

TABLE 2.1 *How Labor Supply and Demand Policies Alter the Labor Supply of or Demand for the Poor*

Intervention	Labor Supply Policies	Labor Demand Policies
Direct public provision	?	<ul style="list-style-type: none"> <li>• Public-service employment</li> <li>• Sheltered workshops</li> <li>• Supported work experience programs</li> </ul>
Incentives	<ul style="list-style-type: none"> <li>• Wage subsidies to workers</li> <li>• Increased earned income disregards in welfare programs</li> <li>• Subsidies for child care and other work-related expenses</li> </ul>	<ul style="list-style-type: none"> <li>• Wage subsidies to employers</li> <li>• On-the-job training subsidies to employers</li> <li>• Economic development programs</li> </ul>
Sanctions	<ul style="list-style-type: none"> <li>• Job search requirements in welfare</li> <li>• Work requirements</li> <li>• Welfare time limits</li> </ul>	<ul style="list-style-type: none"> <li>• Anti-discrimination laws</li> <li>• Affirmative action</li> <li>• Minimum-wage laws, other labor regulation</li> </ul>
Training	<ul style="list-style-type: none"> <li>• Education</li> <li>• Job training</li> </ul>	<ul style="list-style-type: none"> <li>• Diversity training</li> <li>• Job retention services to help employers reduce turnover</li> </ul>
Information provision	<ul style="list-style-type: none"> <li>• Labor exchange services/ job development services—providing information to poor on job openings</li> </ul>	<ul style="list-style-type: none"> <li>• Labor exchange services/ job development services—helping employers find qualified employees among poor</li> </ul>

Source: Author's own classification.

TABLE 2.2 *Organizational Arrangement and Clientele of Supply and Demand Programs That Affect the Employment of the Poor*

Organizational Arrangements and Clientele	Labor Supply Programs	Labor Demand Programs
Second-chance system; employment-related programs	<ul style="list-style-type: none"> <li>• Job training agencies: programs that provide training and placement</li> <li>• Welfare-to-work agencies: programs that provide training and placement</li> <li>• Employment service: information to job seekers on job openings</li> </ul>	<ul style="list-style-type: none"> <li>• Job training agencies: programs that provide summer public-service jobs to youth, work experience, and wage subsidies to employers for OJT</li> <li>• Welfare-to-work agencies: programs that provide public-service jobs, community service jobs, work experience, and grant diversion to subsidize private employers</li> <li>• Employment service: information to employers on job seekers</li> </ul>
First-chance system of education <sup>a</sup>	<ul style="list-style-type: none"> <li>• Head Start</li> <li>• K-12 public schools</li> <li>• Community colleges</li> <li>• Four-year public colleges and universities</li> </ul>	The customized training provided by community colleges to employers is typically funded by economic development agencies, and rarely targeted at the disadvantaged
Economic development programs <sup>b</sup>		<p>Wide variety of subsidies for new and expanding businesses, particularly manufacturing, including:</p> <ul style="list-style-type: none"> <li>• Customized training (see above)</li> <li>• Discretionary tax subsidies, such as property tax abatements</li> <li>• Subsidized infrastructure</li> </ul>
Tax system	Earned Income Tax Credit	Tax credits to employers for hiring disadvantaged groups, such as former Targeted Jobs Tax Credit and current Work Opportunity Tax Credit. Also, some general wage subsidies for hiring were for-

TABLE 2.2 *Continued*

Organizational Arrangements and Clientele	Labor Supply Programs	Labor Demand Programs
Labor regulations		<p>merly used, such as New Jobs Tax Credit of 1977 to 1978.</p> <ul style="list-style-type: none"> <li>• Minimum wage: enforced by federal and state departments of labor</li> <li>• Equal employment opportunity laws: enforced by departments of labor and various civil rights agencies</li> </ul>

*Source:* Author's own classification.

<sup>a</sup> Some education programs are targeted at the poor, but the poor also benefit from general public subsidies for education.

<sup>b</sup> Economic development programs are rarely targeted at disadvantaged persons, but some programs (enterprise zones) are targeted at distressed places.

TABLE 2.3 *Comparison of Labor Supply and Demand Policies in the United States and Other Leading Industrial Nations*

Country (Fiscal Year of Data)	Percentage of GDP Devoted to Labor Supply Policies	Percentage of Labor Force Annually Participating in Labor Supply Policies	Percentage of GDP Devoted to Labor Demand Policies	Percentage of Labor Force Annually Participating in Labor Demand Policies
United States (1997 to 1998)	0.11	1.3	0.01	0.2
Canada (1996 to 1997)	0.20	2.1	0.07	0.7
United Kingdom (1997 to 1998)	0.06	0.9	0.15	1.1
Germany (1998)	0.50	2.4	0.55	2.4
France (1997)	0.40	3.3	0.81	6.9
Japan (1997 to 1998)	0.03	na	0.02	na
Sweden (1998)	0.54	5.6	1.07	6.5

*Source:* Data are derived from author's calculations using Organization for Economic Cooperation and Development (1999, 246–52, table H).

*Notes:* Labor supply programs include training for unemployed adults (category 2a), measures for unemployed and disadvantaged youth (3a), and vocational rehabilitation (5a). Labor demand programs include training for employed adults (2b), support of apprenticeship and related forms of general youth training (3b), subsidized employment (4, which includes subsidies to regular employment in the private sector, support of unemployed persons starting enterprises, and direct job creation, public or nonprofit), and work for the disabled (5b).

TABLE 2.4 *Resources Devoted to Labor Supply and Labor Demand Programs*

Labor Supply Programs	Percentage Share of GDP	Labor Demand Programs <sup>a</sup>	Percentage Share of GDP
Second-chance programs: training and placement	0.060	Second-chance programs: subsidized employment	0.029
Federal job training programs: skills development, job placement	0.027	Federal job training programs: summer youth jobs, OJT, and work experience	0.013
Welfare-to-work: training and placement	0.027	Welfare-to-work: OJT, work experience, community service jobs, public-service jobs	0.011
Employment service: half allocated to supply side	0.006	Employment service: half allocated to demand side	0.006
First-chance education programs: targeted at disadvantaged	0.215		
Head Start	0.051		
Title I spending on disadvantaged students, K-12	0.073		
Pell Grants and state need-based higher education aid	0.091		
First-chance education programs: general subsidies, proportion going to disadvantaged students	0.477	Economic development programs: proportion that benefits the disadvantaged	0.043
Total amount for elementary and secondary education that goes to disadvantaged	0.463		
Total subsidies for higher education that go to disadvantaged students	0.014		
Earned Income Tax Credit	0.353	Work opportunity tax credit and welfare-to-work tax credit	0.004
		Minimum-wage laws: wage increase for low-income workers	0.032
		Equal employment opportunity laws: proportion that benefits disadvantaged	0.010
Total	1.105		0.118

Source: Author's compilation.

Note: More details on the derivation of this table are in chapter 2, note 3.

<sup>a</sup> Past labor demand programs at their peak:

WPA (FY1939) 2.541 percent of GDP

CETA public service employment (FY1979) 0.160 percent of GDP

TABLE 3.1 *Employment Rates for Persons Ages Twenty-Five to Fifty-Four, by Gender, Race, and Marital Status*

	High School Dropouts		High School Graduates		College Graduates	
	1979	1998	1979	1998	1979	1998
<b>Males</b>						
White non-Hispanic	0.846	0.759	0.935	0.905	0.960	0.950
Black non-Hispanic	0.750	0.574	0.867	0.809	0.905	0.918
Hispanic	0.858	0.843	0.912	0.883	0.913	0.917
Other non-Hispanic	0.813	0.735	0.870	0.863	0.893	0.923
<b>Married (spouse present) females</b>						
White non-Hispanic	0.435	0.520	0.551	0.727	0.635	0.791
Black non-Hispanic	0.518	0.577	0.673	0.755	0.833	0.879
Hispanic	0.369	0.435	0.534	0.656	0.626	0.741
Other non-Hispanic	0.429	0.532	0.579	0.669	0.659	0.699
<b>Other females</b>						
White non-Hispanic	0.516	0.480	0.810	0.818	0.893	0.913
Black non-Hispanic	0.423	0.441	0.670	0.731	0.882	0.889
Hispanic	0.414	0.522	0.669	0.738	0.806	0.863
Other non-Hispanic	0.484	0.470	0.791	0.724	0.882	0.826

*Source:* All employment rates were calculated by the author using data from the Outgoing Rotation Group of the Current Population Survey (CPS-ORG).

*Note:* The employment rate is the annual average over twelve monthly surveys of those employed last week as a proportion of the total population of each group.

TABLE 3.2 *Number of New Jobs for Prime-Age Less-Educated Persons Required to Match White Male Employment Rates of 1979*

	Whites	Blacks	Hispanics	Others	Total All Ethnic Groups
High School Dropouts					
Males	0.296	0.265	0.008	0.027	0.596
Unmarried females	0.411	0.318	0.304	0.049	1.082
Total	0.707	0.583	0.312	0.076	1.678
High School Graduates					
Males	0.740	0.538	0.162	0.078	1.518
Unmarried females	0.970	0.687	0.224	0.095	1.975
Total	1.710	1.225	0.386	0.173	3.493
Male total	1.036	0.803	0.170	0.105	2.114
Unmarried female total	1.381	1.005	0.528	0.143	3.057
Total males and unmarried females	2.417	1.808	0.698	0.249	5.171

*Source:* All data are derived from the author's calculations using the CPS-ORG.

*Notes:* All job figures are in millions of jobs, as of 1998, needed for persons ages twenty-five to fifty-four to match white male employment rates for that education category and age range in 1979. High school graduate category does not include college graduates. Data are calculated based on employment rates given in table 3.1 and estimated numbers of persons in each ethnic-education-gender category as of 1998. The formula for calculating needed jobs is: needed jobs for ethnic group  $r$ , education category  $e$ , gender  $g$  = number of persons in group ( $reg$ )  $\times$  (employment rate for white males in education category  $e$  in 1979 - employment rate for group [ $reg$ ] in 1998).

TABLE 3.3 *Number of New Jobs Needed for All Non-Elderly Poor Families to Have One Full-Time Worker*

	Full-time Equivalent Jobs Required (Millions)
Total all families with family head less than sixty-five years old	8.6
Families with children	2.9
Single-mother families with children	2.4
White	4.5
Black	2.2
Hispanic	1.5
Families with no adult worker during the current year	5.8

*Source:* All calculations are derived by the author from the March 1999 CPS.

*Notes:* For all poor families with a family head less than sixty-five years old, I calculated total annual work hours of all adults in the family. 18.8 percent have combined adult work hours of more than 2,000 hours per year and were excluded from further calculations; 43.2 percent have zero adult work hours, and one full-time job was assumed to be needed for each such family. Another 38.0 percent have work hours between 1 and 1,999 per year. The number of extra annual work hours needed for that family's total to equal 2,000 was calculated. The total number of extra annual work hours needed was divided by 2,000 to get the estimated number of full-time equivalent jobs needed.



TABLE 3.4 *Full-Time Equivalent Jobs Needed to Bring All Families with Children Up to the Poverty Line*

Group	Number of Families Below Poverty Line (Millions)	Mean Income Gap Below Poverty Line	Mean Proportion of FTE Jobs Needed to Bring Family up to Poverty Line <sup>a</sup>	Total FTE Jobs Needed (Millions) <sup>b</sup>
Families with children	5.6	\$7,056	0.504	2.8
Single-mother families with children	3.5	\$7,215	0.515	1.8

*SOURCE:* Data are author's calculations based on U.S. Census Bureau (1999, 21, table 4).

<sup>a</sup> A full-time equivalent (FTE) job was assumed to be 2,000 hours per year at \$7.00 per hour.

<sup>b</sup> The mean proportion of full-time equivalent jobs multiplied by the number of families.

TABLE 3.5 *Barriers to Employment and Work Experienced by Welfare Recipients*

Number of Barriers to Work	Percentage of Welfare Recipients with This Number of Barriers	Percentage Estimated to Work at Least Twenty Hours per Week Seven to Ten Months Later
0	15	81
1	21	71
2 to 3	37	62
4 to 6	24	41
7 or more	3	6

*Source:* These figures are taken from Danziger et al. (1999, 29, 34, figure 1, and table 5).

