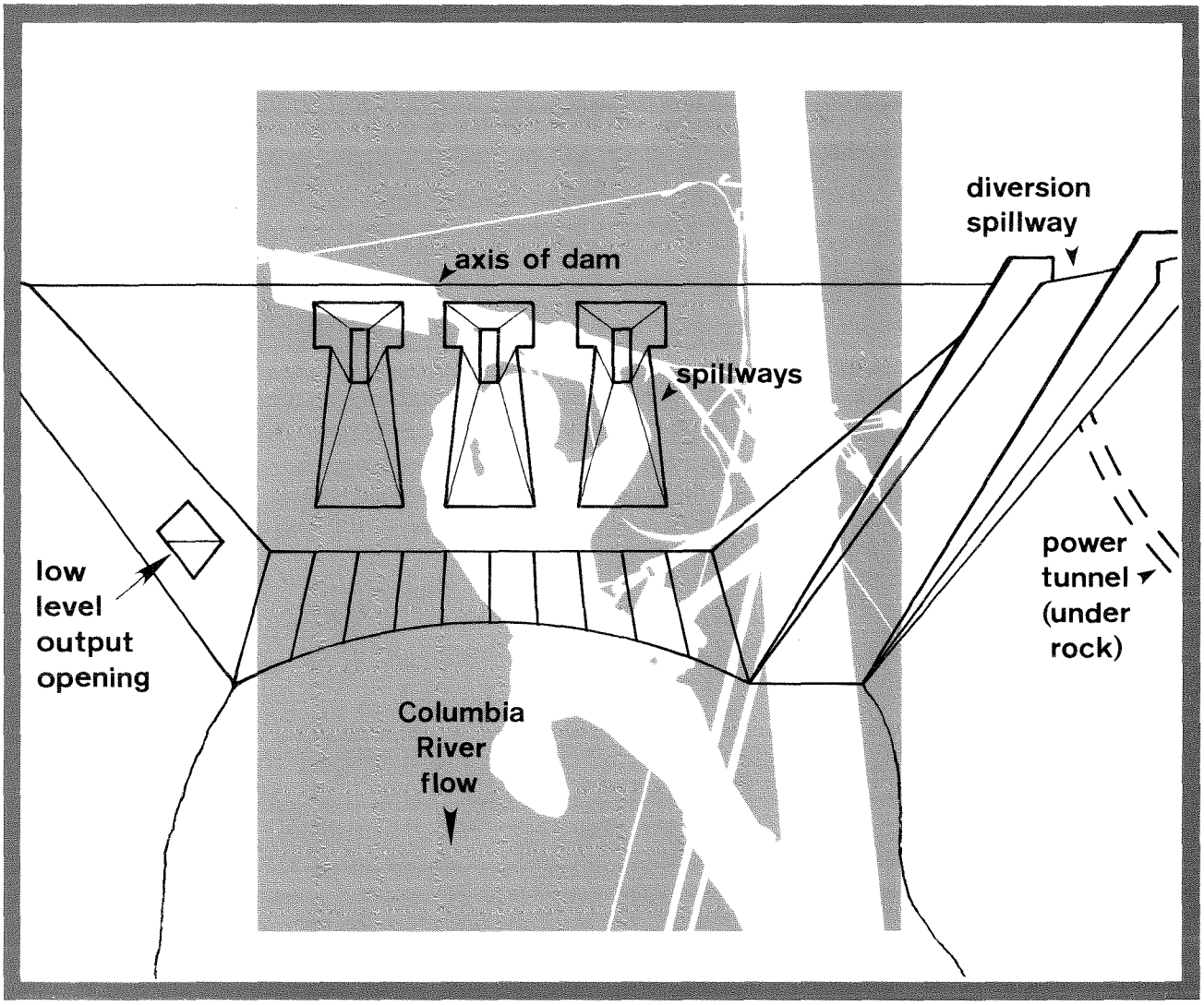
FEDERAL RESERVE BANK
OF SAN FRANCISCO

ECONOMIC REVIEW



STUDIES IN LABOR MARKETS AND UTILITY PRICING

WINTER 1980

Welfare and Youth Unemployment: Evidence From a Controlled Experiment

Randall J. Pozdena*

Joblessness among the young accounts for a substantial portion of total unemployment in the United States. In 1978, approximately half of all unemployed workers were less than 25 years old. Teenagers alone were responsible for more than one-fourth of total unemployment. In 1978, when the overall unemployment rate averaged about 6 percent, the teenage unemployment rate topped 16 percent.

Since unemployment rates critically influence the conduct of macroeconomic and labor-market policy, economists have come to realize the necessity of identifying the origins of the high and growing rates of youth unemployment that the nation has experienced in recent years. To this end, numerous studies have attempted to identify factors contributing to the adverse performance of youth in the labor market, and to determine how much of their unemployment represents a serious problem for our society.[1]

An individual is classified as unemployed if he is seeking part-time or full-time employment but is not currently working. Much of the discussion concerning youth unemployment has focused on developments which may have adversely affected the availability of work—i.e., the *demand* for youthful labor. Several studies, for example, have identified minimum-wage legislation as a major contributor to measured youth unemployment, because it raises the cost of unskilled labor and thereby reduces the demand for such labor.[2] A study by James F. Ragan suggests that as much as 4 percentage points of the increase in youth unemployment between 1966 and 1972 can be traced to increases in the minimum wage and to extension of its coverage.[3]

