to a larger unit based on the sex composition are 24 percent more likely to live in projects. Using TSIV to control for unobserved characteristics of project residents, we find that project families are less likely to suffer from overcrowding and more likely to live in buildings with fewer than 50 units. And children in these families are 11 percentage points less likely to have been held back in school one or more grades. Thus, there is little evidence that the typical child living in a housing project is harmed by being there, and there is some evidence that living in projects may actually improve both living conditions and child outcomes.

The rest of the paper is laid out as follows: Section 2 gives additional background information about the public housing programs. Section 3 discusses methods, while Section 4 describes the data. Results appear in Section 5, and a discussion and conclusion follow.

2. Background

As noted above, public housing 'projects' tend to have very bad reputations. Yet, the publicity generated by the worst projects tends to obscure great heterogeneity between projects. Approximately 3300 public housing authorities

own and operate about 13,200 developments with a total of about 1.4 million units. Seventy percent of these authorities operate fewer than 300 units, while the 40 largest agencies operate 1786 or more units and account for 36 percent of all public housing project units. HUD considers most of the authorities to be well run — only 3 percent are classified as 'troubled' (General Accounting Office, 1995), but the eight worst large agencies account for 12 percent of all project units.

Thus it is not at all clear a priori that participation in the average project entails sacrificing either housing or neighborhood quality.[5] It is possible that most projects are significantly better than some of the low-rent housing that is available on the private market — in New York City alone, 60,000 people live in private housing so unsafe that it is judged to endanger lives (Sontag, 1996). For many families in projects, the alternative may be moving from place to place as they seek accommodations they can afford, interspersed with spells of homelessness. Children in these situations are often forced to change schools frequently which puts them at risk of grade repetition and poor academic achievement (General Accounting Office, 1994; Rubin et al., 1996). The fact that several large cities have lengthy waiting lists for public housing projects also lends credence to the idea that projects may be viewed as better than what is available and affordable privately.[6]

Families are eligible for assistance if they have incomes at or below 50 percent of the area median. Housing authorities may also choose to allocate as many as 25 percent of their units to families with incomes between 50 and 80 percent of the area median. Thus, families in projects are selected to be disadvantaged, something that must be kept in mind when housing quality and child outcomes are examined.

Families in projects have their rents capped at 30 percent of their income (after certain deductions are made), a regulation that may complicate the interpretation of 'rent' since families with more earnings will pay more. In fact, since the Census rent question is ambiguous, it is likely that some project families give the amount that they actually pay, while others attempt to estimate the rental value of their

[5]The effects of neighborhoods remain controversial though many studies have found that they are important. Wilson (1987) and Jencks and Mayer (1990) emphasize that bad neighborhoods may lack role models and desirable peers, as well as opportunities for education, recreation, and employment. Case and Katz (1991) provide some evidence that children in neighborhoods in which a large proportion of other children are involved in crime are more likely to be involved in crime themselves. Similar effects are found for drug and alcohol use and for idleness (i.e. being out of school and out of work). In another study of inner-city Chicago project residents who were allowed to apply for Section 8 housing certificates, Rosenbaum (1992) found that, in families that were able to move to the suburbs, children were less likely to drop out of school and both mothers and children were more likely to be employed.

[6]New York City currently has approximately 340,000 families on waiting lists for housing assistance (Sontag, 1996).

units.[7] In any case, it is not uncommon for researchers using survey data to find that participation in housing programs increases rental payments.[8] Hence, rather than focusing exclusively on rent, and assuming that reported rent is a good summary measure of housing quality, we examine several other measures of housing quality as well as measures of educational attainments. Unfortunately, the measures available in the SIPP and the Census are not identical. The SIPP measures are discussed further in the data section below.

In the Census, we have available two direct measures of housing quality (overcrowding and density), as well as grade repetition, a measure of children's educational attainment.[9] There is a good deal of evidence relating overcrowded conditions to ill health in children. Overcrowding leads to a higher incidence of respiratory illness (Mann et al., 1992), and of stomach infections (Galpin et al., 1992), and Coggon et al. (1993) report that overcrowding was related to a higher probability of death from all causes in a sample of English children.

High-density residential complexes contribute to social malaise among their residents. Fischer and Baldassare (1975) state that density is disliked, makes most people uncomfortable, and reduces local social interaction. This malaise may be linked to higher crime rates. For example, Condon (1991) finds that crime rates were lower in low-rise buildings than in high-rise buildings in the same Chicago projects. Atlas and Dreier (1993) cite similar evidence for New York showing that crime rates are lower in low-rise projects. In any case, HUD is actively engaged in replacing the most notorious large high-rise public housing complexes with

[7]On the 1990 Census form, respondents are instructed to classify their dwelling as 'Rented for cash rent' if any money is paid, even if the rent is paid by persons who are not members of the household, or by a federal, state, or local government agency. Most project residents should be in this category. Other categories include: owned by respondent or by someone in the household with a mortgage or loan; owned by respondent or someone in the household free and clear (without a mortgage); and occupied without cash rent (e.g., tenant sharecroppers or military personnel). Renter respondents are given the following instructions. ''Answer only if you pay rent for this house or apartment — what is the monthly rent?'' The answer categories are: 0–$80, $80–99, $100–124, $125–149, ..., $525–549, $550–599, ..., $700–749, $750–999, and $1000 or more. The supporting documentation (which is not on the questionnaire itself) explains, ''Report the rent agreed to or contracted for, even if the rent for your house, apartment, or mobile home is unpaid or paid by someone else.'' It is not clear how a project resident would interpret this question. In 1990 the American Housing Survey (AHS) changed from asking a similar question about the monthly rent to asking about both the monthly contract rent and the rent actually paid by subsidized families. We find (using the MSA sample) that among project residents in the 1990 to 1994 AHS the mean contract rent was $254 compared to a mean amount actually paid of $155 (nominal dollars).

[8]See, for example, Crews (1996) who uses data from the 1987 American Housing Survey, groups project and voucher recipients together, and finds an increase in rental payments of about 4 percent.

[9]We also tried an additional measure which was whether the household had access to complete plumbing and cooking facilities, but fewer than 1 percent of households lack either of these amenities. Nevertheless, in regressions similar to those reported in Table 4 below, we found that living in a project reduced the probability of lacking these amenities by 6 percent. This effect was significant at the 90 percent level of confidence.

low-rise 'garden' apartments. For example, two high-rises in the Henry Horner Homes in Chicago, the setting for Alex Kotlowitz's shocking book *There Are No Children Here* (Kotlowitz, 1991), are being demolished to make way for 700 townhouses to be located throughout Chicago's west side (HUD, 1996).

The measure of schooling attainment we use is whether a child has been held back one or more grades. Academic performance in early grades has been shown to be a significant predictor of eventual high-school completion (cf. Barrington and Hendricks, 1989; Cairns et al., 1989; Grissom and Shepard, 1989; Ensminger and Slusarcick, 1992), which in turn is linked to future employment probabilities and earnings. Thus, our three outcome measures are intended to capture important dimensions of the child well-being that may be affected by public housing including health, exposure to crime, and academic achievement.