*Notes:* The “percentage estimated to work” are regression-adjusted numbers that hold race, children, age, urban residence, and other demographic characteristics constant. The “percentage estimated to work” is for a welfare recipient who is single, black, living in an urban census tract, twenty-five to thirty-four years old, the mother of one child under two, and a welfare recipient for seven years. Sampling frame draws from single mothers on the welfare rolls of an urban Michigan county in February 1997, and employment status is measured as of an interview in the September 1997 to December 1997 period. The fourteen barriers examined include poor job skills, little work experience, poor mental or physical health, substance abuse, domestic violence, transportation problems, and problems with racial discrimination.

TABLE 3.6 *Top Ten Occupations of the Poor and Nonpoor*

Occupation	Percentage in Occupation	Median Wage	Percentage High School Dropout
<b>Poor men</b>			
Cooks	5.7	4.62	62
Truck drivers	5.1	5.43	29
Farm workers	4.8	5.36	75
Janitors	4.7	5.08	38
Groundskeepers	4.1	5.00	68
Construction laborers	3.8	5.21	44
Carpenters	3.5	5.19	37
Stock handlers, baggers	3.0	4.90	34
Laborers, except construction	2.3	4.68	36
Cashiers	2.1	5.56	36
<b>Nonpoor men</b>			
Managers, administrators	7.0	22.00	3
Supervisors and proprietors, sales occupations	4.1	15.38	4
Truck drivers	3.9	12.02	21
Janitors	1.9	9.13	34
Carpenters	1.8	11.90	22
Cooks	1.7	6.87	42
Sales representatives, mining, manufacturing, and wholesale	1.5	18.26	3
Laborers, except construction	1.4	9.42	29
Computer systems analysts	1.4	24.96	1
Groundskeepers	1.3	7.69	45
<b>Poor women</b>			
Cashiers	11.1	5.00	37
Nursing aides	6.8	5.73	34
Waitresses	4.7	5.03	27
Cooks	4.4	4.61	43
Janitors	3.5	5.00	43
Maids	3.4	5.05	60
Private household cleaners and servants	2.4	4.82	50
Secretaries	2.4	5.29	13
Sales workers, other commodities	2.2	5.96	36
Early childhood teacher's assistants	1.8	4.95	15
<b>Nonpoor women</b>			
Secretaries	4.6	10.60	4
Cashiers	3.8	6.25	32
Managers, administrators	3.8	15.60	2
Supervisors and proprietors, sales occupations	3.1	10.66	5
Registered nurses	3.0	19.71	0
Nursing aides	2.8	8.17	20

*(Table continues on p. 46.)*

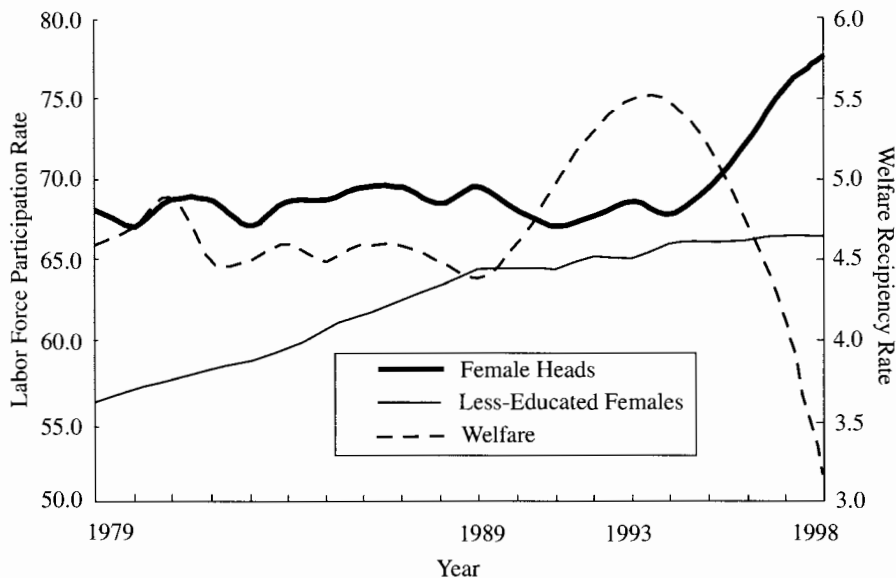
TABLE 3.6 *Continued*

Occupation	Percentage in Occupation	Median Wage	Percentage High School Dropout
Teachers, elementary schools	2.7	14.95	0
Bookkeepers	2.6	11.06	3
Waitresses	1.8	6.59	25
Receptionists	1.7	8.33	7

*Source:* Author's calculations from the March 1999 CPS.

*Notes:* The percentage of each group in its top ten occupations is 39.1 percent for poor men, 25.0 percent for nonpoor men, 42.7 percent for poor women, and 29.9 percent for nonpoor women. All data are based on income and earnings during 1998. Wage rate per hour is defined as earnings of person divided by product of weeks worked during 1998 and usual weekly hours. Hence, this is a rough measure of hourly wages, which may account for some of the median wages that are below the minimum wage. (Subminimum wages may also account for some of this.) All figures use appropriate sampling weights. The wage rate and percentage without a high school degree are calculated separately for each of the four groups for each occupation.

FIGURE 4.1 *Labor Force Participation Rates of Female Heads and Other Less-Educated Females and the Welfare Reciprocity Rate, 1979 to 1998*



Source: U.S. Department of Health and Human Services (2000) and author's calculations.

Notes: Labor force participation data for 1979 to 1998 come from the CPS-ORG. Female heads are female heads of household, with other relatives present in the household, who are ages sixteen to forty-four and have less than sixteen years of education. This is the closest to the group "less-educated single mothers" that can be defined consistently from 1979 to 1998 for the CPS-ORG. Less-educated females are all females, ages sixteen to sixty-four, with less than sixteen years of education, except for female heads. The calculated means use CPS-ORG weights. The welfare reciprocity rate is the number of welfare recipients for that fiscal year as a percentage of the U.S. population for that calendar year.

#### SIDEBAR 4.1     *The Earned Income Tax Credit*

The EITC provides a refundable tax credit based on the earnings of a low-income household. The credit begins at zero earnings and is then phased in at some tax credit rate up to the minimum level of earnings at which the maximum credit is received. The credit stays at the maximum over some range of earnings. The credit is then gradually phased out at some phaseout tax rate. The credit can be reduced, but not added to, by non-earnings income. Three separate credit schedules apply: for households with no children, for households with one child, and for households with two or more children.

The break points at which the credit phase-in starts, and the credit phaseout starts, are adjusted each year for inflation. This implies that the maximum credit is also adjusted each year for inflation. The schedules that applied for 1998 were:

	Type of Household		
	No Children	One Child	Two or More Children
Tax credit rate for phase-in (percentage)	7.65	34.00	40.00
Minimum earnings for maximum credit	\$4,460	\$6,680	\$9,390
Maximum credit	341	2,271	3,756
Phaseout rate (percentage)	7.65	15.98	21.06
Income level at which phaseout begins	\$5,570	\$12,260	\$12,260
Income level beyond which no credit is received	10,030	26,473	30,095

Source: 1998 Green Book, updated using 1998 tax forms from IRS.

## SIDEBAR 4.2    *The Minnesota Family Investment Program (MFIP)*

MFIP was an experimental welfare reform program begun in 1994. The research compared two welfare reform “treatments” to a control group that continued under the regular welfare program. One treatment changed the financial incentives for welfare recipients to work by giving extra MFIP benefits for working and lowering the benefit reduction rate. The other treatment combined these financial incentives with mandatory participation in employment and training services.

The financial incentives under MFIP increased the rewards for working compared to not working, but reduced the rewards for working full-time versus part-time. The table shows how the regular welfare program (AFDC) compares with MFIP in monthly net income for various weekly work hours at \$6 per hour. Net income in this table includes food stamps, the Earned Income Tax Credit, and federal and state taxes. The calculations are for a single parent with two children, in the fifth through twelfth month after beginning employment.

	AFDC	MFIP
Monthly income at zero work hours	\$769	\$769
Monthly income at twenty hours per week, \$6/hour	1,024	1,261
Monthly income at forty hours per week, \$6/hour	1,308	1,456
Average “wage” per hour from working forty hours rather than twenty hours	3.19	6.15
Average “wage” per hour from working forty hours rather than twenty hours	3.55	2.44

Source: Miller et al. (1997, table 1.2).

Why does MFIP offer less of a relative reward for full-time work than AFDC? Under old AFDC rules, most benefits are already phased out before a person reaches forty hours of weekly work. Hence, at some point the wage rate per hour for more work is no longer “taxed” by benefit reductions because benefits are already zero. Under a system like MFIP, which allows individuals to keep more benefits as they work, the implicit “tax” of benefit reductions continues up to higher earnings levels.

### SIDEBAR 4.3 *The Self-Sufficiency Project*

The Self-Sufficiency Project was a welfare reform experiment in the Canadian provinces of New Brunswick and British Columbia, starting in 1992. The traditional welfare system in Canada is quite generous compared to the U.S., but has few work incentives.

The Self-Sufficiency Project provided the treatment group with a large financial incentive to work, tied to a requirement for close to full-time work. Single-parent welfare recipients were eligible for the program if they had been on welfare at least twelve of the previous thirteen months. The experimental program agreed to provide, for a period of up to three years, half the difference between the participant's gross labor earnings and a target break-even level of \$37,000 (Canadian) in British Columbia and \$30,000 (Canadian) in New Brunswick. To receive the payment, however, participants had to work at least thirty hours per week and stay off the regular welfare program.

The program's payments are large. The average family supplement was more than \$800 per month.

The table shows how the program changes work incentives for a single parent with two children:

	British Columbia	New Brunswick
Net annual income at zero work	\$17,011	\$11,782
Net annual after-tax income under regular welfare program, thirty hours per week	23,078	14,847
Net annual after-tax income under SSP, thirty-hour work week	28,267	20,184
Hourly wage rate without taxes or transfers	6.24	5.53
Hourly wage rate under regular welfare program	3.89	1.96
Hourly wage rate under SSP	7.22	5.39

*Source:* Lin and Pan (1998, table G.2). All dollars are Canadian; multiplying by .75 would yield approximate equivalents in U.S. dollars. The calculations allow for all taxes and tax credits. Hourly wage rates without taxes and transfers are predicted wage rates in each province for households with these characteristics.

The program has large effects on effective wage rates. At typical wage rates for program participants, the regular welfare system and other Canadian taxes impose an effective tax rate of 35 to 65 percent. The incentives provided by SSP increase wage rates by more than \$3 per hour (Canadian), so that actual net wage rates are close to what they would be without any taxes or transfers.



#### *SIDEBAR 4.4 New York's Child Assistance Program*

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New York's Child Assistance Program has been run as an experiment in three New York counties since 1988. The program is a voluntary alternative to regular welfare for single parents who have a valid child support order. The base grant under CAP is one-third lower than regular AFDC, but the benefit reduction rate is lower. Under AFDC, benefits were reduced dollar for dollar with earnings. Under CAP, benefits are reduced by ten cents for every dollar of earnings up to the poverty line, and by sixty-seven cents for every dollar of earnings above the poverty line.

Because the program is voluntary, and because participants can switch back and forth from AFDC to CAP, it makes no sense to go on CAP unless a participant has some minimal level of earnings. Hence, the program is effectively providing subsidies targeted at individuals working a minimum number of hours.

In addition to financial incentives, CAP provides distinct welfare-to-work services. CAP participants are assigned case managers who do not work in the same offices as regular welfare case workers and who have lower caseloads than regular welfare caseworkers.

CAP's incentives are structured so that it pays more than regular welfare only after weekly work hours exceed ten to fifteen hours (Blank, Card, and Robins 2000). The benefits of CAP tend to be maximized at weekly work hours of twenty-five to thirty-five hours. According to Blank, Card, and Robins (2000), at thirty hours per week, CAP raises hourly wage rates about \$1.67 per hour.<sup>4</sup> MFIP raises wage rates by \$2.12 per hour, and SSP by \$3.59 per hour (U.S. dollars). The Canadian SSP program is far more generous than the U.S. CAP and MFIP programs.