Other economists have suggested that the movement of industry away from the central city has made job opportunities less available to the heavy concentrations of poor urban youth. This may particularly have affected the demand for labor of minority youth.[4] Trends in general macroeconomic conditions also may have weakened the demand for youth labor. Richard Freeman concludes, for example, that employment of youth is very sensitive to the industrial composition of jobs and the general condition of the economy. Areas with heavy trade and service employment and rapid economic growth tend to have better youth employment opportunities than elsewhere.[5]

Although these and other factors affecting labor demand have undoubtedly contributed to youth unemployment, *supply-side* influences can also play a significant role. Changes in attitudes or changes in family economic circumstances over the last several decades may have reduced young people's willingness to

*Economist, Federal Reserve Bank of San Francisco. The research reported herein was performed while the author was associated with SRI International pursuant to contracts with the states of Washington and Colorado, prime contractors for the Department of Health, Education, and Welfare under contract numbers SRS-70-53 and HEW-100-78-0004, respectively. The opinions expressed in the paper are those of the author, and do not necessarily represent the opinions of the states of Washington or Colorado, any agency of the U.S. Government, or SRI International. Dr. Arden Hall of SRI was a major contributor to the research that generated this paper. The author also gratefully acknowledges the computational assistance of Gretchen Wolfe and Fred Dong and the support of Dr. Robert Spiegelman, Project Director.

supply their labor. Such changes may increase measured unemployment if they reduce young people's willingness to accept or keep available jobs even though they continue to report an interest in finding work.

In this view, youth find jobs available, but their unrealistic wage expectations or their preference for leisure cause more casual or protracted search for employment, thereby generating increases in measured unemployment. Among the factors possibly contributing to such behavior is family welfare assistance. Although the concentration of youth unemployment in areas with traditionally large welfare populations—such as central cities—is suggestive of a link between welfare and unemployment, there are problems in establishing a strong empirical case for such a relationship. When using traditional aggregate data or data from panel surveys, it is difficult to control for all of the factors which may influence behavior. Also, it is difficult to disentangle cause and effect: families may be on welfare *because* of the unemployment of their members and not the other way around.

This paper investigates the relationship between family welfare assistance and youth labor-market behavior using data from a controlled welfare experiment. Welfare policy evaluation increasingly depends on experimentation, because the economic environment of the subjects can be experimentally manipulated and thus the direction of cause and effect can be made clear. In addition, the availability of a control group permits the impact of a welfare program to be isolated from other effects (such as changing macroeconomic conditions) which might affect labor-market behavior as well. (Appendix A contains a discussion of the use of experiments in economic analysis.)

The results demonstrate the potential importance of supply-side factors in determining the labor-market behavior of youth. Although the specific data used here are not ideally suited to direct measurement of unemployment rates, the results are suggestive of an association between youth unemployment and welfare assistance. After a brief discussion of the theoretical link between these variables, the paper describes the experiments that generated the data used in the research, as well as the method of empirical analysis. The paper concludes with a discussion of the experiment's results and their policy implications.

I. Welfare and Youth Unemployment

Why should family welfare assistance influence youth labor-market behavior? To understand the circumstances under which such a linkage might exist, we must (1) review the effects of a welfare program on the economic environment of the family, (2) discuss how the effects of family assistance are transmitted to youth, and (3) examine the likely response of youth to these stimuli. Each of these points has received considerable attention in the literature.[6] Rather than attempt a survey of earlier work, we simply summarize below the key implications of earlier theoretical work.

Although the specific characteristics of existing and proposed welfare programs vary considerably, most have a number of features in common. An obvious common feature is the provision of non-wage income to recipient families through support or benefit payments. By supplementing earned income, welfare programs expand the budgetary opportunities of the affected family. A second feature of family-assistance plans is their incorporation of procedures to "phase out" support as the earned income of the family rises. This is accomplished by reducing the welfare benefits by a fixed fraction of additional earned income. In effect, this linkage of benefits to earned income is a "tax" on earned income, which reduces the perceived net wage associated with additional work. For example, in the original Aid to Families with Dependent Children (AFDC) program, this tax rate was (statutorily) 100 percent, because support was reduced

one dollar for each dollar of additional earned income. In effect, the perceived net wage on the margin was zero because of the implicit 100-percent tax.

Thus welfare programs influence both the income of the family and a major price variable—the net wage. In the case in which welfare support is received by a single individual (rather than a family), the implications of these features are straightforward. The support increases the non-wage income of the individual, and the tax provisions reduce the returns to additional work on the margin. As we will show later, the individual likely will respond to these changes by increasing consumption and reducing the amount of time worked, relative to someone for whom welfare is not available.

The implications of welfare support are much more complicated, however, when we consider that such support is typically directed at a *family* rather than at an individual. That is, although the head of the family is the formal recipient of any payments, the income of *all* of the family members typically enters into the computation of eligibility for support payments. A youth's response will depend upon the manner in which the features of the welfare program are transmitted to the youth through family decision processes.

A family may behave like a single decision unit, for example, maximizing family utility subject to a budget constraint which involves the earnings of all of the family members. In effect, the youth's working behavior is determined jointly with the working behavior of all of the other family members. A welfare program that provides support to the family and taxes additional family earnings will thus directly influence the behavior of all family members, since they are all interdependent.

Under a separate method of decision-making, however, the youth may view his income as his own, so that any change in his situation as a result of welfare is at the parents' discretion. In this case, the youth's perception of his income and net wage would not automatically be affected by the family's participation in a welfare program. But we could visualize some such effect if we make certain assumptions about the way in which parental assistance to their children is likely to be affected. For example, the parents could choose to subsidize the youth's leisure or schooling, as part of their own consumption decision in response to receiving welfare support.