3. Methods

The 1992 and 1993 waves of the SIPP have information about both project participation and interesting child outcomes, which enable us to estimate baseline ordinary least squares (OLS) regressions of the effects of project participation on child outcomes. To do so, we estimate models of the form

$$\text{OUTCOME} = \alpha_0 + \alpha_1 \text{PROJ} + \alpha_2 X + u, \tag{1}$$

where the OUTCOME variables include measures of housing, neighborhood, school quality, and grade repetition which are discussed in greater detail below, and X is a vector of additional exogenous explanatory variables including controls for the household head's gender, age, race, education, marital status, and the number of boys in the family. The X vector also includes MSA-level controls for the per capita availability of projects, vouchers, Section 8 substantial rehabilitation subsidies, and for the fraction of the population receiving the low-income housing tax credit. These variables from HUD's administrative data are added in an attempt to control for the availability of other forms of housing assistance in the metropolitan area. When OUTCOME refers to child educational attainment, dummy variables for the child's age and sex are also included in X. This procedure gives a baseline OLS estimate of the effect of projects on outcomes.

However, there is good reason to believe that OLS estimates will be biased by selection. Whether or not a family lives in a project reflects choices made by both households and program administrators. Many unobserved factors such as whether the family can double-up with friends and relatives or has recently been homeless are likely to affect both participation and outcomes. Our expectation is that failure to control for this source of endogeneity would bias the estimated effects of living in projects downwards since families in projects may be more likely to live in substandard housing in any case, and their children may be more likely to experience negative outcomes. Other factors that may affect participation and

outcomes are observed, but are either poorly measured or also endogenous (e.g., income from other welfare programs).

Thus, it is necessary to develop an instrumental variables strategy. Under HUD rules, the sex composition of children in the household affects the number of bedrooms in the subsidized unit, and therefore affects the size of the subsidy the family is eligible for. Except in the case of very young children, boys and girls cannot be required to share bedrooms, and there can be no more than two children per bedroom.[10] Thus, a family with two boys would be eligible for a two-bedroom apartment while a family with a boy and a girl would be eligible for a three-bedroom apartment. Note that HUD administrative data show that there are roughly equal numbers of two- and three-bedroom apartments in projects across the country.[11] Thus, it will not be the case, for example, that relative scarcity of three-bedroom apartments would result in differential selection rules being applied to mixed sex versus same child sex families. In what follows, we restrict the analysis to families with exactly two related children under 18 in the household in order to focus on the effects of sex composition and abstract from any effects due to the number of children. Alternative estimates using the sample of families with between two and six children are discussed in Table 5 below. Families eligible for larger apartments (i.e. higher subsidies) should be more likely to live in public housing projects other things being equal.

In order for sex composition to be a valid instrument, it must also be the case that it has no independent effect on our outcome measures, however. There is little reason to expect that sex composition will affect overcrowding (at least as we define it below) or density. But there is controversy in the literature about whether sex composition affects educational attainment. Butcher and Case (1994) argue that, for girls, the presence of any sisters reduces educational attainment. They find no effect of sex composition among boys. A closer inspection of their reported findings indicates that in two-child families they find significant sex composition effects only in the Panel Study of Income Dynamics, and not in the Current Population Survey or National Longitudinal Survey of Women data sets. Kuo and

[10]HUD requires that ''The dwelling unit shall contain at least one bedroom or living/sleeping room of appropriate size for each two persons. Persons of opposite sex, other than husband and wife or very young children, shall not be required to occupy the same bedroom or living/sleeping room'' (U.S. Department of Housing and Urban Development, 1993, p. 188). This rule appears to have been in effect at least since the early 1980s.

[1]We also estimated models designed to determine whether two- and three-bedroom apartments seemed to be equally available. To do this, we regressed housing development-level data about the average stay in the development, the average waiting time, and the fraction of movers in the previous year on characteristics of the housing complex including the fraction of one, two, and three-plus bedroom units, as well as MSA fixed effects. We could not reject the hypothesis that the fraction of two-bedroom units had the same effect on turnover as the fraction of three-bedroom units. This suggests that projects with three-bedroom units are as likely to have vacancies as those with two-bedroom units.

Hauser (1996) argue that it is difficult to find any consistent effect of sex composition on educational attainment, while Kaestner (1997) is unable to replicate the Butcher and Case findings using the National Longitudinal Survey of Youth (NLSY). It is possible that their result holds for older cohorts, but not for the younger group observed in the NLSY.

All of these studies focus on completed educational attainment. It is possible that sex composition has no effect on the probability of being held back, but does have some small effect on girls' completed years of schooling. In any case, we will keep the Butcher and Case results in mind and report the effects of project participation on the probability that boys are held back below — if sex composition matters only for girls, then sex composition should be a valid instrument in a sample of boys.[12]

In sum, we think we have identified sex composition as a valid instrument for participation in public housing projects. However, our attempts to implement this IV strategy in the SIPP were unsuccessful because the instrument was not statistically significant in the first stage regressions. We believe that this is largely an issue of sample size, as discussed below. While sex composition is in principle correlated with project participation, the effect may not be large. Other work using sex composition as an instrument (cf. Angrist and Evans, 1998) relies on larger data sets such as the U.S. Census. The Census has data on some interesting outcome variables, but does not have information about whether or not the family lives in a public housing project, the key right-hand-side variable of interest. Hence, we cannot estimate Eq. (1) in Census data using standard instrumental variables techniques and we turn to the TSIV approach.

As discussed in Angrist and Krueger (1992, 1995), TSIV is appropriate in situations in which the outcomes are available in one data set, the endogenous regressor is available in a second data set, and both data sets contain the instrumental variable and the other exogenous variables included in the model. We use the March CPS as the second data set. It contains information about whether or not the family lives in a public housing project, about the sex composition of the children in the household, and about a wealth of other potential control variables, such as parental education, which are expected to influence outcomes.

In our application, the TSIV method involves estimating the first stage equation predicting project residence using the CPS:

$$\text{PROJECT} = \beta_0 + \beta_1 \text{EXTRA} + \beta_2 X + v, \tag{2}$$

where PROJECT is a dummy variable equal to one if the family lives in a project, and EXTRA is a dummy variable equal to one if the family has a boy and a girl, and equal to zero if they have two boys or two girls.

In the second stage, the estimated coefficients from the first stage are used to

[12] See Angrist and Evans (1998) for use of this sex composition instrument in another setting.

predict project residence, PROJECT* in the Census data, and this predicted probability is included in models of outcomes estimated using Census data:

$$\text{OUTCOME} = \gamma_0 + \gamma_1 \text{PROJECT}^* + \gamma_2 X + \varepsilon. \quad (3)$$

The standard errors are then corrected to account for the fact that a predicted value of PROJECT is used in the second stage. Angrist and Krueger show that this procedure produces consistent estimates of the effect of the endogenous variable, PROJECT.