CAP's effects on earnings were highest in Monroe County, next highest in Niagara County, and lowest in Suffolk County. The evaluators (Hamilton et al. 1996) attributed these different effects largely to differences in implementation. Monroe actively recruited harder cases and worked harder to motivate them. Suffolk made a relatively weak recruitment effort. However, it is also noteworthy that Monroe had the lowest unemployment rates during the experiment, and that the control group did worst in Suffolk County.

<sup>4</sup> The figures given by Blank, Card, and Robins (2000) differ slightly from those given in previous tables, no doubt because of some slight differences in the family being analyzed, the taxes considered, and so on.

## SIDEBAR 4.5     *The Center for Employment Training*

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Among training organizations, the Center for Employment Training has the best record of proven success. For example, in the Minority Female Single Parent demonstration, CET was the only one of four sites to show positive results. In the JobStart demonstration, CET had the strongest performance of thirteen sites.

CET was founded in 1967 in San Jose, California, as the response of Chicano activists to the employment problems of displaced farmworkers in East San Jose. From its beginning, CET received support from Silicon Valley firms, which were growing fast and facing labor shortages. CET established affiliates throughout the Southwest in the late 1970s.

CET's training model includes the following features:

- *Strong connections with specific industries in program design and implementation:* CET talks to firms to determine which entry-level jobs have good career prospects and expanding local demand, and then targets those occupations. Each local CET sets up an industrial advisory board that provides oversight and a technical advisory committee for each targeted occupation. Instructors have experience in the industry and occupation for which they provide training. Job developers find jobs for graduates and develop long-term relationships with local firms.
- *Short-term (thirty weeks), but intense (five days a week, eight hours a day) training.*
- *An open-door policy:* CET does not screen training applicants.
- *Open-entry and -exit training:* Individuals can start training any time and stop training when they have demonstrated competency.
- *Training organized like a workplace:* Trainees use a time clock and are expected to be on time. Instructors deal with attendance problems as if the training were a real job.
- *"Integrated" and "contextual" training:* Rather than focus on basic literacy and math skills first, followed by skills training, trainees immediately train for a specific occupation and are taught basic literacy as needed.
- *Addressing the social adjustment as well as the skills needs of trainees, providing counseling as needed:* Job placement assistance is provided by instructors, working with job developers.

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*Source:* This summary is largely based on Meléndez (1996).

## SIDEBAR 4.6 *Project Quest*

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Project Quest in San Antonio is a promising training program. Although it has not been subject to a random assignment evaluation, evidence suggests that it is successful. Before-and-after comparisons of Quest trainees suggest annual earnings gains of more than \$6,000. Individual case studies show that Quest has helped trainees overcome employment barriers. Interviews with community college staff and employers give high ratings to Quest.

Project Quest began operation in 1993. It was organized by two community-based organizations, both affiliated with the Industrial Area Foundations, an organization originally set up by the famed Chicago-based community organizer Saul Alinsky.

Project Quest's important features include:

- *Identifying occupations to target by talking with area employers to forecast employment needs:* Quest helped redesign training curricula for specific occupations to better meet employers' needs. More than half of Quest participants have trained for medical occupations.
- *Upgrading of some occupations through Quest intervention:* Quest discussions with banks have led to a training program for a new occupation, financial customer services. Quest discussions with hospitals led to enhanced training and higher wages for health unit clerks.
- *Training provided by community colleges:* Quest intervention led community colleges to redesign training to better fit what employers wanted.
- *Longer-term (seventeen months) training than is typical for low-income persons.*
- *Community organizations running Quest recruit participants:* Participants must have a high school diploma or GED and minimum math and reading scores.
- *Modest stipend for participants during training.*
- *Counseling provided both during and after training:* Quest provides counselors for trainees who help them with tutoring, dealing with teachers and employers, receiving financial support during training, and mandatory weekly group pep-talk/information sessions. Counselors are credited by community college staff and employers with helping trainees complete the program and stay on the job.

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*Source:* This summary is primarily taken from Lautsch and Osterman (1998).

#### SIDEBAR 4.7     *The Riverside County Welfare-to-Work (GAIN) Program*

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Based on a random assignment evaluation, the welfare-to-work program run by Riverside County, California, during the late 1980s and early 1990s was far more successful than supposedly similar programs in five other California counties. Riverside's three-year earnings gains for single mothers were more than twice the average effects for all the counties (Ricco et al. 1994, xxviii).

The important features of Riverside's GAIN program include:

- *A strong emphasis on jobs:* Ninety-five percent of Riverside case managers said their agency viewed quick job entry for welfare recipients as more important than education and training, whereas less than 20 percent of case managers in other California counties felt their agency emphasized quick job entry (Ricco et al. 1994, 54).
- *Performance standards for local welfare offices and individual case managers:* Unlike other counties studied, case managers, supervisory units, and district offices in Riverside County have job placement goals. Meeting goals is an important part of the evaluation of individual staff (Ricco et al. 1994, 54).
- *Job developers:* Unlike the other counties, every local welfare office in Riverside had its own job developer who worked with local employers to place welfare recipients in jobs.
- *Greater attention to staff selection and staff training:* The director of the county welfare department said that his three principles were "selection of staff, selection of staff, and selection of staff" (Bardach 1993, 26). Staff were selected based on whether they were positive people who could inspire welfare recipients to succeed. Line staff reported that the quality of staff training was extremely high (29).
- *Willingness to threaten and use sanctions (cutting benefits) for noncompliance with the program:* Riverside threatened sanctions for one-third of the caseload, more than the other counties did, although those actually sanctioned were under 7 percent (Ricco et al. 1994, 60).

Riverside's program does not appear to have been as successful in the mid-1990s as it was earlier. The program's impact on earnings was 40 to 50 percent less in the mid-1990s than it was earlier (Hamilton et al. 1997, 274). Riverside's program appears to be addressing a more disadvantaged clientele. In addition, Riverside's economy has deteriorated. In the late 1980s and early 1990s, although Riverside had the second highest average unemployment rate of the six counties studied, it also had by far the fastest employment growth rate of the six counties—an average annual rate of 4.9 percent from 1989 to 1992 (Ricco et al. 1994, 279). More recently, Riverside's employment growth has slowed down, to only 0.5 percent per year from 1992 to 1993 (Hamilton et al. 1997, 276).

TABLE 4.1 *Recent Estimates of the Effects of Welfare Reform on Labor Supply*

Study	Time Period	Labor Supply Effect	Brief Description of Methodology
Meyer and Rosenbaum (1998), adapted by Bartik (1998)	1993 to 1996	249,000	Estimated effects of state welfare policy variables on employment probability of single mothers versus childless single women
Bishop (1998)	1996 to 1998	571,000	Change in single-parent labor force participation rate between second quarters of 1996 and 1998 that cannot be explained by declining unemployment and trends
Daly (1997), adapted by Bartik (1998)	1996 to 1997	325,000	Labor force growth trends for women maintaining families versus U.S. population
McMurrer, Sawhill, and Lerman (1997b)	1996 to 2002	No recession: 832,000 Recession: 1,699,000	Calculated effects of 1996 welfare reform bill's escalating requirements for percentage of caseload that must be working
Chernick and Reschovsky (1996)	1996 to 2002	1,144,000	Calculated effects on state welfare spending of switching from matching grants under AFDC to block grant fixed in nominal terms under the 1996 bill.
Duncan, Harris, and Boisjoly (1998)	1996 to 2005	383,000	Calculated percentage of caseload that will reach five-year lifetime time limit on welfare receipt within eight years.
Bartik estimates (1998)	1993 to 1996	146,000	Estimated declines in welfare rolls that cannot be explained by unemployment, extrapolated into the future, and assumed work participation rates for participants
	1996 to 1997	300,000	
	1996 to 1998	597,000	
	1996 to 2002	1,032,000	
	1996 to 2005	1,256,000	
	1993 to 2005	1,402,000	

Source: Author's compilation.

Note: See Bartik (2000a) and Bartik (1998) for more details on the derivations of these estimates.

TABLE 4.2 *Economic Outcomes Over Time for Men and Women Ages Twenty-Five to Fifty-Four, by Educational Level*

	Males			Females		
	High School Dropout	High School Graduate, Not College Graduate	College Graduate	High School Dropout	High School Graduate, Not College Graduate	College Graduate
<b>Wages per hour</b>						
1979	\$13.91	\$17.23	\$21.91	\$8.85	\$10.99	\$15.06
1998	10.26	14.94	23.26	7.67	11.51	18.60
<b>Unemployment rates</b>						
1979	5.9	3.3	1.7	8.9	4.7	3.5
1998	6.8	3.4	1.7	9.9	3.8	2.1
<b>Employment rates</b>						
1979	83.1	92.7	95.4	44.5	60.9	71.6
1998	76.4	88.8	94.4	49.0	74.4	82.4
<b>Poverty rates</b>						
1979	13.7	4.4	2.6	23.0	7.0	3.1
1998	21.1	6.9	2.6	34.3	10.8	2.8
<b>Percentage of population in each education level</b>						
1979	22.0	53.2	24.8	22.1	61.8	16.2
1998	12.9	58.2	28.9	11.4	61.2	27.4

*Sources:* Author's calculations of weighted means from CPS-ORG for wages, unemployment, employment; March CPS for poverty.

*Note:* Education definitions for 1979 based on completed years of education, for 1998 on actual degrees obtained. Evidence suggests that a small percentage of those in 1979 who completed twelve years of education did not get a diploma; from 1998 data, this number is about 1.1 percent of the female population and 1.3 percent of the male population. Hence, this table underestimates slightly the trend toward fewer high school dropouts, and if 1979 data could be adjusted for this, outcomes would look slightly better for high school dropouts and high school graduates. But adjustment would not be large; degrading the 1998 data to better match 1979 definitions only slightly changes the 1998 means.

TABLE 4.3 *Educational Attainment by 1992 of 1980 High School Sophomores, by Family Background and Race*

	No High School Diploma	High School Diploma or GED, No Bachelor's Degree	Bachelor's Degree or Higher
Overall percentage	5.8	70.4	23.8
1980 socioeconomic status of student's family			
Low quartile	9.0	83.8	7.2
Middle two quartiles	3.9	75.4	21.6
High quartile	1.4	47.3	51.3
By race			
White, non-Hispanic	4.9	58.6	26.5
Black, non-Hispanic	6.9	71.1	12.2
Hispanic	11.9	78.5	9.9

Source: National Center for Education Statistics (1999, table 306).

TABLE 4.4 *Some Indicators of Educational Achievement and Skills in the United States*

Percentage of Adults Ages Nineteen to Twenty-Four at Lowest Literacy Levels <sup>a</sup>	
Prose level 1: Cannot locate single piece of information in short text if “distractors” present (more than one alternative is possible)	14
Document level 1: Cannot match single piece of information in chart/graph/figure if “distractors” present	14
Quantitative level 1: Cannot perform single arithmetic operation using numbers stated in a text	16
Mathematics and Reading Proficiency, Seventeen-Year-Olds in School, 1996 <sup>b</sup>	
Percentage of seventeen-year-olds <i>unable</i> to perform reasoning and problem solving involving fractions, decimals, percents, elementary geometry, and simple algebra	39.9
Percentage of students <i>unable</i> to search for specific information, interrelate ideas, and make generalizations about literature, science, and social studies material	18.6
Relative Achievement Levels of Twelfth-Grade Students from Lowest Quartile of Family Socioeconomic Status <sup>c</sup>	
Percentage of low quartile SES students in lowest quartile on math achievements tests	37.0
Percentage of low quartile SES students in lowest quartile on reading achievement tests	36.3

<sup>a</sup> Kirsch (1993).

<sup>b</sup> National Center for Education Statistics (1999, tables 112, 121).

<sup>c</sup> National Center for Education Statistics (1999, table 129). SES is socioeconomic status of student’s family as measured by index that combines parent education, parent occupation, and family income.