These examples illustrate the difficulty of stating precisely how the features of family welfare programs are transmitted to a young worker. Let us assume simply that youth experiences—to some degree—a reduction in net wages and an improvement in non-wage income as a result of the welfare program. What are the likely effects of this changed economic environment on the youth's labor-market behavior?

The simplest way to conceptualize the effects is to picture the youth making the choice between work and leisure. The trade-off is an obvious one—each additional hour of leisure results in a loss of an hour's wages. Thus, the opportunity cost or "price" of leisure is the wage; in an environment in which taxes are levied on wages, the *after-tax* wage is the price of leisure.

A welfare program can affect this choice because it increases the individual's income and—because of support "phase out" provisions—*lowers* the after-tax wage and thus the "price" of leisure. Both of these changes should increase the demand for leisure (in lieu of work). This is because individuals tend to consume more of a good (like leisure) as their income rises or as the price of the good falls. In the case of a welfare program, both effects occur and reinforce one another.

This simple argument thus suggests that a welfare program will tend to *decrease* the willingness to work on the part of the affected individuals, everything else being equal; that is, it will cause a reduction in labor supply. However, this does not lead to clearcut inferences concerning the effect of welfare on the *unemployment rate*. An increase in that rate requires withdrawal from work without an offsetting withdrawal from the labor force. The simple labor-supply model presented above cannot distinguish between these two effects.

Since the forces which are likely to reduce employment are also likely to reduce labor-force participation, it is not possible *a priori* to determine which effect dominates even with a more complex model.[7] Thus, although it is fairly clear that a welfare program will tend to reduce labor supply, the net effect of welfare on unemployment is theoretically ambiguous.

In sum, theory suggests that young people will reduce work in response to a welfare program if family decision processes cause them to experience a reduction in net wages and an increase in non-wage income. But the reduction in work effort may or may not increase *measured* unemployment—whether unemployment rises or falls when welfare benefits rise is an empirical issue. This paper sheds light on this issue by using data from a welfare experiment. We examine the effects of welfare support separately on the labor-force participation and job-taking behavior of youth, to provide some insight into the possible effects of welfare on unemployment. The results, as we will see, suggest that youth do respond to their family welfare situation in a way which could increase measured unemployment.

II. Welfare Experiments

Interest in experimenting with alternative welfare systems has been prompted by several criticisms of the AFDC program, the primary component of the U.S. welfare system. First, although AFDC is a Federal program, it is administered by the states, and the provisions vary state-by-state depending upon the ability and willingness of individual states to provide welfare support. Large interstate differentials in program payments have been criticized as inequitable. Second, some states restrict AFDC support to families which do not have a father present. This has led to the charge that AFDC encourages the breakup of families. Third, AFDC benefits are reduced sharply as earned income rises. This high rate of "taxation" of additional family earnings has been criticized as a work disincentive. Finally, AFDC has been criticized for not being generous enough, on the grounds that a wealthy society should do more for its poor members.

The Seattle and Denver Income Maintenance Experiments (SIME and DIME respectively) were designed to test a welfare program addressing these criticisms. The experimental program was called a "negative income tax" (NIT), reflecting the view that welfare support should be a logical downward extension of the positive tax system. However, these NIT experiments and the traditional welfare programs differed in detail rather than in concept. Specifically, the NIT embraced all households (husband-wife households as well as female-headed) and was designed to be more generous than the typical AFDC program.

Approximately 4,800 families with below-median incomes in Seattle and Denver became involved. As in a scientific experiment, some of the subjects received experimental "treatment" while some served as the control group. Those in the treatment group were eligible to receive support under one of eleven NIT programs. The control group was not eligible to receive any support through the NIT, but control families were free to enroll in the AFDC programs existing in their states. This control group was the benchmark against which the response of the treatment group to the NIT programs was measured.

The eleven experimental NIT programs of SIME/DIME represented different combinations of welfare-support levels and tax rates on earned income—the program feature which determines the rate at which benefits are reduced as earned income rises. The support level was between $3,800 and $5,600 (in 1971 dollars) for a family of four. The "phase out" tax rate was between 50 and 80 percent;[8] a family on a program with an 80-percent tax rate, for example, would lose 80 cents of support for each additional dollar of earned income.

In comparison with the AFDC programs then existing in the states of Washington and Colorado, these program parameters provided relatively generous welfare assistance. That is,

a family in the treatment group would have a higher family income under SIME and DIME than one with the same characteristics could enjoy under the existing AFDC program.

In terms of impact on labor-supply behavior, the SIME and DIME programs unambiguously offered greater disincentives to work than the existing welfare program. This is because both the support level and the tax rates were higher under SIME and DIME than under the existing AFDC alternative.[9] By comparing the behavior of youth in the treatment group with that of the control group, we are able to evaluate the effect of these greater disincentives.

III. Empirical Analysis

The welfare experiments generated a wealth of data on the labor-market behavior of individuals in the treatment and control groups. The data were collected through a series of periodic interviews conducted at the time of enrollment and also throughout the course of the experiment. For the purposes of our study, youths were defined as individuals who were between 16 and 21 years of age, and living at home, at the time their families enrolled in the experiment.

Since we are interested primarily in the possible effects of the welfare program on measured youth unemployment, we focus on two aspects of labor-market behavior that can influence this measure. The first is labor-force participation behavior. We measure the impact of the experiment by examining the age at which the youths in the experiment first report an active search for work. By contrasting the age of first participation of the treatment group with that of the control group, we are able to obtain a crude indication of the extent to which the welfare experiment delays youth labor-force participation. The second aspect we examine is job-taking behavior. By comparing the age at which the youths in the treatment group first take full or part-time jobs with the experience of the control group, we are able to estimate the extent to which the welfare experiment delays youth employment.