In view of the move towards certificate and voucher programs that was noted in the Introduction, it would be of interest to compare the effects of different housing programs rather than focusing exclusively on projects. Our focus on projects is dictated by the limitations of the SIPP and CPS data on public housing participation. The fundamental problem is that respondents are asked specifically about projects, but the survey is not very specific when asking about participation in other types of public housing programs. For example, in the CPS the relevant questions are: ''Is this house in a public housing project, that is, is it owned by a local housing authority or other public agency?'', and ''Are you paying lower rent because the federal, state, or local government is paying part of the cost?''. The second question covers Section 8 Certificate and Voucher Programs, but it also covers Section 8 Moderate Rehabilitation, and Section 8 New, and Substantive Rehabilitation Programs as well as various other subsidy programs. The SIPP questions are similar. Administrative data from HUD's 'Picture of Subsidized Housing' (HUD, 1997) indicates that less than half of the households answering ''Yes'' to the second question are likely to be participating in certificate or voucher programs.

It might still be the case, however, that the MSA-level variation in the fraction of households answering ''Yes'' to the rent subsidy question is driven by differences in participation rates in the voucher program across MSAs (i.e. that it provides a meaningful measure of participation in voucher programs). However, at the MSA level, we found little correlation between the fraction reporting subsidies in the CPS data and the fraction receiving vouchers in HUD administrative data. In contrast, there is a strong cross-MSA correlation between the fraction participating in projects in the CPS data, and the fraction participating in projects in the HUD data. Thus, the CPS questions can be used for looking at project participation but cannot be used to identify the separate effects of voucher programs.[13]

[13]It is also possible that some households that participate in voucher programs are wrongly classified as participating in projects (though we feel that the CPS question is very clear about what a project is). In this case, what we identify as project effects might in reality be effects of voucher programs. In order to address this problem, we tried limiting the sample to MSAs in which the administrative data indicated that a relatively high proportion of public housing units were located in projects. Unfortunately, the variation in the fraction of units that are located in projects is not high — it varies from about 40 percent to about 60 percent across most MSAs. Thus, this experiment reduced our sample size without eliminating much, if any, reporting error.

This data limitation raises questions about the interpretation of our estimates, since families who are not in public housing projects may either be in private, unsubsidized housing, or in some other form of publicly subsidized housing. While it is unfortunate that we cannot untangle these alternatives given the available data, we think it is not unreasonable, as a first pass, to ask how projects compare to all other alternatives. Public policy has turned from projects, not because there is good evidence that the alternatives are better, but because of revulsion towards the worst projects. The assumption underlying the recent expansion of other forms of assistance is that they will enable families to live in housing that more closely approximates that of unassisted families.

However, the fact that the alternative to public housing projects includes both subsidized and unsubsidized housing will pose problems for estimation of a pure 'project effect' if the instrument is correlated with other forms of housing as well as with residence in a public housing project. In this case, the estimated effect of living in projects will be biased because it will also pick up some of the effect of living in other subsidized housing. To see this, suppose that the true model is not (1) but:

$$\text{OUTCOME} = \alpha_0 + \alpha_1 \text{PROJ} + \alpha_2 \text{OTHER} + \alpha_3 X + u, \tag{4}$$

where the outcome depends on whether one lives in a project (PROJ), whether one lives in other subsidized housing (OTHER), or whether one lives in neither. The size of the bias on α_1 will depend on whether the instrumental variable is correlated with the omitted OTHER. If there is such a correlation, then the size and the magnitude of the bias will also depend on the correlation between PROJ and OTHER, which will in turn be determined by the housing policy in each metropolitan area.

In order to address these concerns, we have used the CPS data to estimate a take-up equation for other subsidized housing. The coefficient on our instrumental variable in this equation is -0.0028 with a standard error of 0.0024. Thus, there is no evidence in the data that our instrument is correlated with residence in other subsidized housing. In view of this result, it is not surprising that if we exclude the 'other public housing' people from the first stage regressions altogether, we obtain very similar results to those obtained using the full sample.

Also, recall that, as discussed above, if our sex-composition instrument is in fact correlated with participation in other subsidized housing programs, then the size of any resulting bias depends on the size of the correlation between PROJ and OTHER. We have investigated this question as follows. First, sex composition may affect participation in vouchers, but is unlikely to have any effect on, say, subsidies given to private contractors to rehabilitate low-income housing. Thus, the relevant correlation is the one between the number of project units and the number of voucher units in an MSA which is only 0.056. Thus, while the data are imperfect, we feel that there are reasonable grounds for interpreting our TSIV

estimates as unbiased estimates of the effects of public housing projects, α_1, since the correlation between sex composition and OTHER is not statistically significant, and the correlation between the number of project units and the number of voucher units in an MSA is quite small.

4. Data

The data from the SIPP comes from topical modules that were added to the ninth wave of the 1992 panel, and the sixth wave of the 1993 panel. Thus, all of the questions were asked in 1994. In addition to questions about public housing participation, parents were asked to subjectively rate their home, neighborhood, and the quality of the school on a scale of 0 to 10. Parents were also asked whether their children had repeated any grades for any reason, about the number of times that their children had changed schools (since frequent moves increase the risk of academic failure), and about the child's participation in extra-curricular activities.

The first two columns of Table 1 show means from the SIPP data by whether the family lived in a housing project or not. Table 1 shows that the available sample size in the SIPP is quite small, with only 86 households reporting residence in projects. Nevertheless, the participation rate is very similar to that reported in the CPS. It is evident that families in projects are less satisfied with their housing, neighborhoods, and schools than other families. Their children are less likely to participate in any extra-curricular activities, are more likely to have changed schools, and are somewhat more likely to have been held back. This latter finding is interesting in view of recent discussion of the social promotion of poor children. It is possible that schools located near housing projects have less rigorous standards for promotion than other schools, so that conditional on ability, fewer children are held back. Table 1 shows that, despite any tendencies in this direction, children who live in projects are more likely to be held back than other children.

The Census outcomes we focus on are recorded in the 1990 Census 1% and 5% Public Use Microdata Samples (PUMS). The Census asks about characteristics of the housing occupied by households. We focus on two variables: whether or not the family lives in high-density housing which is defined as a building with over 50 units; and whether or not the family is overcrowded, which we define as having fewer than three living/bedrooms. It is important to note that our measure of overcrowding is independent of the sex composition of the children, or of the marital status of the mother since all families in our sample are entitled to either a two-bedroom (i.e. three living/bedrooms) or a three-bedroom (i.e. four living/bedrooms) apartment.[14] Unfortunately, the smallest geographical unit identified in

[14] As Table 2 shows, relatively few families in our sample live in less space than this. We tried an alternative measure of overcrowding, which was defined as having the number of rooms plus one being less than the number of persons, and obtained very similar results.

