This report presents preliminary morbidity data from a study of the effects of improved housing on physical and mental health. Eighteen months of rehoused experience reveals no gross difference in physical morbidity between test and control families; possible explanations are offered.

THE EFFECTS OF HOUSING QUALITY ON MORBIDITY

PRELIMINARY FINDINGS OF THE JOHNS HOPKINS LONGITUDINAL STUDY

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OUR PREVIOUS papers reported the general plans for the longitudinal study of the effects of housing quality on physical and mental health and social adjustment of 1,000 families in a large eastern city.[1, 2] We also outlined some of the problems met in organizing and carrying out a study such as this that depended for its success on such matters as the proper matching of test and control groups, the exercise of control over the quality of the data being collected, and on protecting the sample from attrition over time.

In the present paper we describe briefly our method, indicate how successful our matching has been, and present the basic preliminary test-control morbidity findings for the first 18 months of rehoused experience of the test families. We discuss the meaning of these data and indicate what secondary analysis is being pursued at present. We conclude with data on the losses incurred to date in our sample, the attendant possibilities of bias in these losses, and some facts concerned with the unanticipated high moving rate of our control families.

Selecting Test Families and Matching Control Families

From April, 1955, to March, 1956, the newly constructed housing development with which we are concerned was occupied gradually by approximately 800 families who moved from housing of generally poor quality. The plans for the study called for the selection of 400 of these families as a test group. This was accomplished through the co-operation of the local Housing Authority. As lists of successful applicants were drawn up, month after month, the study staff selected a proportion, usually half, plus a safety factor of 20 to 30 per cent, as test families. A number of criteria were used for inclusion of a listed family in the test group, the principal ones being (a) the likelihood of finding matching families from our control reservoir and (b) the probability that once chosen the possible controls would remain in the control group. As the test families were chosen, interviews were conducted in the home to obtain initial measures of housing quality, morbidity, and adjustment. It is important

to note that initial measurement was made prior to the move to the new housing, and in fact prior to the Housing Authority's notifying the family that an apartment was available.

Control families were chosen from the large residue of applications on file at the Housing Authority. While selection of the test families was going on, control families were selected, and matched as well as possible pair by pair with the individual test families in terms of 13 variables which we believed played a role in health and adjustment. The initial matching variables were limited to demographic items appearing in the applications on file. The plans for the study called for 600 control families, some test families receiving single matches, and some, double matches. Initial measurements were made on the control families using instruments identical with those administered to the test families.

At the time of the first of our series of "after" measurements in April and May, 1956, the study sample consisted of 396 test families who had moved to the housing development from housing of generally inferior quality and 633 control families who were still occupying generally inferior housing.

The initial interview was intended first, to obtain exhaustive information about the initial comparability of our two groups, not only on demographic variables but also on the dependent variables themselves; and second, to establish certain base lines from which to measure changes. The initial interview included, therefore, a housing quality inventory adapted from the APHA Appraisal Method; a morbidity survey in which we enumerated on a large fold-out form conditions currently affecting the family as well as certain items of the medical history; and an adjustment inventory which explored such issues as relations within the family, relations with

neighbors, morale, and other matters relating to personal psychological state.

Initial Comparability

Background Characteristics—Table 1, in greatly abbreviated form, shows how the test and control groups compared initially with respect to a number of demographic and other background variables. In general, the two groups show very close correspondence on such matters as age, usual activity, size of family, income, public welfare assistance, marital status, veteran status, education of male and female heads of household, and original date of housing application.

Initial Housing Quality—Because of the basic hypothesis of the study, it is also important to know whether the two groups were initially comparable in matters related to the quality of housing before the test group moved to better housing. Table 2 summarizes selected items from our housing quality inventory. The general correspondence of the two groups is, again, quite close on such items as water supply, the location and sharing of toilet and bath, heating, plumbing, and rodent infestation.

Initial Morbidity—During the initial (before) interview, we obtained data regarding items of the medical history prior to the first visit. On such items (not shown in the tables) as reports of prior surgery, hospitalization for tuberculosis, the use of preventive medical facilities in the six months prior to the interview, and for five communicable diseases of childhood, the two groups are closely comparable.