We can deduce the experiment's impact on unemployment in a rough way by comparing the labor-force participation effect with the job-taking effect. If, for example, welfare tends to delay jobtaking without delaying labor-force participation, the behavior of these first-time job seekers would tend to add to measured youth unemployment. Whether the overall unemployment rate rises, however, depends upon their later behavior. If welfare causes a youth to stay in a job longer, for example, this would tend to offset the effect on the unemployment rate of delays in taking the first job. However, the unemployment of entrants into the job market is a major factor in the high youth-unemployment statistics; the unemployment rate for youths with previous employment is close to the adult unemployment rate.[10] Thus it seems likely that a further delay in employment caused by welfare support would translate into a higher overall youth-unemployment rate.

This is admittedly a very crude method for discerning the impact of the experiment on unemployment. Ideally, the unemployment rate should be studied directly, by simply measuring the percentage of young people who report themselves to be participating in the labor force but without a job at a particular point in time. By contrasting the control group's and the treatment group's unemployment rates, the effect of the experimental program could then be obtained directly. Unfortunately, however, the data available at the time of the study were not suitable to this approach, since the employment status of individuals could not be determined precisely on a day-to-day basis, making direct calculation of unemployment rates impossible.[11] Still, despite the limitations of our approach, it can provide useful indications of the way in which the youth unemployment may be affected by welfare policy.

IV. Statistical Procedures

The behavior of the treatment group is compared with that of the control group by estimating the coefficients of a simple regression equation using the cross-section of data from the youth sample. The general form of the equation is

$$H = Fc + Xb + Ce + E \qquad (1)$$

where

H = measures of labor-market behavior (i.e., age at entry into the labor force or age at which a job is taken).

F = dummy variable which = 1 if the individual is in the treatment group and = 0 if in the control group.

X = set of demographic variables to control for personal or family attributes which may affect labor-market behavior (see Table 1).

C = set of variables to control for other experiment features which may affect labor-market behavior, such as schooling subsidies (see Appendix B).

E = error component,

and c, b and e are vectors of coefficients to be estimated. This general equation was estimated for three different dependent variables: H_1, age of initial labor-force entrance; H_2, age of taking a full-time job; and H_3, age of taking a part-time job.

The experiment's effect on these measures is represented by the coefficient on the dummy variable F. Thus, if those youths eligible for welfare assistance delay the age at which they take jobs (relative to controls), the coefficient on F measures the extent of the delay (in years). The other explanatory variables (sets X and C) primarily control for other factors which may influence labor-market behavior, thereby permitting the effects of the experiment to be isolated. Table 1 lists the demographic variables (set X) which are employed to control for the effects of personal or family attributes on labor-market behavior. Appendix B discusses those variables (set C) which are used to control for the design features of the experiment; these variables are not discussed further because they are not relevant to the issues addressed in this paper.

Table 1
Demographic Variables
(Variable set X)

Variable name	Variable definition
SEATTLE	Dummy variable; takes on the value 1 if youth is from Seattle site, and 0 if from Denver
BLACK	Dummy variable; takes on the value 1 if youth is black, and 0 if otherwise
CHICANO	Dummy variable; takes on the value 1 if youth is Chicano, and 0 if otherwise
SINGLEHEAD	Dummy variable; takes on the value 1 if the youth is from a female-headed (fatherless) household, and 0 if otherwise
FAMILYSIZE	Number of family members in the youth's family at time of enrollment in the experiment
CHILDREN	Number of children younger than 5 years of age in the family at time of enrollment in the experiment
INCOME	Family income (in dollars) at time of enrollment in the experiment

The regression equations were estimated separately for the 517 males and 485 females in the sample, because the labor-market behavior of these groups differed considerably in the early years of their work experience. Appendix B provides further details of the sample and the econometric techniques employed in estimating the regression equations.

V. Results: Effects of Experiment

The coefficients on the dummy variable F measure the effects of welfare eligibility on the labor-market behavior of the youths in the sample. Table 2 summarizes these effects.

First, the experimental welfare program apparently does not significantly affect individual decisions to enter the labor force. That is, the measured delay of entry into the labor force associated with eligibility for the welfare program (row one in Table 2) is not statistically different from zero for either males or females. Those youths whose families are eligible for

welfare support thus appear to enter the labor force at about the same age as youths in control families.

Second, the experimental program in contrast *does* appear to delay the age of initial full time or part-time employment. For male youths, welfare eligibility is associated with a delay of .74 years in the age of full-time employment (row two). For females, the program's effect is not significant for full-time employment but it is significant for part-time employment, where welfare eligibility is associated with a delay of .93 years. The coefficient for part-time job-taking is not significant for males, although the sign is the same as for females. The greater sensitivity of males in the full-time category and females in the part-time category could be expected, because young females tend to seek part-time employment while young males tend to be oriented toward full-time employment.

These results suggest that the expected reduction in labor supply from the welfare program primarily comes about because of delays in accepting employment, rather than delays in entering the labor force. These effects generate increases in measured unemployment among youths just beginning their labor-market experience. As mentioned earlier, the available data are not sufficient to measure the effect on unemployment rates, *per se,* but this finding underscores the importance of considering supply-side factors when investigating youth-unemployment phenomena.