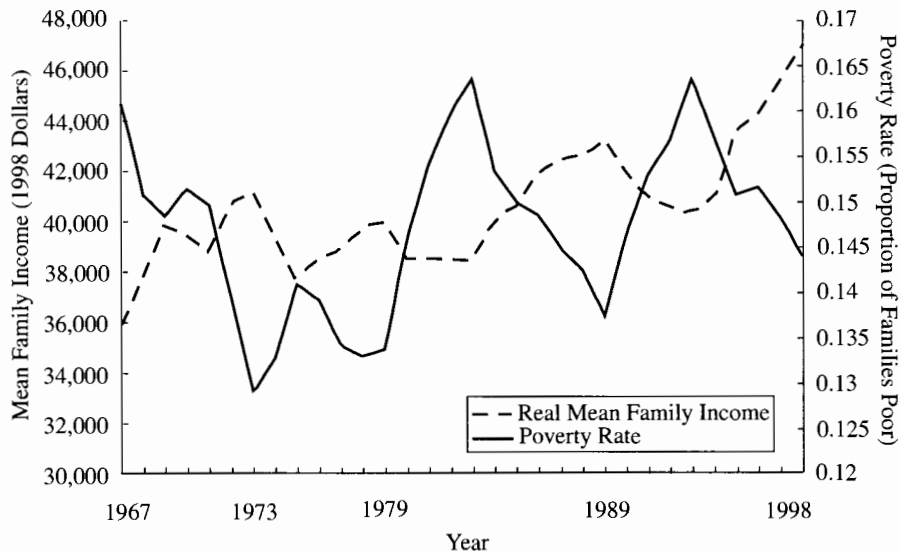
TABLE 4.5 *Effects of Training on Earnings*

Returns to Training for Voluntary Programs				
	Average for All Training Programs	JTPA Evaluation	Center for Employment Training	
Adult men	\$185 (s.e. = 467) (three programs)	\$1,008	—	
Adult women	1,345 (s.e. = 244) (five programs)	997	\$1,803 (Minority Female Single Parent demonstration)	
Youth	103 (s.e. = 207) (six programs)	- 178	4,194 (JobStart demonstration)	
Returns to Training for Mandatory Programs				
	Average	National Welfare-to-Work Evaluation	Portland (Oregon) Welfare-to-Work Program	
Adult welfare women	\$558 (s.e. = \$99) (three programs)	Labor force attachment approach: \$930 Human capital development approach: \$465	\$1,416	\$1,238

*Source:* Author's compilation.

*Notes:* All calculations are in 1998 dollars. All effects are for earnings in second year of program, or as close to second year as possible. Data summarizing many training programs come from Friedlander et al. (1997) and are simple averages of their estimated effects for each separate program (not study); the standard error reported is the standard error of this mean over the number of programs studied, that is, the number of observations is the number of programs. I included only programs in Friedlander et al. (1997) for which they listed (in their tables 1 and 3) classroom training or job search as first primary activity. I excluded programs that were listed as having work experience or on-the-job-training as first primary activities. However, all of the principal federal job training programs were included, even though these programs had OJT or work experience components. JTPA data also are from Friedlander et al. (1997). CET data are from Cave et al. (1993) and Zambrowski et al. (1994). National welfare-to-work data are from Hamilton et al. (1997) and are simple averages of second-year effects in Atlanta and Grand Rapids. Riverside results are from Ricco et al. (1994). Portland results are from Scrivener et al. (1998).

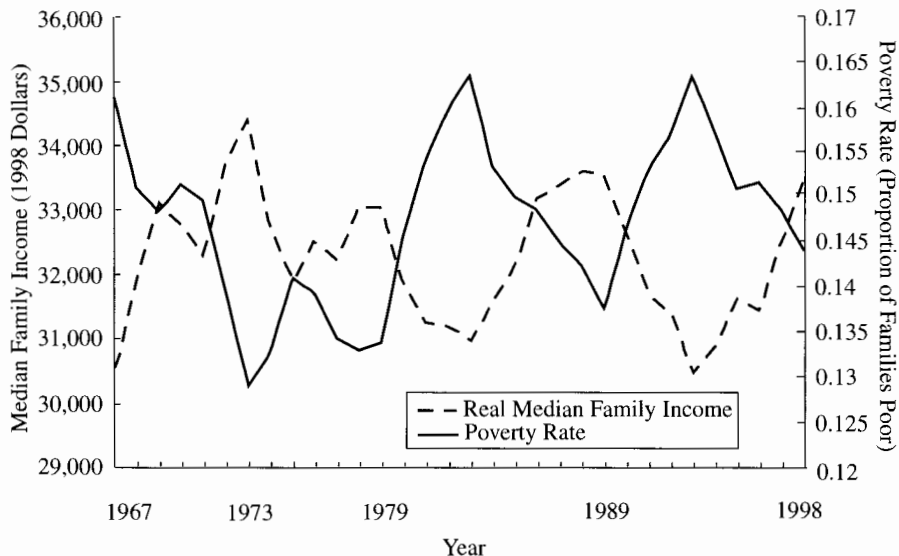
FIGURE 5.1 Mean Family Income and Poverty, 1967 to 1998



Source: Data are derived from March CPS.

Note: Poverty rate is "family" poverty rate, but with unrelated individuals included as "families." Real mean family income uses this same family definition.

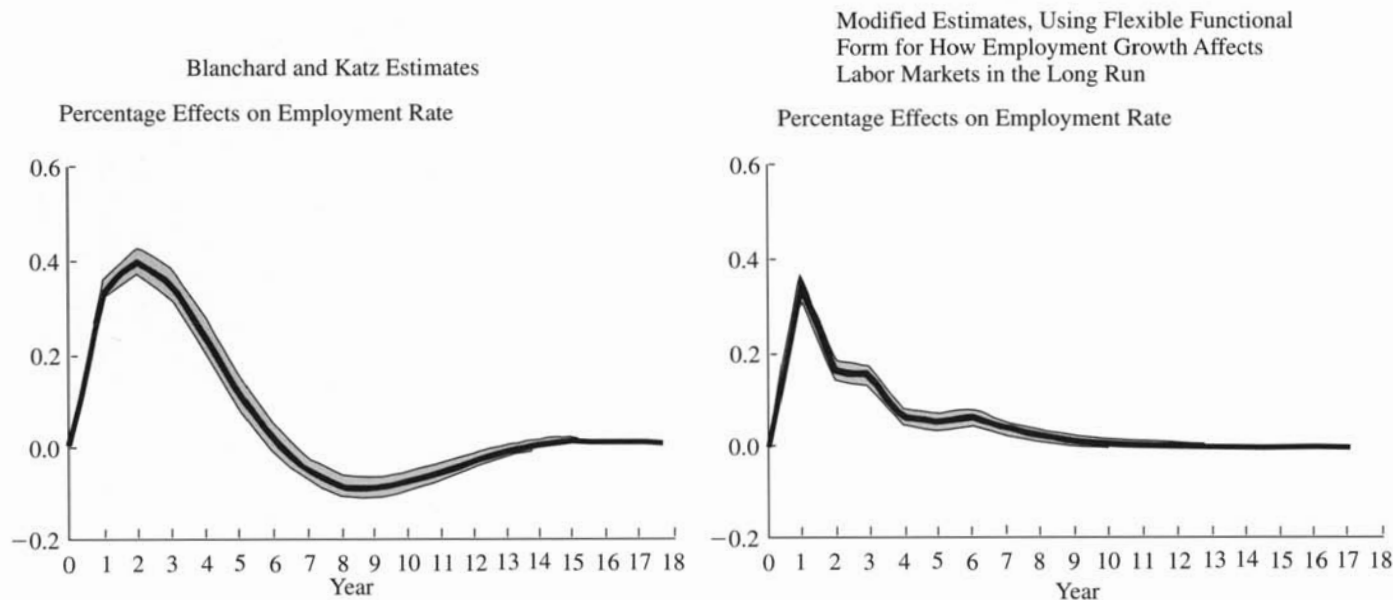
FIGURE 5.2 Median Family Income and Poverty, 1967 to 1998

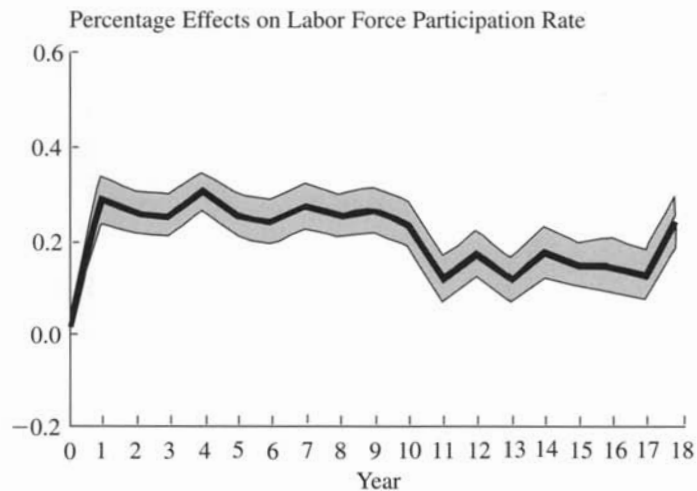
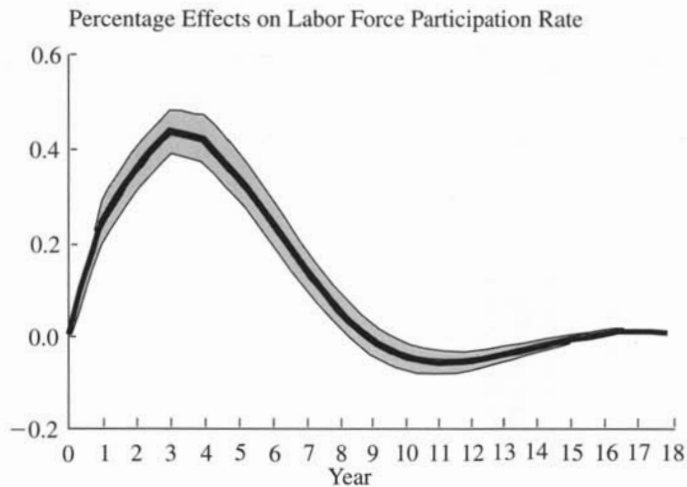


Source: Data are derived from March CPS.

Note: Poverty rate is "family" poverty rate, but with unrelated individuals included as "families." Real median family income uses this same definition.

FIGURE 5.3 *Effect over Time of a 1 Percent Increase in a State's Employment on State Employment Rate and Labor Force Participation Rates: Comparing Blanchard and Katz's (1992) Estimates with Modified Estimates That Use More Flexible Specification of Possible Long-Run Effects*





Source: Derived from Bartik (1993a).

Notes: Both sets of estimates use the same database. Blanchard and Katz (1992) allow for labor force participation and employment rates to be affected by only two lagged years of state employment; Bartik (1993a) allows for effects from up to seventeen lagged years of state employment. Figures show changes over time in employment rates and labor force participation rates in response to once-and-for-all 1 percent shock to employment. Years are years after employment increase. Employment rate is defined as employment/(labor force). One standard error to either side of point estimates is shown by the thin lines in the figure.

## *SIDEBAR 5.1     The Supported Work Program*

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The Supported Work Program was an experimental subsidized work program designed to increase the long-run employability of “hard-core” poor individuals. The experiment was run from 1975 to 1979 by nonprofit organizations at fifteen sites around the United States, with more than 10,000 participants. The four groups targeted by the program were long-term welfare recipients, ex-addicts, ex-offenders, and young high school dropouts.

Supported Work provided a structured work experience for up to one year, with job assignments involving gradually increasing work demands, close supervision by program staff, and peer support. Supported Work projects were usually developed and run by the local program operator, although some participants were placed at other organizations. Jobs were typically service occupations for females and construction occupations for men. One hundred percent of participant wages were subsidized, although sometimes the paid wages were financed in part by diverted welfare benefits. The wages paid were supposed to be less than the local market wage for entry-level workers. In addition to work experience, Supported Work programs provided counseling and sometimes training. After participants had acquired work experience through Supported Work projects, local program operators attempted to place them at unsubsidized jobs.

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*Sources:* Manpower Demonstration Research Corporation (1980); Kemper et al. (1981); Couch (1992); Grossman et al. (1985).