During the initial interview and again in the first "after" interview—that is, wave 1 and wave 2, respectively—we also gathered information about chronic conditions present among persons in our sample during the two years preceding wave 2. Table 3 shows similar rates for

Table 1—Selected Demographic and Other Background Characteristics: Initial Comparability of Test and Control Families

			Test	Control*
	a. Person Data:	N =	(1,828)	(2,977)
			Per cent	
(1) Age of persons	Under 5		30.9	30.5
	5–19		34.5	35.8
	20 and over		34.6	33.7
(2) Usual activity of all persons	Works full or part time		17.5	19.5
	Keeps house		15.8	14.0
	School		31.9	31.3
	All other (preschool, etc.)		34.8	35.2
	b. Family Data:	N =	(396)	(633)
(3) Size of family	2–4 persons		48.9	49.1
(4) Income	Under $2,000		38.1	38.6
	$2,000–$2,499		25.5	21.1
	$2,500 and over		36.4	40.3
(5) Receiving welfare	Yes		22.0	23.0
(6) Marital status (female)	Married now		62.3	61.7
(7) Veteran status	Veteran family		28.7	31.1
(8) Education at most 9th grade	Wife		57.8	62.6
	Husband (per cent where present)		55.1	55.2
(9) Original application date	1952, or earlier		36.9	32.2

* Control per cents and rates in all tables were adjusted because of the two control families per test family in about half of our matched "pairings." In effect, these double-control families were each given a weight of 0.5.

the test and control groups not only for total chronic conditions, but also for most specific diagnostic categories, of which selected items are shown.

Table 4 shows episodes of illness during the two months preceding the initial interview and includes total conditions (all diagnoses) and two individual diagnostic categories. Comparison of the first two columns of this table reveals that for total conditions the rate for the control families is considerably higher than the rate for the test families. However, the difference is explained by the fact that the season of the initial interview varied considerably between the two groups of families.

When the seasonal difference is taken into account by appropriate weighting,

Table 2—Selected Housing Quality Characteristics: Initial Comparability of Test and Control Families

Housing Quality	Test	Control*
	Per cent	
Hot and cold running water in apartment	72.2	75.6
Bathtub in the apartment	71.2	71.2
Share bathtub with nonfamily persons	41.9	37.0
Toilet in the apartment	80.8	82.5
Share toilet with non-family persons	48.0	43.1
Central heating	53.3	50.4
Leaks in pipes or plumbing	25.3	21.0
Rats in the apartment	27.0	21.3
Number of families	(396)	(633)

* Control per cents weighted.

Table 3—Chronic Conditions Present During the Two Years Preceding Wave 2 as a Rate per 1,000 Persons Surveyed: Initial Comparability of Test and Control Families

Chronic Condition:	Test	Control *
Number of Persons Surveyed	(1,828)	(2,977)
Total Chronic Conditions	517.5	537.0
Tuberculosis	8.2	5.7
Venereal disease	4.4	5.5
Asthma	17.0	18.8
Diabetes mellitus	5.5	3.7
Diseases of nervous sys-		
Diseases of the blood	23.0	17.5
tem and sense organs	48.7	50.4
Diseases of the circu-		
latory system	96.8	108.6
Bronchitis	19.1	22.5
Hernia	21.9	17.0
Diseases of skin and		
cellular tissue	27.4	27.7
Diseases of bones and		
organs of movement	41.6	35.3
Impairments	170.7	193.5

* Control rates weighted.

the difference between test and control groups effectively disappears. This is shown by comparing the test group rates in Table 4 with the adjusted (for season) rates of the control group.

Other Initial Data—The final kind of data which throw light on the comparability of test and control groups is derived from initial measures of personal and family adjustment. A prelimi-

nary look at the adjustment data reveals that for the vast majority of items, the same similarity exists between the two groups as has already been described in connection with background information and initial housing quality.

Summary of Initial Comparability—Our present impression is that extensive efforts have produced two apparently well matched groups of families. It is possible that this evaluation may change when we have constructed indexes of initial comparability, comprising a number of items taken together. In that event we will have to take initial characteristics into account when comparing rates of diseases between our groups in the after-period. In the meantime, we will proceed to examine the "after" morbidity information without serious concern about initial uncomparability.