Table 2
Effects of Experimental Welfare Program

Delay (in years)	Males	Females
In joining the labor force	−.127	.149
	(.287)	(.208)
In taking a full-time job	.739**	.183
	(.267)	(.143)
In taking a part-time job	.565	.929*
	(.402)	(.370)

Note: The delay experienced by those eligible for the experimental welfare program is measured relative to the behavior of the controls. Standard errors are in parentheses.

*Coefficient differs from zero at the 5-percent level.
**Coefficient differs from zero at the 1-percent level.

VI. Results: Other Factors

The coefficients on the various demographic control variables, (variable set x), while not relevant to an evaluation of the welfare experiment, usefully illustrate how other factors can affect labor-market behavior. The coefficients associated with these variables are presented in Table 3.

The dummy variable SEATTLE indicates the location of an individual in the sample, whether Denver or Seattle. The coefficient on this variable thus captures *differences* in labor-market behavior in the two cities. Although such differences could indicate either demand or supply differences, in the present context the coefficient most likely captures differences in the *demand* for labor.

At the time of the experiments (early to mid-1970's), the Seattle economy was severely depressed because of a decline in the locally-important aerospace industry. As a result, we would expect the demand for labor of all kinds to be lower in Seattle than in Denver, and for this to lead to delays in finding work. (It could also discourage youths from entering the labor force.) The expectation is borne out in the positive sign of the Seattle dummy variable. This implies that the age of labor-force entry and (for females) the age of job-taking tended to be higher in Seattle than Denver. Males in Seattle, for example, took 1.47 years longer to find a full-time job than their Denver counterparts. The fact that *both* the entry age and the employment age were higher in Seattle illustrates the earlier point that the participation decision and the employment decision tend to be affected by the same factors and to move in the same direction. The unemployment impact depends upon the net effect of these factors. In the present case, the employment age apparently is delayed by more than the entry

age as a result of Seattle residency; this would tend to make measured unemployment higher there than in Denver.

The coefficients of the race variables BLACK and CHICANO in Table 3 measure the *difference* in behavior between non-white and white youths. The coefficients suggest a relationship between labor-market behavior and race that is consistent with aggregate unemployment statistics. Black males, for example, enter the labor force .80 years later than whites, and take full-time jobs and part-time jobs 1.53 and .96 years later, respectively. The greater delay in job-taking than in entry is qualitatively consistent with the higher rates of black youth unemployment that are typically observed in aggregate statistics. This phenomenon may, of course, be a manifestation of either demand or supply-side factors.

Among the Chicanos in the sample, the greatest effect is observed among males. Relative to white youth, they experience delay in full-time job taking of .80 years and in part-time job taking of 1.03 years. The relative delay in entering the labor force is not significant for males, but it is significant for Chicano females.

The coefficients on the variables that describe family characteristics suggest that family composition influences the participation and job-taking decisions of youth as well. The dummy variable SINGLEHEAD, for example, indicates which youths come from female-headed households. The negative sign on this variable, for both males and females, suggests that this family structure is associated with earlier labor-force participation and earlier employment. However, the effects are only significant for females.

The number of family members (measured by FAMILYSIZE) appears to be negatively related to the age at which youths enter the labor force. An additional family member lowers the age of labor-force entry by .22 and .15 years for males and females, respectively. However, for female youths, the presence of

Table 3
Coefficients Associated With Various Control Variables

Dependent variable	Age of Entry into Labor Force		Age of Taking Full-time Job		Age of Taking Part-time Job	
	Males	**Females**	**Males**	**Females**	**Males**	**Females**
Independent variable						
SEATTLE (0,1)	.024	.444*	1.475***	.603***	.277	.539
	(.306)	(.231)	(.296)	(.269)	(.441)	(.414)
BLACK (0,1)	.809***	.209	1.526***	.962***	.924***	.359
	(.310)	(.225)	(.294)	(.267)	(.443)	(.404)
CHICANO (0,1)	.558	.767***	.805**	.336	1.034*	.688
	(.433)	(.323)	(.366)	(.366)	(.621)	(.564)
SINGLEHEAD (0,1)	−.545	−.517**	−.128	−.458	−.522	−1.011**
	(.341)	(.262)	(.327)	(.311)	(.488)	(.477)
FAMILY SIZE	−.218**	−.150*	.019	.061	0.226	−.108
(number)	(.103)	(.079)	(.098)	(.096)	(.147)	(.146)
CHILDREN	−.260	.677***	−.029	.209	.268	.735
(number)	(.405)	(.254)	(.365)	(.317)	(.555)	(.474)
INCOME ($1000)	.025	.008	−.052	−.063	−.044	.002
	(.050)	(.036)	(.046)	(.044)	(.075)	(.007)

***Coefficient differs from zero at the 1 percent level.
**Coefficient differs from zero at the 5 percent level.
*Coefficient differs from zero at the 10 percent level.

small children in the household (CHILDREN) tends to delay labor-force participation, presumably because young females perform child-care services in the home.

Family income (INCOME) appears to have no statistically significant effect on youths' participation and job-taking decisions. The signs of the coefficients generally suggest delayed entry but earlier job-taking, but the low level of statistical precision suggests that these results may be spurious. Other observers have noted the poor association between income and youth unemployment, but in this study the problem is compounded by the use of family income to assign individuals to the welfare experiment (see Appendix 3).

The statistical importance of many of these demographic variables clearly indicates the variety of factors accounting for observed patterns of labor-market behavior. Thus, no single factor is likely to explain satisfactorily the level and changes in youth-unemployment rates that the United States has experienced in recent years. Even after controlling for these demographic factors, however, an important association appears to exist between the experimental welfare program and delayed youth employment.