Table 1
Variable means (standard errors)[a]

	SIPP		CPS		CPS		Census	
	Projects=1	Projects=0	Projects=1	Projects=0	Extra bedroom=1	Extra bedroom=0	Extra bedroom=1	Extra bedroom=0
Part. in public housing	1.0 (0)	0.0 (0)	1.0 (0)	0.0 (0)	0.053 (0.002)	0.044 (0.002)	–	–
Extra bedroom	0.523 (0.054)	0.511 (0.012)	0.543 (0.015)	0.493 (0.003)	1.0 (0)	0.0 (0)	1.0 (0)	0.0 (0)
Rating of home	6.909 (0.337)	7.873 (0.048)	–	–	–	–	–	–
Rating of neighborhood	5.506 (0.384)	7.496 (0.051)	–	–	–	–	–	–
Rating of school	6.777 (0.312)	7.326 (0.056)	–	–	–	–	–	–
Child extracurricular	0.450 (0.056)	0.606 (0.011)	–	–	–	–	–	–
Changed schools	0.547 (0.058)	0.402 (0.011)	–	–	–	–	–	–
Child held back	0.111 (0.037)	0.101 (0.007)	–	–	–	–	0.038 (0.000)	0.039 (0.000)
Rent/1000	–	–	–	–	–	–	0.530 (0.001)	0.521 (0.001)
Family is overcrowded	–	–	–	–	–	–	0.040 (0.001)	0.043 (0.001)
Dense building	–	–	–	–	–	–	0.021 (0.000)	0.023 (0.000)
Child's age	11.030 (0.351)	11.003 (0.068)	10.721 (0.097)	10.941 (0.021)	10.894 (0.029)	10.971 (0.030)	10.878 (0.008)	10.916 (0.008)
Child is girl	0.554 (0.050)	0.497 (0.010)	0.464 (0.015)	0.486 (0.003)	0.495 (0.004)	0.475 (0.004)	0.500 (0.001)	0.474 (0.001)
Head's age	34.884 (1.109)	37.105 (0.173)	32.835 (0.260)	36.216 (0.054)	35.762 (0.072)	36.339 (0.077)	35.814 (0.020)	35.844 (0.021)
Head married	0.349 (0.052)	0.736 (0.010)	0.249 (0.013)	0.717 (0.003)	0.707 (0.004)	0.681 (0.004)	0.727 (0.001)	0.711 (0.001)
Head female	0.733 (0.048)	0.348 (0.011)	0.737 (0.014)	0.254 (0.003)	0.269 (0.004)	0.286 (0.004)	0.237 (0.001)	0.248 (0.001)
Head black	0.570 (0.054)	0.133 (0.008)	0.486 (0.015)	0.131 (0.002)	0.144 (0.003)	0.152 (0.003)	0.149 (0.001)	0.155 (0.001)
Head other	0.023 (0.016)	0.058 (0.005)	0.059 (0.007)	0.056 (0.002)	0.056 (0.002)	0.055 (0.002)	0.111 (0.001)	0.110 (0.001)
Head hispanic origin	0.244 (0.047)	0.178 (0.009)	0.236 (0.013)	0.217 (0.003)	0.216 (0.004)	0.220 (0.004)	0.140 (0.001)	0.141 (0.001)
9≤Hd ed≤11	0.314 (0.050)	0.100 (0.007)	0.256 (0.013)	0.122 (0.002)	0.128 (0.003)	0.130 (0.003)	0.148 (0.001)	0.152 (0.001)
Hd ed=12	0.372 (0.052)	0.378 (0.011)	0.418 (0.015)	0.385 (0.003)	0.383 (0.005)	0.390 (0.005)	0.321 (0.001)	0.318 (0.001)
13≤Hd ed≤15	0.198 (0.043)	0.266 (0.010)	0.190 (0.012)	0.261 (0.003)	0.265 (0.004)	0.250 (0.004)	0.312 (0.001)	0.311 (0.001)
Hd ed ≥16	0.047 (0.023)	0.181 (0.009)	0.039 (0.006)	0.157 (0.003)	0.151 (0.003)	0.151 (0.003)	0.156 (0.001)	0.155 (0.001)
No. of boys	0.919 (0.074)	1.018 (0.016)	1.003 (0.021)	0.998 (0.005)	1.0 (0)	0.996 (0.010)	1.0 (0)	1.050 (0.003)
No. of observations	86	1868	1048	20,670	10,755	10,963	142,995	136,134

[a] Notes. For variables dealing with children (held back, child's age, and child's gender), means and standard errors drawn from the 'child sample'; otherwise, means and standard errors from 'housing sample.' Monthly rent is computed only for renters; homeowners are excluded.

the PUMS is the MSA, so it is not possible to look at the effects of project participation on neighborhood characteristics using these data.

The Census does not ask about grade repetition per se, but does ask about children's educational attainment. The answers are grouped as follows: nursery school, kindergarten, grades 1 to 4, grades 5 to 8, grade 9, grade 10, grade 11, and higher grades (which are not relevant for our purposes). We define children as having been 'held back' at least one grade if they are 6 years old and have not completed nursery school; if they are 7 years old and have not completed kindergarten; if they are 8 to 11 years old and are not in at least grades 1 to 4; if they are 12 to 15 years old and are not in at least grades 5 to 8; if they are 16 years old and have not completed grade 9; and if they are 17 years old and have not yet completed grade 10.

Because grades are grouped together, the probability of being held back varies with the child's age — for example, as shown in Appendix A, we classify 4.5 percent of 8-year-olds as being held back, but only less than 1 percent of 11-year-olds because we cannot distinguish in the data between an 11-year-old in grade 4, and an 11-year-old in grade 1 or 2. Only 11-year-olds who are lagging very far behind (they are in less than grade 1) can be classified for certain as 'held back.' The probability of being classified as held back rises to 6.1 percent for 12-year-olds, and 6.7 percent for 16-year-olds, so our measure does rise with age, as it should, among children for whom 'held back' is defined in approximately the same way. In order to deal with this measurement problem, we include single year of age dummies in the models of 'held back.' We also repeat our analyses for the subsample of children for whom 'held back' is defined most similarly (8-, 12-, 16-, and 17-year-olds), and for a sample that excludes 6- and 7-year-olds, since, among these children, low educational achievement may reflect delays in starting school rather than failure to complete a grade.

Although the Census data on children's education is imperfect, it is better than that available in either the CPS or the main SIPP survey which ask about education only for children 15 and older. However, it must be kept in mind that, for most of our sample children, what we are measuring is very severe age-grade delay. That is, children who began school at 6 rather than 5, or who were held back only one grade are unlikely to pass our definition of 'held back'.

As discussed above, we focus on households with two related children under 18. There are a number of additional screens applied to the data. We exclude individuals in households with members over the age of 61, since they may be eligible for public housing on the grounds of age. Since we go on to match Census data with information from the CPS, we focus on the subset of MSAs that are identified in the CPS.[15] This restriction has the effect of eliminating project residents in some smaller towns from our sample. But anecdotal evidence as well

[15]The CPS identifies the 113 largest MSAs, plus 89 selected MSAs, and 66 selected PMSAs.

as HUD evaluations suggest that it is the largest projects that are most troubled, and these projects are unlikely to be located in small urban areas. Hence, this sample restriction is likely to exaggerate any negative effects of projects. We also restrict attention to households in which the head and spouse (if present) are over the age of 17, and in order to come up with one observation per household, we use data only from household heads. Finally, we restrict attention to households with incomes less than $50,000 in order to focus on households for whom project residence is likely to be an option.[16] We call the resulting samples of households the 'housing samples.' The samples we use for examining educational attainment are somewhat different, since the unit of observation is the child, and the children must be between 6 and 17 years old, inclusive.