Preliminary "After" Findings

The "after" period of the study began in the spring of 1956. By that time, the test families had been living in the new housing development for about nine months on the average, the range being from approximately two months to a year. Since that time, all families in both groups have been interviewed once every ten weeks. At each wave of interviewing we conduct a morbidity interview, and, in addition, varying by

Table 4—Episodes of Total and Selected Illness During the Two Months Preceding Wave 1 as a Rate per 1,000 Persons Surveyed: Initial Comparability of Test and Control Families

Diagnosis	Test	Control *	Control * (Adjusted for Season)
Number of Persons Surveyed	(1,828)	(2,977)	(2,977)
All diagnoses	1,040.5	1,165.6	1,060.5
Diseases of the respiratory system	281.2	388.9	299.5
Diseases of the digestive system	145.0	141.5	130.5

* Control rates weighted.

wave, a diet survey, or an interview dealing with matters of adjustment.

The morbidity interview is conducted by lay interviewers trained by the study staff. The interviews are conducted with the female heads of households, who report for each family member in turn on episodes of illness, aftermaths of disability, and medical attention received. One feature of our every-wave morbidity survey form is the use of a small check-list of six relatively minor conditions including colds, digestive disturbances, and home accidents. A second feature is an inquiry about specific chronic conditions that have been enumerated in preceding waves. The questioning covers the "past two months," that is, the two months prior to the date of the interview. In classifying the conditions responsible for episodes of illness, we use as a guide the sixth edition of the "Manual of the International Statistical Classification of Diseases, Injuries and Causes of Death," with a few adaptations and recombinations made necessary by the nature of the primary information.

The preliminary data are presented wave by wave for the first five afterwaves of morbidity interviewing. The period corresponds to the first 18 months of rehoused experience for the test families. Topics covered are episodes of illness by age (an episode being a condition "bothering" a person one or more times during the two months preceding the survey), persons experiencing one or more days of disability, and persons medically attended.

Part (a) of Table 5 shows the rates of episodes of illness of all degrees of severity. Rates of episodes among test families are higher than those among control families during waves 2 and 3. During the next three waves, the situation is reversed, control families having the higher rates. However, each difference is within the acceptable range of sampling error.

Examination of the specific diagnostic categories (not shown in the table) also reveals generally small test-control differences wave by wave. The absolute levels of the rates reveal that respiratory and digestive conditions make up approximately half the episodes occurring in each wave.

Examination of the same data by age in Table 5 reveals the familiar U-shaped distribution, highest rates in each wave occurring in the younger and older ages. For the age category 35 and over, test rates are consistently (but not significantly) higher than control rates. However, in the age ranges 5–9 and 10–19, a tendency appears, beginning with wave 4, for test rates to be lower than control rates.

Part (b) of Table 5 shows the per cent of persons in both samples who have experienced one or more days of disability in the periods covered. Disability includes the categories "in bed" and "kept from usual activities." For these data the test-control differences are small, the wave-by-wave pattern resembling that of episodes of illness in part (a) of the table.

Part (c) of Table 5 gives the per cent of persons medically attended in each wave. In all but wave 4, the test group has a small excess of medically attended persons over the control group. Analysis by age (not shown) reveals that the age groups from 10 to 19, and 35 and over play a role in this "reversal" of expectancy; the situation among the latter group corresponding to the analogous finding regarding rates of episodes of illness among older persons.

Our general impression is that for the 18-month rehoused period under examination the move to good housing has not on the average resulted in measurable improvement in rates of episodes and related matters for the test group as a whole over the control group. It may be that as the study progresses, further basic analysis will confirm this tentative judg-