VII. Conclusions

It has not been possible to measure the impact of a welfare program on youth unemployment directly. Nonetheless, the results observed in this study are consistent with the notion that youths respond to welfare programs by reducing labor-market activity. Thus, young people do not appear to be insulated from the work-retarding effects of welfare programs.[12]

Secondly, the results are also consistent with the argument that family welfare support contributes to measured youth unemployment by delaying employment without delaying entry into the labor-force. Despite our inability to calculate the precise effect of the experimental welfare program on unemployment, we can see that the delay in job-taking is large and significant. For males taking full-time jobs, for example, the delay caused by the experiment is roughly the same as the delay associated with being Chicano (rather than white). Since Chicano unemployment rates are several percentage points greater than white unemployment rates, the effect of the welfare program may be of that same order of magnitude.

Finally, and most importantly, the study highlights the relevance of considering supply as well as demand factors in studying the youth-unemployment problem. There may be considerably more volition in the pattern of youth unemployment than is generally assumed. Although it is very difficult to determine precisely the effect of supply-side factors—such as attitudes, tastes, family structure, and family economic status—these factors may contribute significantly to the trends that have been observed in youth unemployment. Policy prescriptions thus can differ considerably, depending upon whether the problem has a demand-side or supply-side genesis. The results of this study suggest that a policy to eradicate youth unemployment by making jobs more available—through public-employment programs, for example—may not be completely successful in reducing unemployment among youths from welfare families.

Unfortunately, we cannot use the results of this study to infer how the *existing* welfare program affects youth labor-market behavior. Although the experimental welfare programs were *more* conducive to creating unemployment than the existing (AFDC) program, their effects were measured against the effects of the existing program since the control group remained eligible for AFDC. Therefore, our results may either over- or under-state the effects of the existing program (vs. no welfare program). However, the results do suggest qualitatively that the existing welfare program contributes to youth-employment problems.

Much remains to be learned about the causes of joblessness among the young. The problem is a multifaceted one, and no single factor can be held responsible for the trends that have been observed in youth labor-market behavior. Still, our results emphasize one potential source of youth unemployment that policymakers should consider and explore further.

Appendix A: Experimentation in Economics

Although experimental research is commonplace in laboratory sciences such as chemistry and pharmacology, it is unusual in economics. Economists do not, in general, have the opportunity to manipulate economic variables in a controlled manner and observe the consequences on individual firms or households in the economy. Economics is, by its very nature, a *social* science, and most economic research involves observation of behavior in the natural state of the economy. The relationships between economic variables are normally inferred from observed patterns of behavior in the context of a model, utilizing a set of statistical procedures that are consistent with the assumptions of the model.

What is an economic experiment?

In contrast to the "arm's length" nature of most economic analysis, economic experimentation involves direct manipulation of economic variables. In the typical economic experiment, a site for the experiment (usually a community or area) is selected, and part of the population of the area is enrolled to participate in the experiment. The participants are assigned to either a "treatment group" or a "control group." The economic environment

Table A-1
Recent Experiments in Economic Policy

Experiment	Site(s)	Objectives
1. Income maintenance	New Jersey; Pennsylvania (1968); Gary, Indiana (1970); Iowa; North Carolina (1969); Seattle, Washington; Denver, Colorado (1971)	To evaluate the effects of a negative income tax on aggregate labor supply.
2. Health insurance	Dayton, Ohio; Seattle, Washington; selected counties in Massachusetts and South Carolina (1973)	To evaluate the response of health-care demand to changes in the price of health-care services.
3. Supported work	Fifteen different cities and rural areas (1975)	To test the effectiveness of job-training programs on individuals with traditionally poor records of employability, such as ex-offenders and former drug addicts.
4. Employment service	Minneapolis, Minnesota; Salt Lake City, Utah; West Palm Beach, Florida (1975)	To evaluate the effect of job counseling on the labor-market experience of the unemployed.
5. Housing allowances	Various sites including Pittsburgh, Pennsylvania; Phoenix, Arizona; Green Bay, Wisconsin; and South Bend, Indiana (1976)	To evaluate the effect of cash housing allowances on the demand and supply of housing.
6. Electric power rates	Six utilities in various states (1976)	To evaluate the effect of different electricity-rate schedules on the consumption of electric power, with particular emphasis on time-of-day pricing.
7. Medicare coverage	Entire state of Colorado (1976)	To evaluate alternative coverage plans on the use and cost of mental-health services.
8. Public employment	Thirty sites in various states (1979)	To evaluate the effect of a large-scale public-employment program on employment and wages through the use of treatment and control sites (demonstration project combined with policy experiments).
9. Youth employment subsidy	Detroit, Michigan (in the planning stage)	To evaluate the effect of wage subsidies on youth employment.

of the treatment group is manipulated as part of the experiment, while the control group is simply a source of "baseline" data. The comparison of the behavior of the treatment and control groups measures the effect of the experimental program. (The use of a control group distinguishes an *experiment* from a *demonstration;* the latter is simply a test of the administrative feasibility of a project, and cannot provide a precise evaluation of its effects.)

Economic experiments have been conducted in a wide range of policy contexts. Experiments have involved manipulation of such economic variables as electricity prices, housing rentals, health-care costs, education costs, and the generosity of welfare programs. (See Table A-1 for a partial list of recent economic experiments.)