A limitation of the SIPP and CPS participation data is that it refers to whether or not a household was living in public housing in March of the survey year. The effects of public housing on schooling attainment cannot be expected to be instantaneous — thus, our estimates of the effects of participation on the probability of being held back are only meaningful if current residence in a project is a marker for probable longer term residence. The HUD administrative data speak to this issue — the average length of time since the household moved in is 7 years with a standard deviation of about 5 years, and the average total stay of households is 12 years.

The third and fourth columns of Table 1 show means of the CPS data used to estimate the first stage by whether or not the household lives in a project. A comparison of columns 1 and 2 indicates that households who live in projects are more likely to be eligible for an extra bedroom: 54 percent of these households have a boy and a girl compared to 49 percent of households outside of projects. Table 1 also confirms that, as discussed above, households in projects are likely to be disadvantaged along a number of observable dimensions. For example, they are more likely to be female-headed and the heads are less educated.

The next four columns of Table 1 divide the CPS and Census samples by whether or not the family is entitled to an extra bedroom. The families in columns 5 and 7 have a boy and a girl, whereas families in columns 6 and 8 have either two boys or two girls. The raw CPS data in the first row shows that families who are entitled to an extra bedroom are 21 percent more likely to live in a project. However, the remainder of the table shows that these families also differ from other families in some respects — in particular, they are less likely to be female-headed. These small differences imply that, other things being equal, families with mixed-sex children should be less likely to be in projects. Hence, the fact that they are in fact more likely to live in projects suggests that the availability

[16]For instance, we wish to exclude wealthy homeowners from the sample. An alternative method of achieving this would be to exclude all homeowners from the sample and focus on renters. However, for relatively low-income people the availability of projects could conceivably influence home ownership decisions, and so being a renter would have to be viewed as an endogenous choice.

of an extra bedroom has a strong incentive effect on families who are choosing between projects and other housing.

However, average differences between families with boys and girls, and families with two same-sex children raise the possibility that sex composition affects outcomes not only by raising the probability of living in a project, but also through other unspecified means. It is important to note, however, that because only approximately 5 percent of sample households participate in public housing, the differences between columns 5 and 6 and between columns 7 and 8 are driven primarily by differences between households who do not live in projects. If we compare mixed-child-sex families in projects to same-child-sex families who are not in public housing, then the former are indeed very disadvantaged relative to the later (as columns 1 and 3 would suggest).

Table 1 indicates that, by restricting our sample to families with exactly two children, we end up with more boys than girls. In other words, there are more families with exactly two boys than there are families with exactly two girls. This finding is in keeping with Angrist and Evans' (1998) observation that families whose first two children are girls are more likely to have a third child than families who initially have two boys. Moreover, the second two columns of Table 1 show that households outside of projects are more likely to be two girl than two boy families. Clearly, boy/girl families are more likely to live in the projects than girl/girl families. But the same is also true for boy/boy families.[17] Thus, boy/girl families are more likely to live in projects than same-sex households regardless of whether the latter have boys or girls.[18]

Finally, a comparison between the CPS figures shown in columns 5 and 6 of Table 1 and the Census figures shown in columns 7 and 8 suggests that there are only slight differences between the two samples. One exception is that the Census

[17]The percentages in public housing are 2.4 for girl/girl households, 3.0 for boy/girl households, and 2.7 for boy/boy households.

[18]This finding may create concern that, because families with two same-sex children are more likely to have a third child, these families could be selected out of our sample. While this is true, it is unlikely that this type of selection could explain the difference in take-up rates between families with a boy and a girl, and families with two same-sex children. To see this, start with Angrist and Evans' (1998) finding that having two same-sex children leads to a 6.28 percentage point increase in the likelihood of having another child, where the mean is 37.5 percent. That is, families with the first two children of the same sex are 16.7 percent more likely to select out of our sample. Table 3 shows that those entitled to an extra bedroom have a participation rate of 5.2 percent compared to a participation rate of 4.3 percent among those with two same-sex children. Suppose that the 16.7 percent who had selected out of our sample of families with same-sex children actually had a participation rate as high as those with a boy and a girl (i.e. 5.2 percent). Then adding these families back into the same-sex sample would raise their propensity to participate by only 4.45 percent. Those who selected out would have to have an average propensity to participate of 9.68 percent in order to explain the gap between the two groups. This issue is explored further below.

families are less likely to be female-headed, and less likely to be classified as Hispanic rather than 'other origin.'[19]

5. Results

5.1. OLS estimates of the effects of project participation

OLS estimates of Eq. (1) are shown in Table 2 for the SIPP outcome variables. We find that after controlling for observable characteristics, households in projects are still less satisfied with their housing and their neighborhood than other households. Moreover, project children are still more likely to have changed schools. However, there are no significant differences between project and other children in school ratings, extra-curricular activities, and grade retention once observables are controlled for.

Recall that, as discussed above, we expect omitted variables to bias the coefficients on PROJ towards finding more negative outcomes. Hence, we turn to TSIV to try to identify the 'true' causal effect of project residence on outcomes.

5.2. TSIV estimates

The first stage estimates of Eq. (2) are shown in Table 3 for the CPS child sample and for the housing sample. In both cases the extra bedroom/sex composition variable is a highly significant determinant of project participation with *t*-statistics of 4 and 3, respectively. To understand the magnitude of this effect, consider the coefficient estimate in the first column. The baseline participation rate in projects is 4.75 percent, while the marginal effect of adding an extra bedroom is 1.13 percentage points. Thus, adding an extra bedroom increases the likelihood of project participation by 24 percent. The other controls included in the model indicate that participation declines with the age of the head, is much lower for married heads, and is highest among blacks and those with less than a high school education. The dummy variables for child age are not individually or jointly statistically significant, indicating that the probability of living in a project does not vary with child age.

TSIV estimates of Eq. (3) appear in Table 4. The first column, which shows the

[19]There is some discrepancy between the CPS and the Census in the number of married/spouse present households who report that the household head is female (94 percent in the Census compared to 89 percent in the pooled CPS sample). Hence, in order to use the same definition of 'female headed' in the two samples, we have adopted the conservative strategy of assuming that the household head is male in these cases. Hispanic is derived from the 'detailed Hispanic origin code' in the Census, and from the 'origin' code in the CPS. We coded any respondent who answered "yes" to Hispanic ethnicity as hispanic regardless of racial origin (white/black/other).