## *SIDEBAR 5.2    The New Hope Program*

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New Hope was an experiment that guaranteed employment at a living wage to low-income residents of two Milwaukee neighborhoods. The experiment was run from 1994 to 1998 in two Milwaukee zip codes. Those eligible were all adults in those zip codes whose family income was below 150 percent of the poverty line.

For individuals who worked at least thirty hours per week, the program provided:

- earnings supplements to increase family income to the poverty level;
- subsidized health insurance;
- subsidized child care.

For individuals unable to find full-time employment, the program provided community service jobs at nonprofit organizations for up to six months at a time, and for a maximum of twelve months during the program. In addition, project representatives provided counseling and mentoring.

Early results from the random assignment evaluation of the program indicate that New Hope's impacts depended on whether program participants were employed full-time at program entry. Among the two-thirds not employed full-time upon program entry, the program reduced the percentage never employed from 13 to 6 percent, increased market earnings by almost \$700 per year, increased income by more than \$1,300 per year, and reduced the percentage in poverty by an average of 7 percent. Among those already employed full-time at program entry, the program modestly reduced earnings owing to cutbacks in overtime hours. However, the cutting back on overtime hours apparently led to some improvements in parenting quality, as judged by surveys of program participants, their children, and their children's schools.

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*Sources:* Bos et al. (1999); Brock et al. (1997).

### *SIDEBAR 5.3      The Summer Youth Employment Program*

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The Summer Youth Employment Program was originally enacted as part of the War on Poverty initiatives of President Johnson in 1964. The program provides summer jobs at the minimum wage to economically disadvantaged youth at a variety of public and private contractors, with jobs typically lasting about nine weeks and averaging twenty to thirty hours per week. The program peaked in size in 1978 at 1.2 million participants. During the 1980s and 1990s, the program fluctuated between 400,000 and 800,000 participants per year, at an annual cost (in 1998 dollars) that fluctuated between \$0.4 billion and \$1.4 billion. The program was folded into the general block grant for employment and training provided to states and local workforce investment boards under the Workforce Investment Act enacted in 1998.

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*Sources:* Ellwood and Crane (1984); U.S. House of Representatives (1998); U.S. Department of Labor web site.



## SIDEBAR 5.4    *The Youth Incentive Entitlement Pilot Program*

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YIEPP was one of several experimental youth employment programs enacted under the 1977 Youth Employment and Demonstration Projects Act and eliminated once the Reagan administration took office in 1981. What was unique about YIEPP was that it guaranteed job availability by creating jobs, with a condition for getting the job: enrollment in a program to get a high school degree. Under YIEPP, local job training sponsors were responsible for ensuring that eligible youth were provided with *guaranteed* minimum-wage jobs, part-time during the school year and full-time during the summer, with 100 percent of the wage, of these jobs paid for by YIEPP. The program was operated as an entitlement for all disadvantaged youth in the seventeen project areas who enrolled in school or agreed to go back to school or enroll in a program to get a GED. Seventy-six thousand youth participated in these programs, on average for fifteen months. Total program cost was about \$600 million in 1998 dollars. Jobs created were in both the public sector (70 percent) and the private sector (30 percent).

Evaluation of YIEPP was based on:

- interviews and surveys of youth, employer, and training sponsor views of the program;
- comparison of youth employment, earnings, and school enrollment in three project areas with three matched areas, with the matching based on similarities in local labor markets.

The evaluation suggested that it is feasible to implement a large-scale job guarantee for disadvantaged youth, in that sufficient jobs can be created and disadvantaged youth will take such jobs. Program participation was high, particularly among disadvantaged black youth, among whom more than 60 percent of those eligible participated. The program attracted more interest from in-school youths than from high school dropouts. Program sponsors were able to encourage significant private-sector participation, although job development costs were high because most private businesses were not interested in hiring disadvantaged youths. Surveys of both employers and youth participants suggest that most participants were able to do productive work rather than “make-work,” and that one-fifth of participating private businesses offered regular jobs to youths they had employed through the program. Comparing project areas with matched areas, the program significantly increased the employment rates and earnings of eligible youth during the program’s operation, particularly for black youth. However, the program had no effect on school enrollment. A three-month follow-up suggested that effects on earnings persisted even after the program’s guaranteed jobs were gone.

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*Sources:* Gueron (1984); Betsey, Hollister, and Papageorgiou (1985).

TABLE 5.1 *Estimated Effects of Regional Economic Conditions on Regional Poverty Rates in the United States, 1967 to 1997*

Independent Variables	Full Sample Period: 1967 to 1997	Early Sample Period: 1967 to 1979	Middle Sample Period: 1980 to 1989	Late Sample Period: 1990 to 1997
Unemployment rate	0.37 (0.06)	0.65 (0.11)	0.37 (0.08)	0.58 (0.18)
Median family income	-0.66 (0.04)	-0.65 (0.05)	-0.47 (0.05)	-0.69 (0.08)
Mean family income	-0.47 (0.04)	-0.52 (0.06)	-0.44 (0.05)	-0.59 (0.09)

*Source:* Author's estimates based on Bartik (2000b). Model derived from Blank and Card (1993).

*Notes:* Numbers in table show effects of independent variable measuring overall economic conditions in region on regional poverty rate. Standard errors of this coefficient estimate are in parentheses below estimated coefficient. Each row-column combination corresponds to a single regression. The observations in this regression are on region-year cells. The regressions differ in the independent variable used to measure region-year aggregate demand and in the years used in the estimation. The dependent variable in each regression is the mean poverty rate for a particular region-year cell. The independent variables in each regression are of three types: a complete set of year dummies; a complete set of region dummies; and a single variable measuring region-year aggregate demand and defined as some mean or median variable for each region-year cell. The means or medians for region-year cells are calculated by the author from various years of the March CPS. The twenty-one regions here are either an individual state or some grouping of states. The grouping here is arbitrarily defined based on what groupings of states one can calculate means or medians for using all the years of the March CPS from 1967 on. In calculating median or mean "family" income, I include as "families" what the Census Bureau calls "unrelated individuals." Poverty rate and unemployment rate are in percentages. Median and mean family income are in thousands of 1997 dollars. Statistical tests were done to determine whether the estimated effects in this table for particular subperiods differed from the estimated effects for the entire period. Only two subperiods are significantly different from the overall period: estimated effects of unemployment on poverty are significantly higher in the period 1967 to 1979 than for the entire period; the estimated effects of median family income on poverty are significantly smaller (in absolute value) for the period 1980 to 1989 than the estimated effect for the entire period from 1967 to 1997.

TABLE 5.2 *Effects of Lower Unemployment*

Dependent Variable	Percentage Change in Dependent Variable for One-Percentage-Point Reduction in Unemployment Rate
Aggregate or overall average variables	
GNP	2 to 3
Labor force participation rate	0.3 to 0.6
Wage rate	0.5 to 1.5
Occupational upgrading	0.2 to 0.7
Average real earnings	1.5 to 3.5
Average real income	0.9 to 1.5
Distributional measures	
Income share of lowest income quintile	0.02 to 0.1 increase in percentage point share of national income. At mean share of 3 to 5 percent, income of lowest quintile increases by 1 to 3 percent more than average.
Poverty rate (change in rate points)	-0.3 to -0.9.
Percentage effect on earnings of bottom income quintile versus effect on overall earnings	Earnings effect on lowest income quintile is 2 to 6 percent greater than that for average family.
Various labor market variables, ef- fects on different education groups	Percentage effects are greater on less-educated than more-educated individuals for employ- ment rate and earnings; unclear for wages. One-point reduction in unemployment in- creases earnings of high school dropouts by 1 to 3 percent more than earnings of college graduates.
Various labor market variables, ef- fects of overall lower unemploy- ment on blacks versus whites	Most studies find greater effects on blacks than on whites in percentage terms, although the magnitude of differences varies greatly. Most of the greater effects appear to be due to ef- fects on hours worked, not wage rates.

Source: See chapter 5, note 3, for more details.

TABLE 5.3 *Effect of 1 Percent Lower Unemployment on Mean Annual Earnings, Mean Annual Income, and Share of Total Family Income, by Family Income Quintile, 1963 to 1997*

Measure	All Families	Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5
Estimated effect of lower unemployment on mean earnings (standard error)	474 (55)	100 (16)	414 (49)	472 (63)	632 (72)	751 (152)
Mean earnings	32,477	2,367	11,977	25,375	42,236	80,428
Implied percentage effect	1.46	4.22	3.46	1.86	1.50	0.93
Estimated effect of lower unemployment on mean income (standard error)	473 (57)	131 (21)	351 (45)	445 (54)	571 (68)	868 (159)
Mean income	38,662	6,879	18,601	31,353	47,437	89,039
Implied percentage effect	1.22	1.90	1.89	1.42	1.20	0.97
Estimated effect of lower unemployment on quintile's percentage share of total income (standard error)	—	0.02 (0.01)	0.06 (0.02)	0.03 (0.01)	-0.01 (0.02)	-0.09 (0.04)

*Source:* Author's estimates using March CPS data (Bartik 2000b). Model derived from Blank and Card (1993).

*Notes:* Standard errors are in parentheses. Earnings data were available only for 1967 to 1997. Observations are on region-year cells, with the United States divided into twenty-one groupings of states. Quintiles are defined by family income, with an "unrelated individual" included as a "family." All dollar figures are in 1997 dollars. All regressions and calculations are weighted by 1997 population for region. All regressions include year and regional dummies.

TABLE 5.4 *Effects of 1 Percent Lower Unemployment on the Employment Rates of Different Education and Gender Groups of Prime Working-Age Individuals*

	Effects on Men	Effects on Single Women	Effects on Married Women
High school dropouts	0.676 (6.92)	0.890 (5.35)	0.728 (5.73)
High school graduates	1.030 (33.35)	1.127 (16.10)	0.803 (13.44)
College graduates	0.401 (12.83)	0.393 (4.67)	0.272 (3.52)

*Source:* Author's estimates.

*Notes:* The table is based on nine different regressions; each cell shows effect from a separate regression. *T*-statistics are in parentheses. Both employment rates and unemployment rates are measured in proportions. All regressions also include a full set of year dummies and state dummies. Regressions are weighted by estimated 1998 total state population of everyone ages twenty-five to fifty-four. Data span fifty-one states (including D.C.), and all years from 1979 to 1998. Data are weighted state means derived from CPS-ORG.

TABLE 5.5 *Effects of Lower Unemployment on Employment of Various Groups, Compared to "Needed Jobs"*

Group	Number of Needed Jobs (In Millions)	Number of Jobs Created by 1 Percent Lower Unemployment (In Millions)	Number of Jobs Created by 4 Percent Lower Unemployment (In Millions)
Male high school dropouts	0.596	0.050	0.198
Male high school graduates	1.518	0.341	1.365
Single female high school dropouts	1.082	0.027	0.108
Single female high school graduates	1.975	0.149	0.597
Total	5.171	0.567	2.268

*Source:* Author's estimates and calculations.

*Notes:* Figures for needed jobs are directly taken from table 3.2 and represent the number of jobs needed for employment rates for these four groups to change from 1998 levels to the 1979 levels of the same education group for white non-Hispanic males. Figures for job creation due to lower unemployment are taken by calculating, based on table 5.4, how much lower unemployment would increase the employment rate of each group, and then multiplying this by the change in unemployment rate and estimated size of each group in 1998.

TABLE 5.6 *Unemployment, Labor Force Participation, and Potential Labor Force Participants Among Different Family Income Quintiles, 1997 to 1998*

Income Quintile	Unemployment Rate	Number of Unemployed (In Millions)	Labor Force Participation Rate	Number of Labor Force Participants (In Millions)	Number of Adults Sixteen and Over (In Millions)
1 (Lowest income)	15.8	1.762	40.5	11.150	27.502
2	8.1	1.510	53.7	18.674	34.715
3	4.8	1.273	66.1	26.522	40.122
4	3.6	1.316	76.2	36.546	47.989
5 (Highest income)	2.4	1.057	80.1	44.039	54.970

*Source:* Author's calculations based on the March 1998 CPS.

*Notes:* Income quintile status is assigned based on 1997 family income, with single individuals counted as "families" (unlike Census Bureau conventions). Of course, an equal number of families are in each quintile. Unemployment and labor force status are based on employment status as of the March 1998 interview.