Experimental vs non-experimental

Although the non-experimental mode of analysis has served the economics profession well, the available theory or data are not always sufficient to provide the information necessary to resolve practical problems. Simple parameters such as the elasticity of labor supply with respect to the wage rate, for example, have been disturbingly difficult to estimate using non-experimental procedures. As a result, economic analysis has provided little help to policymakers interested in accurately predicting the labor-market effects of proposals such as a negative income tax, wage subsidies, public-employment programs, and so on.

Experimentation offers several advantages for policy evaluation. First, the direction and magnitude of the effect of one economic variable on another can potentially be measured with considerable precision, because the policy variable can be exogenously manipulated in a measurable way. The price of a commodity, the income of an individual, or the quality of a product can be manipulated at will, independent of other variables. In the non-experimental environment, these changes usually occur in concert with other changes, making it difficult to isolate the effects of individual factors and the direction of causality.

Second, experimentation offers the opportunity to examine a variety of social effects of a policy—often beyond that which economic modelling and analysis is capable of doing with non-experimental data. For example, the effect of a policy on the child-bearing or marital behavior of a family is very difficult to predict precisely with existing economic models and data. Yet these effects may be very important to policymakers and may have important economic implications as well. An experiment can be designed to monitor these effects.

Finally, experimentation permits programs with complex economic and administrative features to be evaluated. It is often very difficult to model these features and evaluate the response without making many questionable assumptions. By putting the policy into practice on an experimental basis and observing the consequences, the effects of a specific program can be directly evaluated.

Appendix B: Technical Details of the Model and Sample

Econometric Considerations

Estimation of the relationship in equation (1) encounters a number of econometric problems. First, the data on the dependent variables (H_1, H_2, and H_3) suffer from "censoring" because of the relatively short observation period. To understand this problem of censoring, suppose, for example, that a youth never takes a full-time job during the course of the observation period. What age should then be used to construct H_2? The only available datum is the youth's age at the time when he was last observed. This age is clearly less than the true age at which he ultimately takes a job, however; that is, the measure is "censored" from above. Similarly, if a youth already has labor-market experience when first enrolled in the experiment, the only information we have is his age at that time; these observations are censored from below.

The econometric implication is that the distribution of the error term in equation (1) does

not have the properties assumed in the classical regression model. Without appropriate treatment of this effect, the estimated coefficients are biased. However, with a procedure developed by James Tobin (called Tobit), we are able to obtain unbiased estimation under the conditions created by censored data.[13] The procedure uses the age at which the observation was censored along with the information about the type of censoring experienced (i.e., from above or below). The program employs maximum likelihood techniques to derive the necessary estimates, but the coefficients may be interpreted in the normal manner.

A second major statistical concern involves defining the independent variables so that the effect of the experiments is not commingled with variables which we are using for control purposes. For example, the experiment could conceivably affect the family's childbearing behavior. Thus, if the variable FAMILYSIZE is measured during the experiment, it may contain some effects of the experiments, thereby biasing the measure of the experimental effect obtained with the variable F. The approach taken here to limit this bias involves using pre-experimental measures of all of the control variables. This ensures that these measures are unaffected by the experiment. Although this method introduces measurement error—since the pre-experimental values may be imprecise measures of the relevant value—it is assumed that these effects are less serious than the problem of commingling experimental and control variables.

Table B-1
Sample Characteristics

	Male	Female
Age at enrollment (%)		
16	28.8	33.6
17	28.6	29.7
18	17.4	18.6
19	12.2	11.3
20	9.3	5.6
21	3.7	1.2
Race (%)		
Black	46.8	44.1
White	33.5	37.7
Chicano	19.7	18.1
Hours worked per week		
at enrollment	6.4	4.5
2½ years into experiment	16.9	15.5
School registration (%)		
at enrollment	73.9	79.9
2½ years into experiment	27.8	27.2
Outside labor force (%)		
at enrollment	51.3	62.9
2½ years into experiment	44.0	57.5
Family type at enrollment (%)		
Husband-wife	44.1	43.3
Female head	55.9	56.7
Treatment status (%)		
Eligible for experimental welfare program	52.4	51.1
Seattle residency (%)	39.5	48.5
Persons in family (No.)	4.8	4.6
Young Children in family (No.)	.1	.2
Family income ($)	6,511.0	6,680.0
Sample size	517	485

Additional control variables

In addition to the variables reported in Table 1, two additional types of variables were included in the variable set C in order to control for features of the experiment which would influence the behavior of the individuals in the sample. First, in addition to the welfare provisions of the experiments, certain families were eligible for manpower programs that provided subsidies for training and educational activities. To control for these effects, the regressions contain dummy variables for each manpower program. The coefficients of these variables were not significantly different from zero, and are not reported here because these programs are not of direct interest to our discussion.

A final consideration involves the need to control for the way in which families were assigned to the control group vs. the "treatment" group. As is typical in many experimental designs, the assignment was not completely random because of cost considerations; in particular, each family's pre-enrollment income was one of the characteristics used to assign experimental treatment. This non-random assignment can cause bias in the measurement of the coefficients.[14] The simplest approach to mini-

mize this bias is to include (as we do) a dummy variable in the regression which indicates which income-classification group the family was placed in for purposes of assignment to experimental treatment. (Because the coefficients on these dummy variables have no policy interpretation, they are not discussed in this paper. However, it should be noted that the use of two "income" variables—the control dummies and INCOME—makes it difficult to interpret the latter's coefficients.)

Description of sample

The individuals chosen for use in this study were between 16 and 21 years of age at the time that their families agreed to participate in the experiment. The sample was confined to the sons, daughters, grandsons, granddaughters, stepsons or stepdaughters of the head of the household. All were living with their families at the time of enrollment in the experiment.