Table 2
Results from SIPP using OLS[a]

	Rating of home (11-point scale)	Rating of neighborhood	Rating of school	Child ever held back	Changed schools	Extra-curricular activities
Participation in public housing	−0.4621 (0.2449)	−1.1602 (0.2598)	−0.3212 (0.2886)	−0.0374 (0.0377)	0.1197 (0.0576)	−0.0073 (0.0564)
Child is age 7	–	–	–	0.0318 (0.0286)	0.1699 (0.0445)	0.1344 (0.0443)
Child is age 8	–	–	–	0.0537 (0.0301)	0.2753 (0.0467)	0.1498 (0.0467)
Child is age 9	–	–	–	0.0793 (0.0296)	0.3861 (0.0460)	0.1817 (0.0457)
Child is age 10	–	–	–	0.0794 (0.0305)	0.2946 (0.0475)	0.2190 (0.0471)
Child is age 11	–	–	–	0.0799 (0.0307)	0.4313 (0.0475)	0.2692 (0.0474)
Child is age 12	–	–	–	0.1260 (0.0329)	0.5660 (0.0509)	0.2662 (0.0504)
Child is age 13	–	–	–	0.1249 (0.0305)	0.5313 (0.0474)	0.2465 (0.0469)
Child is age 14	–	–	–	0.0832 (0.0322)	0.5485 (0.0500)	0.1684 (0.0496)
Child is age 15	–	–	–	0.1035 (0.0336)	0.5655 (0.0525)	0.1694 (0.0516)
Child is age 16	–	–	–	0.1665 (0.0350)	0.5923 (0.0545)	0.2294 (0.0538)
Child is age 17	–	–	–	0.2143 (0.0374)	0.5648 (0.0583)	0.1089 (0.0574)
Child is female	–	–	–	−0.0207 (0.0182)	−0.0046 (0.0285)	−0.0435 (0.0282)
Head's age	0.1023 (0.0468)	0.1741 (0.0496)	0.1283 (0.0561)	−0.0093 (0.0090)	−0.0127 (0.0140)	−0.0082 (0.0137)
Head's age^2/100	−0.1244 (0.0604)	−0.2217 (0.0640)	−0.1613 (0.0720)	0.0114 (0.0110)	0.0063 (0.0173)	0.0111 (0.0168)
Head married	0.2888 (0.1519)	0.4655 (0.1611)	−0.1168 (0.1777)	0.0203 (0.0210)	−0.0344 (0.0326)	0.0561 (0.0322)
Head female	−0.1352 (0.1410)	−0.4064 (0.1495)	−0.2191 (0.1654)	0.0413 (0.0202)	0.0034 (0.0314)	−0.0239 (0.0309)
Head black	−0.3458 (0.1471)	−0.5583 (0.1561)	−0.2551 (0.1724)	0.0288 (0.0205)	0.0129 (0.0320)	−0.1060 (0.0316)
Head other	−0.5587 (0.2103)	−0.3181 (0.2231)	0.1803 (0.2475)	−0.0146 (0.0278)	0.0799 (0.0441)	−0.1112 (0.0430)
Head hispanic	0.1815 (0.1336)	−0.0419 (0.1420)	0.2617 (0.1574)	−0.0411 (0.0188)	0.0249 (0.0292)	−0.1622 (0.0287)
Heed ed 9–11	−0.0630 (0.2336)	−0.2129 (0.2476)	−0.2031 (0.2751)	0.0006 (0.0329)	0.0591 (0.0512)	−0.1070 (0.0502)
Head ed 12	0.0817 (0.2039)	−0.2294 (0.2167)	−0.1405 (0.2402)	−0.0448 (0.0286)	−0.0074 (0.0444)	0.0635 (0.0434)
Head ed 13–15	0.2910 (0.2096)	0.1567 (0.2229)	−0.1929 (0.2473)	−0.0515 (0.0295)	−0.0100 (0.0458)	0.1960 (0.0448)
Head ed 16+	0.4660 (0.2228)	0.1748 (0.2369)	−0.3514 (0.2622)	−0.0932 (0.0311)	0.0727 (0.0482)	0.2277 (0.0474)
No. of boys	0.0433 (0.0680)	−0.0079 (0.0721)	−0.0760 (0.0798)	0.0215 (0.0132)	−0.0008 (0.0206)	−0.0225 (0.0203)
Constant term	5.4986 (0.9161)	4.1366 (0.9711)	5.2736 (1.1055)	0.2113 (0.1806)	0.4205 (0.2817)	0.5343 (0.2769)
Sample	Housing sample	Housing sample	Housing sample	Child sample	Child sample	Child sample
No. of observations	1778	1778	1706	2009	1989	2080
R^2	0.0411	0.0985	0.0138	0.0510	0.1425	0.1176

[a] Notes. 1992/1993 SIPP topical modules. Observations differ across columns because of missing values.

Table 3
Results from CPS first stage regression on public housing participation[a]

Extra bedroom	0.0116 (0.0028)	0.0097 (0.0031)
Child's age 7	–	0.0027 (0.0053)
Child's age 8	–	−0.0011 (0.0055)
Child's age 9	–	0.0078 (0.0057)
Child's age 10	–	0.0043 (0.0056)
Child's age 11	–	0.0030 (0.0056)
Child's age 12	–	−0.0018 (0.0057)
Child's age 13	–	0.0044 (0.0059)
Child's age 14	–	0.0051 (0.0059)
Child's age 15	–	0.0005 (0.0063)
Child's age 16	–	−0.0048 (0.0062)
Child's age 17	–	−0.0103 (0.0062)
Child is girl	–	−0.0057 (0.0020)
Head's age	−0.0099 (0.0013)	−0.0086 (0.0021)
Head's age^2/100	0.0105 (0.0017)	0.0087 (0.0025)
Head married	−0.0187 (0.0063)	−0.0154 (0.0073)
Head female	0.0630 (0.0065)	0.0580 (0.0080)
Head black	0.0937 (0.0042)	0.0814 (0.0070)
Head other	0.0237 (0.0061)	0.0276 (0.0076)
Head hispanic origin	0.0014 (0.0038)	0.0050 (0.0044)
9≤Head's ed≤11 years	0.0077 (0.0065)	−0.0065 (0.0095)
Head's ed=12 years	−0.0204 (0.0059)	−0.0282 (0.0082)
13≤Head's ed≤15 years	−0.0311 (0.0062)	−0.0399 (0.0083)
Head's ed ≥16 years	−0.0328 (0.0067)	−0.0446 (0.0082)
% households in projects	0.7229 (0.0991)	0.7221 (0.1592)
% households with vouchers	1.7776 (0.2818)	1.8378 (0.3112)
% households in substantial rehabilitation	0.4383 (0.1415)	0.4684 (0.1850)
% households with LIHTC	0.3608 (0.1105)	0.3468 (0.1882)
No. of boys	0.0041 (0.0020)	0.0003 (0.0024)
Constant term	0.2126 (0.0257)	0.2015 (0.0429)
Sample	Housing sample	Children's sample
No. of observations	21,718	26,093
R^2	0.0974	0.0854

[a] Notes. Standard error in the second column is corrected for multiple children in same household.

effects on monthly rental payments, is estimated using the subsample of renters only. Although using this subsample raises issues of choice-based sampling, we wished to follow the existing public housing literature and look at the estimated effect of project participation on reported rent. Column 1 of Table 5 shows that the estimated effect on rent is positive and statistically significant, a finding that suggests that many households are reporting the rental value of their accommodations rather than what they actually pay. If this is the case, then the estimates in column 1 suggest that families in projects live in housing of better quality than the housing they would otherwise have inhabited.