TABLE 5.7 *Net Distributional Effects of Local Economic Development Policies Under Alternative Cost Scenarios*

Quintile	Real Income Increase	Real Income Equivalent of Real Estate Gains	Business Tax Cuts Financed by Increases in Personal Taxes		Business Tax Cuts Financed by Reduced Welfare Spending	
			Tax Cost	Net Effect	Cost	Net Effect
1 (Lowest income)	0.787	0.071	-0.257	0.601	-2.861	-2.003
2	0.477	0.069	-0.192	0.354	-0.420	0.126
3	0.396	0.068	-0.169	0.295	-0.073	0.391
4	0.276	0.069	-0.158	0.187	-0.022	0.323
5 (Highest income)	0.166	0.086	-0.173	0.079	-0.007	0.245
Average family	0.286	0.077	-0.174	0.189	-0.174	0.189

Source: Bartik (1994a, table 3).

Notes: The elasticity of employment with regard to business tax rates is assumed to be  $-0.25$ , based on Bartik (1991). Real income increases are derived from column 2 in table 1 of Bartik (1994a). All numbers show benefits and costs, as percentages of average income in that particular quintile, of some policy that increases MSA employment by 1 percent. The policy is assumed to be a business tax cut, financed by either increasing personal taxes or cutting welfare spending. The net effect under each policy scenario sums real income gains, the annual income equivalent of real estate capital gains, and the cost of financing the policy. For example, under the tax financing scenario, the lowest income quintile's average per family income increases by 0.787 percent, its real estate value increases by an amount equivalent to a 0.071 percent increase in annual income, and its taxes increase by 0.257 percent of annual income, for a net effect of increasing the lowest income quintile's income by 0.601 percent. When the business tax cut is financed by reduced welfare spending, real income and real estate increases the same, but reduced welfare spending cuts the lowest income quintile's net income by 2.861 percent. Average family totals are calculated as weighted sums of quintile figures, using quintile shares as weights. These quintile percentage shares are: Q1, 3.91; Q2, 9.84; Q3, 16.46; Q4, 24.96; Q5, 44.83.



TABLE 5.8 *Impact of Small-Scale, Policy-Induced Demand Shocks on Employment and Wage Rates of Program Participants*

Program	Ratio of Employment and Earnings Effects on Participants to Program-Related Employment and Earnings of Participants, In-Program Period	Wage Effects
Supported Work	0.92 for AFDC mothers 0.68 for ex-addicts 0.69 for ex-offenders 0.85 for youth <sup>b</sup>	Changes due largely to program policy: 17 percent for AFDC mothers <sup>a</sup> – 17 percent for ex-addicts – 10 percent for ex-offenders 0 percent for youths
OJT component of JTPA	1.57 for adult women 0.77 for adult men 5.15 for female youth – 1.83 for male youth non-arrestees <sup>c</sup>	Not available for this subgroup, but overall wage effect of JTPA appears very modest, perhaps 2 or 3 percent
New Hope <sup>d</sup>	0.680	Reduces wage slightly, largely owing to wage supplement policy

*Source:* Author's compilation.

<sup>a</sup> These are taken by dividing average monthly earnings for seven to nine months after enrollment by average monthly hours for experimental group, and for control group, separately (MDRC 1980).

<sup>b</sup> These are ratios of experimental-control differences in earnings, seven to nine months after enrollment, divided by program earnings during same time period, as reported in MDRC (1980).

<sup>c</sup> These figures are ratios of earnings gains for experimental versus control group for months 1 through 6 after enrollment, to twice the total OJT earning subsidies (because subsidies are half of wages) provided to that group, as reported in Orr et al. (1996). The analysis here is complicated because clearly the OJT group received more than this service, and some of the OJT subsidy occurs after the six-month period.

<sup>d</sup> The ratio of in-program earnings effects to program earnings is derived as the two-year effect on average earnings per participant of \$646, versus two-year provisions of \$945 in earnings from a community service job per participant (Bos et al. 1999).

TABLE 5.9 *Short-Run Market Displacement and Wage Effects of Policy-Induced Shocks to Labor Demand*

Program	Ratio of Marketwide Effects on Employment of Target Group to Program-Related Increase in Employment: In-Program, Short-Run Effects	Wage Effects
Estimates from actual programs		
YIEPP	0.67 for school year jobs	2 percent for out-of-school youth
	0.44 for summer jobs <sup>a</sup>	- 1 percent for in-school youth
Summer Youth Jobs	0.67 for nonwhite youth <sup>a</sup>	n.a.
Simulated effects		
Partial equilibrium model of appendix 1, using assumed parameters <sup>c</sup>	0.64	Elasticity of 0.21
General equilibrium wage curve model of appendix 2, using estimated parameters <sup>d</sup>		Wage elasticities of:
	0.65 for single mothers	0.14 for single mothers
	0.58 for other less-educated women	0.95 for other less-educated women
	0.64 for less-educated men	1.27 for less-educated men

<sup>a</sup> Author's compilation. Farkas et al. (1982, 125, table 5.8). Derived from estimated program effect in site city versus comparison city, divided by program employment in site.

<sup>b</sup> Ellwood and Crane (1984).

<sup>c</sup> Source: Table A1.1.

<sup>d</sup> Table A2.12. Note that results for single mothers are for a group that is a small portion of the labor market, only about 4 percent of the overall population. Results for other less-educated women and for less-educated men are for groups that are a much larger portion of the labor market, about 36 percent of the population for less-educated women, and 38 percent for less-educated men (see table A2.2).

TABLE 5.10 *Earnings Effects Five Years Later of Employment for 2,000 Hours per Year, at Mean Wage for Each Group, Compared to Zero Annual Work Hours*

Group	Earnings Effects Five Years Later	Earnings Effects as Proportion of Initial Earnings Shock
Female high school dropouts	\$1,617 (257)	0.117
Female high school graduates (but not college graduates)	4,674 (283)	0.246
Male high school dropouts	3,191 (1,077)	0.125
Male high school graduates	4,559 (1,775)	0.133

*Source:* Estimates are based on model and data described more fully in Bartik (2000d) and appendix 8. Standard error of estimation is in parentheses. Estimates are based on panel data on individuals from the PSID, 1975 to 1991.

*Notes:* All estimates include only individuals for whom we have fifteen years of observations, and who were household head or wife during each of those fifteen years. Model regresses annual individual earnings on a number of control variables and three variables reflecting employment experiences of that individual five years before: a dummy for whether he or she worked at all during the year; the annual work hours during that year; the ln of the wage rate (annual earnings/annual hours) for those who worked five years before. Those not working are arbitrarily assigned a mean wage, but this is of no consequence because of the inclusion of the dummy for whether the individual worked five years before. Control variables include: fixed effects for year and state of residence; fixed effects for each individual; the individual's average earnings during the period from six to eight years before; discrete variables for whether the individual is married, whether the individual is black, whether the individual is Hispanic, and whether the individual has any children; the number of children; the years of education of the individual; the standard experience proxy (age - education - 6); experience squared. The model is estimated separately for the four groups included in the table. The number of individuals/observations, where an observation is an individual by year cell, are: female high school dropout: 407/6,732; female high school graduate: 665/10,940; male high school dropout: 364/5,967; male high school graduates: 217/3,530. The mean wage rates used to generate these simulations are: female high school dropout: \$6.93 per hour; female high school graduate: \$9.50; male high school dropout: \$12.77; male high school graduate: \$17.17. The simulations consider simultaneous change in two variables: the dummy for working changes from zero to one, and the annual work hours changes from zero to 2,000. The wage rate in this simulation does not change because those not working are arbitrarily assigned the mean wage anyway.

TABLE 5.11 *Estimates of Long-Term Average Effects of the Supported Work Program on the Annual Earnings of Female Welfare Recipients*

Year	Annual Earnings Effect (1998 Dollars)
1982 (Four years after program was over)	\$1,093 (570)
1983	1,140 (498)
1984	1,313 (503)
1985	1,033 (518)
1986 (Eight years after program was over)	938 (558)

*Source:* Derived from Couch (1992, table 1).

*Note:* Standard errors are in parentheses.

Created in 1935, the WPA was by far the largest work relief program of the New Deal, spending more on job creation than all other New Deal programs put together (Kesselman 1978, 158). President Roosevelt thought of WPA work relief activities as a long-term activity of the federal government (Kennedy 1999, 249–50). The experience of the lengthy Great Depression suggested that the private economy would not provide an adequate numbers of jobs.

Rather than using contractors, the WPA directly administered its projects—using an average of thirty thousand employees in many local offices (Briscoe 1972, 98)—because of the perceived difficulties in getting contractors to hire the needy (Howard 1943, 151–53). A state or local government match was required on WPA projects—originally an average of a 19 percent local match, and later 25 percent (99). Seventy-eight percent of WPA projects were public works projects, including LaGuardia Airport in New York City (106).

Those hired for the WPA had to meet criteria for being considered needy, including having exhausted any unemployment benefits. It is clear that most WPA hires would not have been employed nearly as much during the Depression without the WPA. When Congress mandated an eighteen-month limit to continuous WPA employment in 1939 (with a minimum of thirty days before workers could reapply to WPA), only 13 percent of these laid-off WPA workers were employed in regular jobs when surveyed two or three months later (Kesselman 1978, 198, 203).

Workers were paid a security wage, which was supposed to exceed what could be obtained from direct cash relief yet be somewhat below prevailing private wages for that particular type of job. However, the WPA was openly willing to pay above what were thought to be unfairly low wages, particularly in the rural South. Average wages paid would be equivalent to \$600 to \$650 per month in 1998 dollars (Howard 1943, 158–67, 181).

The WPA was administered by local WPA offices, some of which gave substantial control over hiring to local political machines. The Memphis political boss Edward Crump required WPA workers to contribute to the local Democratic machine, and New Jersey WPA workers were required to contribute 3 percent of their wages to Frank Hague's Democratic machine (Kennedy 1999, 253).

The WPA was controversial from its beginnings. Businesses objected that the WPA was luring workers away from private-sector jobs, or producing goods or services in competition with the private sector. The WPA often had to abandon slum clearance projects because of objections from building-wrecker businesses and rat extermination projects because of opposition from exterminators (Howard 1943, 134, 166). A 1939 public opinion poll found that the WPA was cited more than any other program as both the “greatest accomplishment” of the Roosevelt administration and the “worst thing” the administration had done (Howard 1943, 105).

With conservatives beginning to take control of Congress after the 1938 elections, the WPA and many other New Deal programs were increasingly restricted in scope and size. With unemployment almost eliminated by World War II, the WPA was abolished by Congress in 1943 (Kennedy 1999, 783).

The Comprehensive Employment and Training Act (CETA) of 1973 was originally adopted as a compromise between President Nixon's desire to decentralize federal training programs into a block grant to state and local governments and congressional interest in expanding public-service employment. CETA set up block grants for training and PSE, distributed by formula to local agencies. Originally, CETA emphasized training over PSE, with \$2.0 billion for PSE and \$3.3 billion for training in fiscal year 1974 (1998 dollars) (Barnow 2000). However, as time went on, PSE took over the CETA program owing to the large recession of 1974 to 1975, the political weakness of Republicans after Watergate, and campaign commitments by President Carter. By fiscal year 1978, CETA public-service employment had expanded to \$11.8 billion, versus \$4.8 billion for CETA training programs (Barnow 2000).

CETA PSE funding initially was quite generous with few restrictions. Anyone who had been unemployed at least thirty days was eligible to be hired under CETA, and the federal government would pay up to \$33,000 annually, with the local government permitted to supplement this amount. No duration limit was imposed on positions or on individuals funded by CETA. This led to the criticism that local governments were using this program to hire laid-off city workers, or at least workers very similar to those they would have hired anyway. The program was perceived as allowing local governments to hold down taxes rather than helping the disadvantaged get jobs (Cook et al. 1985, ch. 1; Mucciaroni 1990, ch. 7).

In response to criticisms, over time CETA PSE was made less generous and more restrictive in who could be hired and under what terms. In 1976 new CETA positions were limited to less than one year, and half of all CETA participants were required to be low-income and long-term unemployed. In 1978 federal law required that all CETA participants be low-income and long-term unemployed or welfare recipients; furthermore, individual participants couldn't be funded under CETA for more than eighteen months during a five-year period; finally, local governments could not supplement the federal CETA salary (Cook et al. 1985, 14–15).