The experiments did not enroll families headed by a single male, but over half of the sample was composed of female-headed households. The families in the experiment were chosen with incomes at or below the 1971 median income. In order to permit estimation of effects by race, black and Chicano families were heavily sampled. Table B-1 contains selected statistics which describe the sample.

FOOTNOTES

1. The literature on youth unemployment is extensive. A broad and useful introduction to the topic is available in **The Teenage Unemployment Problem: What are the Options?** Congressional Budget Office (October 1976).

2. See, for example, Welch (1974), Gramlich (1976) and Ragan (1977).

3. Ragan (1977).

4. This argument has been put forth by Moynihan (1968), for example.

5. See Freeman (1979).

6. See, for example, Ashenfelter and Heckman (1979), Killingsworth (1976), and MacDonald and Stephanson (forthcoming).

7. Seater analyzes labor-force behavior in the context of an optimal-control model which permits simultaneous determination of the optimal paths of time allocation to labor, job search and leisure over the life cycle. He concludes, "The response of unemployment [to exogenous changes] is ambiguous because unemployment is a "middle" state between employment and nonparticipation. Changes which tend to induce some people to leave unemployment for employment also tend to induce other people to leave nonparticipation for unemployment, leaving the net change in unemployment ambiguous." Seater (1977), p. 369.

8. Six of the programs employed fixed tax rates of either 50, 70 or 80 percent. The other five programs employed a rate which was initially at one of these levels, but declined with increasing income. These declining tax-rate programs were designed to determine if the work disincentive effects of the NIT could be eased by a smoother transition between NIT tax rates and the tax rates of the normal (positive) income-tax system.

9. At the time of the experiments, the **statutory** tax rate embodied in the AFDC program was 67 percent. However, as Halsey (1978) has shown, when the integration of AFDC with other welfare programs (housing and food-stamp programs) and the positive income-tax system is properly analyzed, the **effective** tax rate is around 47 percent, a lower rate than the effective tax rates employed in the SIME and DIME programs.

10. Freeman (1979), Table 2.

11. The construction of the spell-oriented data file necessary for this computation is feasible, however, using the raw SIME and DIME data. The construction of such a data file may be undertaken by SRI International in 1980.

12. Although we are interested in the effects on measured unemployment **per se,** from a long-run policy point of view, the delays in job-taking need not be wholly deleterious. For example, youth may be spending more time in school as a result of family welfare support. The evidence on this from the SIME and DIME programs is not very encouraging, however. West (1979) found that the experiments were **not** associated with significant increases in school-going propensity.

13. See Tobin (1958). The program used in this analysis was written by Arden Hall of SRI.

14. The seriousness of the bias caused by non-random assignment has been the subject of considerable debate. In this research, the results were not noticeably changed when the variables designed to correct for assignment were omitted from the regression specification. For a discussion of experimental assignment procedures and their effects, see Conlisk and Watts (1979) and Keeley and Robins (forthcoming).

REFERENCES

Ashenfelter, O. and J. Heckman, "The Estimation of Income and Substitution Effects in a Model of Family Labor Supply," **Econometrica** (January 1979).

Conlisk, J. and H. Watts, "A Model for Optimizing Experimental Designs for Estimating Response Surfaces," **American Statistical Association Proceedings, Social Statistics Section** (1969).

Ferber, R. and W. Hirsch, "Social Experimentation and Economic Policy: A Survey," **Journal of Economic Literature** (March 1978).

Gramlich, E., "Impact of Minimum Wages on Other Wages, Employment, and Family Incomes," **Brookings Papers on Economic Activity** (2:1976).

Halsey, H., "AFDC, Food Stamp and Public Housing Taxes in Seattle and Denver in 1970–1971," SRI International, Menlo Park, CA., March 1978.

Keeley, M. C., et al., "The Estimation of Labor Supply Models Using Experimental Data," **American Economic Review** (December 1978).

Keeley, M. C. and P. K. Robins, "The Design of Social Experiments: A Critique of the Conlisk-Watts Assignment Model," **Research in Labor Economics,** editor R. Ehrenberg (forthcoming 1980).

Killingsworth, M. R., "Must a Negative Income Tax Reduce Labor Supply: A Study of the Family's Allocation of Time," **Journal of Human Resources** (Summer 1976).

Mallar, C., "The Educational and Labor Supply Response of Young Adults in Experimental Families," **The New Jersey Income Maintenance Experiment,** Volume II, Chapter 5, eds. H. Watts and A. Rees, New York: Academic Press (1977).

McDonald, J. and S. Stephanson, "The Effects of Income Maintenance on the School Enrollment and Labor Supply Decisions of Teenagers," **Journal of Human Resources** (forthcoming).

Moynihan, D., "Poverty in Cities," in J. Q. Wilson, ed., **The Metropolitan Enigma** (Boston, Harvard University Press, 1968).

Ragan, J., "Minimum Wages and the Youth Labor Market," **The Review of Economics and Statistics** (May 1977).

Seater, J., "A Unified Model of Consumption, Labor Supply, and Job Search," **Journal of Economic Theory** (April 1977).

Tobin, J., "Estimation of Relationships for Limited Dependent Variables," **Econometrica** (January 1958).

Welch, F., "Minimum Wage Legislation in the United States," **Economic Inquiry** (September 1974).

West, R., "The Impact of the Seattle and Denver Income Maintenance Experiments on Youth Labor Supply and Schooling Choices," SRI International (May 1979).