Table 5—Morbidity During the Two Months Preceding the Survey, by Wave

		Wave				
		2	3	4	5	6
Date Wave Began:		4/9/56	6/18/56	8/27/56	11/5/56	1/14/57
No. of Persons Surveyed:	Test	(1,888)	(1,893)	(1,915)	(1,883)	(1,891)
	Control	(3,018)	(3,014)	(2,961)	(2,922)	(2,893)
(a) Episodes of Illness* as a Rate† per 1,000 Persons Surveyed, by Age						
All ages	Test	1,341.7	1,114.6	891.4	970.2	910.6
	Control	1,321.9	1,018.9	960.1	993.1	959.6
Under 5	Test	1,194.5	1,023.5	723.1	862.1	860.6
	Control	1,125.3	952.1	821.4	884.2	868.2
5–9	Test	1,057.4	848.9	547.5	672.6	621.6
	Control	1,061.7	723.0	594.0	787.2	760.9
10–19	Test	1,059.8	752.1	673.9	762.8	668.2
	Control	1,030.4	689.9	717.9	800.8	820.5
20–34	Test	1,634.4	1,396.5	1,254.9	1,138.5	1,105.5
	Control	1,709.7	1,314.5	1,374.6	1,200.0	1,116.2
35 and over	Test	2,156.6	1,836.4	1,701.9	1,956.0	1,630.6
	Control	2,008.0	1,633.7	1,607.8	1,620.7	1,551.4
(b) Persons Experiencing One or More Days of Disability, as a Per cent† of Persons Surveyed						
	Test	29.6	23.7	17.7	20.7	21.4
	Control	28.1	20.3	20.2	21.8	23.0
(c) Persons Medically Attended, as a Per cent† of Persons Surveyed						
	Test	21.9	19.7	15.7	18.4	17.2
	Control	20.7	16.1	17.9	17.2	15.7

* Includes symptoms.
† Control rates and per cents weighted.

ment. In that case, the first possibility that comes to mind is that if housing quality has measurable effects at all and if our instruments are sensitive enough, it may take longer than 18 months for the effects to emerge. Another possibility is that housing quality already has had effects in the expected direction, but that a number of factors obscure these effects. Let us consider several modes of analysis being pursued at present to explore each of these possible obscuring factors.

Additional Analyses Planned

The first factor has to do with special aspects of the particular housing develop-

ment in which the rehoused families live. As regards facilities traditionally considered important—such as water, heat, kitchen and toilet—the rehoused families have in general fewer deficiencies than the control families. However, several aspects of the housing development may operate in a contrary direction for particular health matters such as the communicability of certain diseases. For example, a majority of the test families live in 11-story, 110-family buildings served by two elevators. Because of these so-called high-rise buildings, the density of persons per square foot of ground area is much higher for these residents than for the controls and their neighbors. A possible consequence

is heightened opportunity for aerial transmission of certain diseases. One kind of analysis is to examine the episode rates of families living in high-rise buildings versus those living in low-rise, two-story buildings, for certain conditions such as upper respiratory and childhood communicable diseases. The low-rise buildings more nearly approximate the average slum block in density.

A second factor possibly obscuring housing quality effects has to do with the temporary and permanent losses incurred during each wave. The general loss picture is satisfactorily low, due to the extraordinary efforts exerted by the field staff in this direction. Through wave 6, permanent losses average less than 1 per cent of (the original sample) each wave. However, the question arises concerning the extent of bias in these losses. If families lost among the controls, for example, are composed of persons more prone to sickness, and those among the test group less prone to sickness, this would tend to obscure the effects of housing quality.

A possibly more serious source of similar bias occurs because of moves among the control families. It should be noted that we are continuing to follow up in ordinary fashion every family that moves, obtaining, in addition, complete housing information for each new place of residence. While only 6 per cent of the test families have moved from the housing development through wave 6, the more astonishing fact is that some 52 per cent of the control families have moved from their wave 1 place of residence. One-fifth of these have moved to some project under the Housing Authority's management and thus to dwelling units of clearly good quality. Furthermore, tabulations of the "new" and "old" housing quality of the controls who moved about in the city also show sizable improvement in housing quality among these movers as well. The consequence is, first, that the in-

dependent variable difference is possibly being narrowed markedly due to "natural" causes, and second, there arises the strong possibility of bias in the personal characteristics of the movers themselves and in their morbidity experience.

We are at present analyzing both "loss" and "move" data to establish possible bias, and if bias has arisen, the possible lessening of observable effects of housing quality that may have occurred.

Finally, there is another mode of analysis now under way which will throw light on the likelihood that certain subgroups among the test families have already profited substantially from the rehoused experience to date even though others may not. The initial housing quality of the test and control families was of generally poor quality, but not homogeneously so. Some dwelling units and structures had many deficiencies and some only few, although whatever the quality of test family residence, we tried to match similarly for the paired control. It seems reasonable to believe that the test families at the lower end of the housing quality scale were the ones most likely to profit from the move to good housing. A thorough test of this supposition awaits construction of indexes of housing quality in which we combine, largely according to the APHA Appraisal Method weighting scheme, many elements of housing quality.