Table 4
Results from Census using two sample IV[a]

	Rental payment/1000	Family is overcrowded	Dense building	Child was held back
Participation in public housing	0.3717 (0.0589)	−0.1595 (0.0624)	−0.1154 (0.0468)	−0.1113 (0.0691)
Child's age 7	–	–	–	−0.0640 (0.0017)
Child's age 8	–	–	–	−0.0382 (0.0019)
Child's age 9	–	–	–	−0.0621 (0.0018)
Child's age 10	–	–	–	−0.0770 (0.0016)
Child's age 11	–	–	–	−0.0768 (0.0016)
Child's age 12	–	–	–	−0.0144 (0.0021)
Child's age 13	–	–	–	−0.0555 (0.0018)
Child's age 14	–	–	–	−0.0681 (0.0017)
Child's age 15	–	–	–	−0.0739 (0.0016)
Child's age 16	–	–	–	0.0092 (0.0025)
Child's age 17	–	–	–	0.0353 (0.0028)
Child is girl	–	–	–	−0.0082 (0.0009)
Head's age	0.0193 (0.0008)	−0.0085 (0.0007)	−0.0032 (0.0005)	−0.0067 (0.0007)
Head's age^2/100	−0.0199 (0.0011)	0.0081 (0.0008)	0.0032 (0.0006)	0.0072 (0.0008)
Head married	0.0199 (0.0033)	−0.0239 (0.0022)	−0.0098 (0.0017)	−0.0102 (0.0022)
Head female	−0.0976 (0.0056)	−0.0025 (0.0044)	0.0154 (0.0033)	−0.0027 (0.0045)
Head black	−0.1211 (0.0070)	0.0472 (0.0059)	0.0343 (0.0045)	0.0086 (0.0057)
Head other	−0.0164 (0.0027)	0.0880 (0.0020)	0.0215 (0.0015)	0.0011 (0.0023)
Head hispanic origin	0.0005 (0.0021)	0.0731 (0.0013)	0.0263 (0.0010)	−0.0008 (0.0013)
9≤Head's ed≤11 years	0.0337 (0.0028)	−0.0562 (0.0018)	0.0076 (0.0014)	−0.0191 (0.0020)
Head's ed=12 years	0.0768 (0.0029)	−0.0787 (0.0021)	0.0020 (0.0016)	−0.0308 (0.0027)
13≤Head's ed≤15 years	0.1345 (0.0036)	−0.0844 (0.0026)	0.0031 (0.0019)	−0.0363 (0.0033)
Head's ed ≥16 years	0.2047 (0.0041)	−0.0788 (0.0028)	0.0055 (0.0021)	−0.0373 (0.0037)
% households in projects	−0.8652 (0.0638)	−0.1738 (0.0539)	2.1997 (0.0405)	0.0263 (0.0563)
% households with vouchers	−2.9203 (0.2748)	0.7380 (0.1348)	−0.5096 (0.1012)	0.1307 (0.1453)
% households in subst. rehabilitation	−1.0260 (0.0840)	−0.1974 (0.0466)	−0.4683 (0.0350)	0.0367 (0.0480)
% households with LIHTC	−0.7055 (0.0889)	−0.1382 (0.0384)	0.1661 (0.0288)	0.0772 (0.0379)
No. of boys	−0.0044 (0.0010)	0.0001 (0.0006)	0.0000 (0.0004)	0.0008 (0.0007)
Constant term	0.1193 (0.0158)	0.3039 (0.0152)	0.0733 (0.0114)	0.2680 (0.0171)
Sample	Housing sample (renters)	Housing sample	Housing sample	Child sample
No. of observations	116,901	279,129	279,129	340,081
R^2	0.1489	0.0854	0.0544	0.0373

[a] Notes. Standard errors in fourth column are corrected for multiple children in same household.

This interpretation is supported by the point estimates in columns 2 and 3 of Table 4 (estimated using the full sample), which show that households in projects are less likely to be overcrowded, and also less likely to live in large, dense, complexes than other families. Finally, column 4 suggests that families in projects are not trading off physical housing amenities against other factors that harm child outcomes — we estimate that children in the projects are 11 percentage points less

Table 5
Robustness checks (from Census using two sample IV)[a]

	Family is overcrowded	Dense building	Child was held back
A. Other covariates include: child's age and sex (in column 3), head's age, sex, race, number of boys, and MSA-level controls for public housing supply			
Participation in public housing	−0.1698 (0.0639)	−0.1217 (0.0478)	−0.1161 (0.0683)
B. Other covariates include: covariates in Table 4, plus household income and its square			
Participation in public housing	−0.1704 (0.0654)	−0.1220 (0.0492)	−0.1108 (0.0680)
C. Head has high school or less, includes covariates in Table 4, 148,712 in 'housing sample' and 182,437 in 'child sample'			
Participation in public housing	−0.2761 (0.0885)	−0.1659 (0.0609)	−0.1621 (0.1011)
D. Head has at least some college, includes covariates in Table 4, 130,417 in 'housing sample' and 157,644 in 'child sample'			
Participation in public housing	0.0094 (0.0874)	−0.0445 (0.0787)	−0.0592 (0.0885)
E. Households with income less than $25,000, includes same covariates as in Table 4, 114,711 in 'housing sample' and 134,926 in 'child sample'			
Participation in public housing	−0.2621 (0.0791)	−0.1308 (0.0574)	−0.1433 (0.0671)
F. Households with income between $25,000 and $50,000, includes same covariates as Table 4, 164,418 in 'housing sample' and 205,155 in 'child sample'			
Participation in public housing	0.0763 (0.2149)	−0.2069 (0.1753)	−0.0918 (0.3611)
G. Expand sample to households with two to six children, 449,506 in 'housing sample' and 724,777 in 'child sample'			
Participation in public housing	−0.1876 (0.0779)	−0.1193 (0.0565)	−0.1729 (0.0883)
Sample	Housing sample	Housing sample	Child sample

[a] Notes. Standard errors in third column are corrected for multiple children in same household.

likely to have been held back than children in other rental accommodation, though this finding is significant only at the 90 percent level of confidence.

The other demographic variables included in these models have the expected signs. Families whose heads are older, married, white, and better educated tend to have better outcomes, whether or not they live in projects. The child age dummies are individually statistically significant and pick up the pattern of classification error discussed above and documented in Appendix A: for example, the estimated probability of being held back rises sharply between the ages of 11 and 12, and then falls again until the child reaches age 16.

As discussed above, there is some controversy in the literature about whether sex composition is a valid instrument for educational attainment, at least for girls. When we restrict the sample to boys only, the estimated reduction in the probability of being held back is −0.18 with a standard error of 0.10. For girls, the

corresponding coefficient and standard error is −0.06 and 0.10. Thus, it appears that the beneficial effects of projects on schooling attainment are confined to boys.

We also repeat our analyses for the subsample of children for whom 'held back' is defined most similarly (8-, 12-, 16-, and 17-year-olds). The estimated effects of projects on the probability of being held back are exactly the same in this subsample, although it is significant only at the 87 percent level of confidence.