Because of these restrictions, state and local governments became much less enthusiastic about CETA PSE. This doomed the program in the face of political opposition and bad publicity. Conservatives continued to oppose PSE on principle, and the CETA program received bad publicity for program abuses. As one congressional aide said in the late 1970s, "CETA has become a four-letter word" (Mucciaroni 1990, 162). According to Cook and his colleagues (1985, 128): "Toward the end of [the 1970s], when tighter restrictions on wages and participant eligibility limited the range of services that could be provided, we found that the program had lost much of its appeal to state and local officials. It is mainly because of this change that the Reagan administration's 1981 proposal to eliminate PSE was not more vigorously opposed." CETA was replaced by the Job Training Partnership Act (JTPA) in 1982, and JTPA forbade the use of funds for public-service employment.

### *SIDEBAR 7.3    The W-2 Program*

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Wisconsin's W-2 program is one of the most radical state welfare reforms. W-2 requires most welfare recipients to work in positions created by the program. Welfare benefits are considered a "wage" paid to a needy person for working or preparing for work. For example, W-2 payments do not vary with family size, just as wages paid by employers do not vary with family size.

W-2's enactment was in part a political accident and in part a reflection of the state's political culture. In an attempt to outdo Republican Governor Tommy Thompson in reforming welfare, Democrats in the Wisconsin legislature proposed in 1993 to abolish AFDC and replace it with a new welfare system. Governor Thompson called the Democrats' bluff by agreeing to the abolition of AFDC, and his administration then designed the W-2 with help from the conservative Hudson Institute (Kaplan 1998). W-2 was enacted in 1996 and went into effect on September 1, 1997. Its enactment reflects a state political culture in which Republicans are willing to use government aggressively and many Democrats support work requirements for the poor (Mead 1999).

Although W-2 includes several possible assignments of clients, the most common assignment (about half of all clients) is to community service jobs (CSJs). CSJ participants must work twenty to thirty hours per week, in exchange for which they receive \$673 per month in W-2 benefits (plus food stamps). W-2 payments are reduced when clients have unexcused absences from their community service positions (Wiseman 1999). Local W-2 agencies develop the CSJs, generally at nonprofit employers but sometimes also at for-profit employers, public agencies, or new businesses owned by the W-2 agency (Kaplan and Rothe 1999).

W-2 has been criticized by "neoliberals" such as Mayor John Norquist of Milwaukee for not creating "real jobs," in that CSJ participants are ineligible for the Earned Income Tax Credit and do not participate in social security (Norquist 1998). More conventional liberal voices, such as the Institute for Wisconsin's Future (1998), criticize the CSJ program by arguing that these jobs provide little on-the-job-training and make it difficult for participants to have time for education and training.

## *SIDEBAR 7.4 New York City's Work Experience Program*

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New York City is currently running the largest work experience program for welfare recipients. The program began with "home relief" recipients, the term in New York State for what in other states is called "general relief": welfare benefits for adults who do not have custody of a child and do not qualify for regular welfare programs. The program has since been extended to welfare recipients with children.

About half of New York City's work experience participants are placed in the city parks or the sanitation department (Lerman and Rosenberg 1997). Most do menial jobs. General relief recipients' work hours are determined by dividing their welfare benefits by the minimum wage, whereas welfare recipients work twenty hours per week. The types of jobs, work expectations, and implicit wage rates do not seem unusual compared to work experience programs in other states.

What is unusual about New York City's program is its size. The program has about 36,000 participants in an average month, about half of whom are welfare recipients with children and half of whom are general relief recipients. Although the welfare recipients represent only 8 percent of New York City's welfare caseload, the 36,000 work experience participants loom large compared to the total New York City workforce of 175,000. Particularly in a city that has cut back city employment in recent years, the large size of the program raises questions about whether work experience participants are replacing regular New York City workers.

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*Source:* Most of this discussion is based on Ellwood and Welty (2000).



EOPP was originally designed to test the guaranteed jobs component of President Carter's 1978 welfare reform proposal, the Program for Better Jobs and Income (PBJI). PBJI envisioned requiring welfare recipients to work or enter training, with PBJI providing funding for a guaranteed public-service job or training position. PBJI planning assumed that the creation of 1.4 million jobs and training slots would be required, and EOPP was designed to test the feasibility and impacts of these guaranteed jobs.

However, Congress rejected PBJI, and EOPP was refocused on testing the impact of job search assistance, although a subsidized employment component was also included when EOPP began enrollment in mid-1979. After the Reagan administration took office in 1981, the subsidized employment component of EOPP was terminated, and EOPP was scaled back and then ended in 1981 (Brown et al. 1983, ch. 2).

EOPP was administered by local job training agencies at fourteen sites around the country. Those eligible were families that had children and were on welfare, eligible for welfare, or low-income. EOPP enrollees initially participated in five to eight weeks of self-directed job search, backed up by job club meetings and counseling. If the job search was unsuccessful, an EOPP family's "primary earner" was eligible for subsidized employment and training for up to one year. Two-thirds of those enrolled in subsidized employment got public-service jobs, one-fourth got classroom training, 14 percent were placed in a work experience position (working without pay), and 7 percent received on-the-job-training with a private employer. The average length of enrollment in public-service jobs was five months. About one-third of those participating in subsidized employment and training left it to take a regular job.

Although EOPP was supposed to provide an entitlement to a job, in practice the program did not deliver the entitlement at most sites. Most eligible non-enrollees (about 80 percent) said that they had never heard of EOPP (Brown et al. 1983, 124). Owing to administrative problems, some enrollees had to wait more than three weeks before being assigned to job search, or, after job search, being assigned to a job (Brown et al. 1983, 16). Some sites pushed subsidized employment and training more than others, with two sites enrolling more than 75 percent of those who had completed job search and two other sites enrolling less than 40 percent (Brown et al. 1983, 114).

The available non-experimental analyses of EOPP suggest that the program had positive effects on the employment rates of unmarried women. Brown and his colleagues (1983) find that EOPP raised the average employment rates of unmarried women by twelve to sixteen percentage points. Grossman and her colleagues (1985, 71) find that subsidized employment and training doubled the employment rates of female household heads during the subsidy period, and increased the employment rate of this group from 36 to 42 percent seven quarters after initial enrollment, when almost all EOPP enrollees had completed their subsidized employment.

## *SIDEBAR 7.6 Community Service Jobs in the New Hope Program*

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One component of the New Hope Program (see chapter 5) was community service jobs. For those eligible for the New Hope Program (low-income residents in two Milwaukee neighborhoods) who were unable to find a regular job after a search, the program provided temporary community service jobs at local nonprofit organizations. The jobs could last up to six months, paid minimum wage, and required that job holders work at least thirty hours per week, a requirement that made them eligible for the supplemental benefits of New Hope (earnings supplements, subsidized health insurance, and child care).

To create the one hundred jobs needed during the course of the program, New Hope used one job developer. The program had one coordinator for every twenty-five or thirty people working in CSJs to resolve any problems between the job holder and the employer. To make the program more attractive to employers, the local training agency, under contract with New Hope, handled the payroll for CSJ employees and paid workers' compensation for these employees, while the nonprofit agency sponsoring the CSJ job had full control over hiring, supervising, and firing the CSJ job holder.

Although some evaluation results are available for New Hope, it is impossible to separate out the effects of CSJs from the many other components of New Hope. However, about one of five CSJ participants was hired by the host agency after completing subsidized employment, suggesting that the CSJs may have had postprogram effects on employment.

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*Source:* Pogliinco et al. (1998); Kaplan and Rothe (1999).

TABLE 7.1 *In-Program Impact of Publicly Funded Jobs Programs*

Program	Comments	Ratio of Employment/Earnings Effects to Program-Induced Increase in Employment/Earnings for In-Program Period
Supported Work	Experimental estimates. Program: employment experience with graduated stress.	0.92 for AFDC single mothers 0.68 for ex-addicts 0.69 for ex-offenders 0.85 for youth <sup>a</sup>
New Hope	Experimental estimates. Program: earnings and benefits supplements, community service jobs.	0.68 <sup>b</sup>
YIEPP	Non-experimental estimates, including displacement. Program: guaranteed jobs for youth, part-time during school, full-time in summer.	0.67 for school-year jobs 0.44 for summer jobs <sup>c</sup>
Summer Youth Jobs	Non-experimental estimates, including displacement.	0.67 for nonwhite youth <sup>d</sup>
Youth Corps	Experimental estimates. Program: provided jobs in conservation or human services both summer and year-round for disadvantaged youths, ages sixteen to twenty-five.	0.57 <sup>e</sup>

<sup>a</sup> These are ratios of experimental-control differences in earnings, seven to nine months after enrollment, divided by program earnings during the same time period, as reported in MDRC (1980).

<sup>b</sup> This is derived as the two-year effect on average earnings per participant of \$646, versus two-year provisions of \$945 in earnings from a community service job per participant (Bos et al. 1999).

<sup>c</sup> Derived from Farkas et al. (1982, 125, table 5.8), from estimated program effect in site city versus comparison city, divided by program employment in site.

<sup>d</sup> Ellwood and Crane (1984).

<sup>e</sup> Derived from Abt report (Jastrzab et al. 1996), which estimates that provided earnings of \$6.76 per service hour are offset by \$2.92 in reduced other earnings that treatment group would have received without program.

TABLE 7.2 *Estimates of Postprogram Earnings Effects of Programs for the Disadvantaged with Significant Public-Service Employment/Work Experience Component*

Program	Comments	Annual Postprogram Earnings Effects (1998 Dollars)	Postprogram Earnings Effects as Proportion of Program Earnings
Supported Work Program	Experiment. Jobs with graduated stress. Four target groups: AFDC (mostly women), addicts (mostly men), offenders (mostly men), and youth.	AFDC: \$2,771 <sup>a</sup> Ex-addicts: 934 (n.s.) <sup>b</sup> Ex-offenders: 106 (n.s.) Youth: 531 (n.s.)	AFDC: 0.22 <sup>c</sup> Ex-addicts: 0.10 Ex-offenders: 0.01 Youth: 0.07
CETA PSE jobs	Non-experimental results.	Adult women: 2,120 <sup>d</sup> Adult men: -295 Young women: 1,200 Young men: -763	Adult women: 0.09 <sup>e</sup> Adult men: -0.01 Young women: 0.05 Young men: -0.03
CETA work experience jobs	Non-experimental results.	Adult women: 568 <sup>f</sup> Adult men: -814 Young women: -401 Young men: -2,569	—
AFDC Homemaker Home Health Aide Program	Experimental estimates. Subsidized work as welfare recipients for home health aide.	923 <sup>g</sup>	0.25 <sup>h</sup>
Community work experience programs	Experimental estimates. Work experience programs within welfare system. AFDC (mostly women) and AFDC-U (mostly men).	San Diego: <sup>i</sup> • AFDC applicants: 588 • AFDC-U applicants: (-220) (ns) Cook County: 51 <sup>i</sup> (n.s.) West Virginia: <sup>i</sup> (n.s.) • AFDC recipients: 1 (n.s.) • AFDC-U recipients: (-245) (n.s.)	—
WIN program: subsidized employment and training component	Non-experimental estimates. Subsidized employment or training for welfare recipients.	3,128 <sup>g</sup>	—
Employment Opportunity Pilot Project: subsidized employment and training component	Non-experimental estimates. Subsidized employment and training for low-income persons.	878 <sup>g</sup>	0.16 <sup>i</sup>

(Table continues on p. 184.)