The Home Interview as a Method of Collecting Data

Finally, we wish to touch upon the procedures being followed in collecting data, that is, the personal interview conducted in the home by a nonmedical interviewer. If the method is excessively crude or open to uncontrolled bias, then even if the true change in morbidity due to housing quality alteration were great during the period covered by waves 1 through 6, the change might go unde-

tected. The question of method is especially salient in the light of recent findings that cast doubt on the sensitivity of the home interview when compared to the outcomes of clinical examinations, the main import being that there is apparently serious under-reporting of chronic conditions in the home interview.[3, 4] However, the possibility that under-reporting occurs at all, or to the same degree in connection with acute conditions, still needs confirmation.

The issue of insensitivity was considered at the outset of the present study, while arriving at a decision as to the method of data collection. Some desired data, we became convinced, could not very well be obtained other than by some sort of interview: for example, occurrence of minor acute conditions, and an account of exacerbations of chronic conditions. Our attention to minor episodes was, of course, due to the reasonable a priori opinion that in a three-year period of rehoused experience, it was unlikely that test-control differences would lie mainly among major and serious complaints. We could not reasonably expect, for example, that there would emerge in our sample a significant test-control difference in new cases of tuberculosis. Our conclusion was to use a bigger, and perhaps coarser net.

It was necessary to consider measures which would give assurance that, even though the net is coarse for the test families, it must not be coarser for the control families. To achieve this end, we introduced a number of quality-control measures that help to minimize bias possibly arising from our method of data collection. These quality-control devices are as follows:

1. The senior staff of the study is in direct charge of field work, and is responsible for all the hiring, training, and supervision of the interviewers.

2. More than ordinary attention is paid to the training and preparation of interviewers. Two weeks of intensive training with our morbidity instruments, including observed interviews in nonsample homes, precede independent interviewing, regardless of prior nonstudy interviewing experience. There is refresher training for all interviewers three days prior to the beginning of each wave. Each interviewer is observed in the field in actual interviews at least once a week throughout a wave, the proper ratio of control to test observations being maintained. The results of these observations are made known to interviewers the day of the observation, and in more general terms, during regular, weekly interviewer staff meetings.

3. After the regular interview wave, one quarter of the families are reinterviewed with an abbreviated questionnaire, to enable us to check on the performance of interviewers when they are not under direct staff observation.

4. Twice during each wave, hand tabulations are made of the number of illness conditions enumerated by each interviewer. The purpose of these "interviewer variability" tabulations and of the reinterviews mentioned above is the detection of deviant interviewers with a view to making over-all interviewing procedures more uniform.

5. To prevent interviewer variability from introducing bias in either group, assignment of interviews is done in such a way that during a wave, each interviewer conducts both test and control interviews, the proper ratio of test to control being maintained for each interviewer.

Summary

We have presented preliminary morbidity data from the Johns Hopkins study of the effects of housing quality on physical and mental health. The study design includes initial measurement of two groups of families, one of which moved from dwellings in slum areas to a newly constructed public housing development, the other group remaining in the slum at least at the outset of the study.

Control families were selected and matched as well as possible with test families, once the latter were designated. Data have been presented regarding a number of background characteristics such as age, usual activity, size of family, income, education; regarding initial housing quality such as water supply, kitchen and bath facilities; and regard-

ing initial morbidity. The impression from the item-by-item comparison of the two groups is that they are well matched on many variables suspected of being related to the dependent variables of the study. We have also presented test-control morbidity data for less than half of the "after" period of the study, corresponding to the first 18 months of re-housed experience of the test families. These findings in general reveal no gross test-control differences attributable to the difference in housing quality through this period.

Several factors have been discussed that may tend to obscure test-control differences, and plans are now under way to explore these factors. A final section is devoted to measures for quality control which tend to insure lack of bias in the method we are using to collect the morbidity information.

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Report of St. Louis Study Available

"Public Health and Hospitals in the St. Louis Area: A Mid-Century Appraisal" is the study of the St. Louis Health Services completed in 1957 by the American Public Health Association with the help of an impressive battery of consultants in each of nine major health areas. This 438-page volume has been described as potentially having "a positive effect on the development of planned community health services throughout the nation" (A.J.P.H. Aug., 1958, p. 1068). As such it has value for many others besides the St. Louis community. Thus it is being made available generally for purchase. Social Planning Council, 417 North 10th St., St. Louis 1, Mo.; $3.75 including postage and handling charges.