We explore the robustness of our estimates to some additional changes in specification in Table 5. Panels A and B show that our results are not sensitive to the exclusion of variables measuring family structure and marital status or to the inclusion of measures of family income. The point estimates and standard errors in these panels are very similar to those reported in the main tables. Panels C and D show that the effects of public housing are largest when the head has low educational attainment, as one might expect if our estimates are really picking up the effects of housing programs. Similarly, panels E and F show that the effects are larger among those of low income than among those of higher income, where, again, the estimated effects are not statistically significant.

Panel G shows estimates computed including all families with between two and six children. Briefly, the first stage results are weaker than before, and we think there is good reason for this. Among families with three or five children, our sex-composition instrument has no explanatory power, since, for example, a family with three children will be entitled to three bedrooms no matter what the sex composition of the children is. Also, in its administrative records, HUD does not even break out project units with more than three bedrooms as a separate category, leading us to believe that there are few units of this type. Thus, our instrument may have little power for large families who are constrained by the lack of large apartments in projects. In fact, we found that the first stage was extremely weak for families with three to six children, suggesting that the two-child families (56 percent of families with two or more children) are driving the results. Despite these problems, which lead us to prefer the estimates based on two-child families, the TSIV estimates are qualitatively similar, though somewhat less precisely estimated, than those estimated for two-child families.

Finally, Table 6 shows both OLS and TSIV results estimated separately by race. These estimates can be compared to those that appear in Tables 2 and 4. We find using the SIPP data that relative to non-project residents of the same race, black project residents are more dissatisfied with their homes and neighborhoods than whites are. This observation is consistent with the observation that blacks tend to be over-represented in the worst projects, as lawsuits in many cities have alleged. On the other hand, white project children are much more likely to have changed schools than other white children, while this is not the case for blacks.

The TSIV estimates suggest that rental payments increase more with project residents for whites than for blacks, which is also consistent with the idea that white projects are of higher quality than those inhabited predominantly by blacks. Overcrowding falls by a similar amount among blacks and whites, while density is

Table 6
Results from SIPP using OLS[a]

	OLS results from SIPP for whites					
	Rating of home	Rating of neighborhood	Rating of school	Child ever held back	Changed schools	Extra-curricular activities
Participation in public housing	−0.0129 (0.4492)	−0.7176 (0.4715)	0.3336 (0.5619)	−0.0887 (0.0722)	0.3879 (0.1084)	−0.0117 (0.1016)
Number of observations	1131	1131	1085	1259	1250	1299
Mean of outcome	7.951	7.637	7.288	0.098 (0.008)	0.386 (0.014)	0.678 (0.013)
	TSIV results from Census for whites					
	Rental payment/1000	Family is overcrowded	Dense building	Child was held back		
Participation in public housing	0.6899 (0.1218)	−0.1659 (0.0805)	−0.0564 (0.0756)	−0.0488 (0.1352)		
Number of observations	59,444	187,596	187,596	228,355		
Mean of outcome	0.564	0.0091	0.0085	0.0362		
	OLS results from SIPP for nonwhites					
	Rating of home	Rating of neighborhood	Rating of school	Child ever held back	Changed schools	Extra-curricular activities
Participation in public housing	−0.6070 (0.3081)	−1.2823 (0.3325)	−0.5370 (0.3310)	−0.0113 (0.0457)	0.0413 (0.0702)	−0.0374 (0.0714)
Number of observations	647	647	621	750	739	781
Mean of outcome	7.622	7.013	7.326	0.107 (0.011)	0.444 (0.018)	0.470 (0.018)
	TSIV results from Census for nonwhites					
	Rental payment/1000	Family is overcrowded	Dense building	Child was held back		
Participation in public housing	0.2197 (0.0658)	−0.1541 (0.0962)	−0.2518 (0.0675)	−0.1886 (0.0854)		
Number of observations	57,457	91,533	91,533	111,726		
Mean of outcome	0.4852	0.0964	0.0491	0.0426		

[a] Notes. The SIPP regressions include the same covariates as Table 2, and the Census regressions include the same covariates as Table 4.

reduced significantly among blacks, but not among whites. Finally, there is a striking racial difference in the effect of projects on grade repetition among blacks and whites. For whites, projects have no significant effect, while for blacks, living in projects is estimated to reduce the probability of grade repetition by 19 percent.

Thus, these estimates paint a mixed picture of the housing available to low-income blacks. On the one hand, black project residents are more dissatisfied with their homes and neighborhoods than are white project residents. On the other hand, project units may well be of higher quality than those that would otherwise be available to poor black families, and there is evidence that grade repetition is lower among black project children than among other similar children.

6. Discussion and conclusions

Although it is widely assumed that public housing projects are bad for children, there is little empirical research on this question. A likely reason is that there are few large data sets that combine information about project participation, housing quality, and child outcomes. In this paper, we combine information from several sources in order to take a first look at the effects of project participation on housing quality and on educational attainment, a very important child outcome.

In view of the negative image of public housing projects, our results are surprising. While the correlation between project participation and the outcomes we examine is negative, we conclude that this is mainly due to unmeasured characteristics of project participants. When these characteristics are controlled for using TSIV techniques, our point estimates suggest that projects actually have positive effects on both housing quality and children's academic achievement.

These results do not imply that the recent shift away from projects is misguided. It is possible, for example, that these same children would be better served by a voucher program.[20] But they do suggest that projects as a group have been wrongly vilified. Atlas and Dreier (1993) point out that ''Public housing seems to many Americans a metaphor for the failures of activist government . . . '', but perhaps they are correct that in reality ''the best kept secret about public housing is that most of it actually provides decent affordable housing to many people''.

One important limitation of our work is that we are unable to assess the effects of participation in projects on neighborhood quality because the Census Public Use Samples do not contain Census tract or county identifiers. Linking geographic information of this kind to our data would allow a more direct test of hypotheses about the relationship between housing projects and neighborhoods. A second limitation stems from the relative crudity of our indicators of housing quality and

[20]This question is the subject of ongoing research (cf. Katz et al., 1997).

child well-being. We hope that future research using better data will be able to pin down the benefits of projects more precisely.

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Appendix A. Census definition of held back in the Census, and probability of being classified as held back by age

Age	6		7		8–11		12–15		16		17	
Held back if:	<Nursery school		<Kindergarten		<Grades 1–4		<Grades 5–8		<Grade 9		<Grade 10	
Age	6	7	8	9	10	11	12	13	14	15	16	17
% held back	0.0845	0.0196	0.0451	0.0192	0.0039	0.0033	0.0657	0.0234	0.0108	0.0055	0.0887	0.1161

The 1990 Census PUMS lists the following categories for educational attainment: 0, N/A (less than 3 years old); 1, no school completed; 2, nursery school; 3, kindergarten; 4, 1st, 2nd, 3rd, or 4th grade; 5, 5th, 6th, 7th, or 8th grade; 6, 9th grade; 7, 10th grade; 8, 11th grade; 9, 12th grade; 10, high school graduate, diploma, or GED; 11, some college, but no degree; 12, associate degree in college, occupational program; 13, associate degree in college, academic program; 14, bachelor's degree; 15, master's degree; 16, professional degree; 17, doctorate degree.

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