TABLE 7.2 *Continued*

Program	Comments	Annual Postprogram Earnings Effects (1998 Dollars)	Postprogram Earnings Effects as Proportion of Program Earnings
Youth Incentive Entitlement Pilot Project	Non-experimental estimates. Guaranteed jobs for disadvantaged youth.	1,306 <sup>a</sup>	—
Maine TOPS program	Experimental estimates. Training and work experience and subsidized employment for welfare recipients.	1,243 <sup>a</sup>	—
STETS program	Experimental estimates. Work experience plus subsidized employment for mentally retarded.	1,021 <sup>a</sup>	—

*Note:* Programs considered are those in which a significant share of participants go through public employment or work experience. Where possible, I estimate the separate contribution of public employment or work experience, although this is seldom possible. In general, I estimate annual earnings effects based on earnings effects for some time period close to one year after the end of public employment or work experience. All estimates are expressed in 1998 dollars and are earnings effects per year. I also, where possible, state the earnings effects as a proportion of the earnings shock provided during the program period by the public employment or work experience. In some cases, the earnings effects can be stated only as a proportion of actual earnings paid by the program; in general that proportion is greater than the earnings shock provided by the program because participants would have had some earnings even without the program.

<sup>a</sup> Real earnings effects taken from various chapters of Hollister, Kemper, and Maynard (1984). Postprogram effects taken from months 25 to 27 after random assignment.

<sup>b</sup> n.s.: not statistically significant at 5 percent level.

<sup>c</sup> Earnings effects for months 25 to 27 of random assignment are taken relative to earnings effects of one to three months after random assignment.

<sup>d</sup> Estimates given are medians for studies summarized in Barnow (1987). For each study, I estimate effects for women and men overall by combining results for whites and minorities by assumption, based on Cook et al. (1985, 34), that 66 percent of CETA PSE participants during this period were white and 34 percent minority. For Bassi's (1993) study, I combined effects for economically disadvantaged not on welfare and those on welfare using the proportions in PSE from each group during this period from Cook et al. (1985, 34). For adult women and men, six studies are summarized. Effects for women in 1998 dollars vary from \$1,160 to \$3,348, effects for men from -\$2,090 to \$1,233. For young women and young men, only two studies are summarized, in which effects for young women in 1998 dollars are \$130 and \$2,530; for young men these two studies give estimates of -\$518 and -\$1,007.

<sup>e</sup> Proportions given are based on statement by Bassi (1983) that average costs of CETA PSE per participant were \$23,579 (after conversion to 1998 dollars). The net earnings shock of CETA is less than this because participants would have had some earnings even without the program. Hence, figures for postprogram effects as a proportion of earnings given in the table for CETA PSE are understated relative to what effects would be as a proportion of the in-program earnings increase provided by CETA. If this displacement effect was 0.3, then the AFDC number for the proportions would be 0.13 rather than 0.09.

<sup>f</sup> Estimates are derived from Barnow (1987) and are medians of a number of studies. The methodology to derive these results is similar to that used for CETA PSE.

TABLE 7.2 *Continued*

<sup>e</sup> Estimated effects are two years after the program, for 1987, and are derived from Bell et al. (1995, 62). Effects cited are per participant.

<sup>f</sup> Proportion is derived as the earnings effect for 1987 (second year after program), divided by the earnings effect per participant for 1985, the main in-program year (Bell et al. 1995).

<sup>g</sup> Estimated effects are derived from Brock et al. (1993). AFDC-U results in West Virginia are actually nonexperimental effects based on a comparison area. Estimated effects are annual averages of effects from three months to eighteen months after random assignment. As noted in chapter 7, particularly in West Virginia, where no limit was placed on tenure in work experience assignments, this period was clearly the period during which some work in work experience jobs, which are not counted as “earnings,” was taking place.

<sup>j</sup> These estimates come from Grossman et al. (1985) and are their figures for quarter 7 after enrollment in WIN. The Grossman estimates represent the effects of participating in the subsidized employment and training component of WIN, not WIN in general. They attempt to control for selection bias in these non-experimental estimates. It is unclear to what extent the estimates given here are effects for subsidized employment and to what extent they are effects for subsidized training.

<sup>k</sup> These estimates are based on Grossman et al. (1985) and are their figures for quarter 7 after enrollment in EOPP. The Grossman model shows estimated effects for participating in EOPP subsidized employment and training; they attempt to model for selection into SET and correct for selection bias. Because the most common EOPP assignment after job search was to subsidized employment, not training, we can be fairly certain that these estimates represent effects of subsidized employment.

<sup>l</sup> The proportion is the earnings effects for quarter 7 in Grossman et al. (1985), divided by the earnings effects for quarter 2 after random assignment.

<sup>m</sup> These estimates are based on the fall 1981 follow-up. I use numbers reported by Farkas et al. (1984) per YIEPP participant (xxvi); these are blown up from effects per program eligible by a factor of one over 0.73, the proportion of those eligible who participated at some point during full-scale program operations. Fall 1981 is just after the program ceased operation during the summer of 1981, but as Farkas et al. (1984) point out, the program was being phased out during 1980 to 1981 and only 13 percent of the individuals in the group followed for this estimate had a YIEPP job during this period; 43 percent had YIEPP job in 1978 to 1979, and 39 percent had YIEPP job in 1979 to 1980.

<sup>n</sup> This estimate is derived from the quarter 11 earnings effects of the program per experimental (Auspos et al. 1988, xxi).

<sup>o</sup> These estimates are derived from estimated effects on earnings twenty-two months after random assignment (Bangser 1985, 23).

TABLE 7.3 *Long-Run Effects of Publicly Funded Jobs*

Program	Comments	Effects of Program Four or Five Years After Program Completion for Groups That Experienced Significant Postprogram Earnings Effects (1998 Dollars)	Long-Run Effects as Proportion of Immediate Postprogram Effects
Supported Work	Experiment. Subsidized jobs featuring graduated stress.	AFDC target group (only group with significant postprogram earnings effects): \$1,140 <sup>a</sup>	1.84 <sup>b</sup>
AFDC Homemaker Home Health Aide Program	Experimental estimates. Subsidized work as home health aide.	585 <sup>c</sup>	0.63 <sup>d</sup>
WIN program: subsidized employment and training component	Non-experimental estimates. Subsidized employment and training for welfare recipients.	2,910 <sup>e</sup>	0.93 <sup>f</sup>

*Sources:* <sup>a</sup> Long-run effects are taken from 1983 effects in Couch (1992), the fifth postprogram year. Because Couch's estimates are taken from social security records, they probably miss considerable amounts of earnings, as noted by Grossman et al. (1985, 74). Many Supported Work participants ended up getting public-sector jobs that might during this time period have been uncovered by social security. Couch also estimates effects through 1986. Compared to 1983 annual effects of \$1,140, subsequent years' effects are: 1984—\$1,313; 1985—\$1,033; 1986—\$938 (all in 1998 dollars). There is very little sign of a falloff in the real dollar effects of Supported Work over time.

<sup>b</sup> This is the ratio of the earnings effects that Couch (1992) estimates for AFDC recipients in 1983 to the effects estimated by Couch for 1979. The estimates from the original Supported Work study are much higher because of uncovered earnings of Supported Work participants.

<sup>c</sup> Estimated effects are five years after the program, for 1990, and are derived from Bell et al. (1995, 62). Effects cited are per participant.

<sup>d</sup> The proportion is derived as the earnings effect for 1990 (fifth year after program), divided by the earnings effect per participant for 1987, the second postprogram year, and are derived from Bell et al. (1995).

<sup>e</sup> These estimates come from Grossman et al. (1985) and are their figures for quarter 15 after enrollment in WIN. The Grossman estimates represent effects for participating in the subsidized employment and training component of WIN, not WIN in general. They attempt to control for selection bias in these non-experimental estimates. It is unclear to what extent the estimates given here are effects for subsidized employment and to what extent they are effects for subsidized training.

<sup>f</sup> These are the estimated effects from Grossman et al. (1985) for quarter 15 after enrollment in WIN, divided by the estimated effects for quarter 7 after enrollment in WIN.

*SIDEBAR 8.1 Minnesota's MEED Program: A "Liberal" Approach to Wage Subsidies for the Disadvantaged*

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Minnesota's MEED<sup>®</sup> wage subsidy program, active from 1983 to 1989, was initiated and backed by liberal state legislators and a coalition of labor, social service, and black and Hispanic groups. Among wage subsidy programs, MEED was most distinguished by deep wage subsidies, requirements for job retention, an emphasis on economic development and job creation, and a focus on small business.

The program provided subsidies of up to \$4 per hour for wages (equivalent to \$6.55 per hour in 1998 dollars), and \$1 per hour for fringe benefits (\$1.64 per hour in 1998 dollars) for placements of the unemployed at public agencies and private businesses. At MEED's start, 60 percent of placements were at public agencies, but over time MEED's emphasis shifted to private placements, with over 75 percent of all job placements being made in the private sector. The jobs provided were required to be new jobs. Actual wages were about \$1 per hour more than the MEED subsidy payment. The wage subsidies were payable for up to six months for up to forty hours per week. Private-sector employers were expected to retain subsidized workers for at least one year after the six-month subsidy was ended. If the worker quit or was fired by the employer before this period ended, the employer was supposed to either accept another MEED worker or repay most of the subsidy.

Participating workers and employers were selected and matched by local job training agencies. Eligible workers were broadly defined to include any unemployed worker who was not receiving unemployment benefits. Local training agencies were supposed to place a priority on selecting workers who were on public assistance, and in practice about half of MEED workers were public assistance recipients. Training agencies were also supposed to place a priority on nonretail small businesses, manufacturing firms, and firms that sold their product outside of Minnesota ("export-base" firms). More than 80 percent of participating private businesses had twenty or fewer full-time employees, over 20 percent were engaged in manufacturing, and 15 percent made more than half of their sales outside of Minnesota.

The Minnesota Jobs Now Coalition, a key supporter of MEED, conducted several surveys of employers who were using it. Based on these surveys and other evidence, the following observations seem warranted:

- The program's subsidies led to job creation at subsidized firms. Surveys indicate that more than 55 percent of participating employers claimed that they would not have expanded without the MEED program. Employers do not have strong incentives to lie, since the program did not require that the jobs be ones that would not have been created but for the subsidy. More than 60 percent of these businesses claimed that they had lacked sufficient cash flow or capital to expand without the MEED assistance.



## SIDEBAR 8.1 *Continued*

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- The program can be run on a large scale. At its height, during its second year, MEED placed more than 7,000 workers at private firms over a ten-month period. This is equivalent to running a national program at a level of 552,000 subsidized hires in 1998. In surveys of participating employers, more than 90 percent reported that the program had a minimum of red tape and its rules were easy to understand. More than 80 percent of participating employers were satisfied with the performance of their MEED workers.
- The long-run national political viability of approaches like the MEED program is uncertain. It was possible to run the program on a large scale in a pro-government state such as Minnesota in the aftermath of the early 1980s recession. However, even in the Minnesota political context, the program was phased out as the economy improved.
- The long-run effects of the program are uncertain. I have been unable to find any data on the long-run job retention of MEED workers. Apparently 78 percent of the MEED workers completed their six-month subsidy period plus sixty days.

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*Source:* This summary is based on Rode (1988) and Rangan (1984).

<sup>a</sup> MEED originally was an acronym for Minnesota Emergency Employment Development, a name that was later changed to Minnesota Employment and Economic Development.

*SIDEBAR 8.2 Oregon's JOBS Plus Program: A "Conservative" Version of Wage Subsidies for the Disadvantaged*

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Oregon's JOBS Plus Program was initiated through political pressure from the Jeld-Wen Corporation, an important Oregon wood products manufacturer. The program reflects the belief that government income support for the poor and the unemployed can in most cases be replaced by immediate employment and on-the-job training in the private sector, although such employment may require subsidies. The designers of the program, now working for Jeld-Wen's nonprofit affiliate, the American Institute for Full Employment, include several individuals associated with welfare policy in the Reagan administration and some former affiliates of the conservative Heritage Foundation.

Among wage subsidy programs, the JOBS Plus Program is most distinguished by its extremely deep subsidies and relatively few formal requirements of participating employers. In JOBS Plus, welfare recipients and unemployment insurance recipients are placed with participating employers, which receive a subsidy of the Oregon minimum wage (\$6 an hour) plus payroll taxes and workers' compensation reimbursement, for up to six months. Employers are required to pay the "trainees" the prevailing wage for similar jobs in the company (on average, this appears to be a bit less than \$1 over the Oregon minimum wage); not to use the trainees to replace existing workers or fill vacancies; and to provide the trainees with a workplace mentor and appropriate OJT. After four months of wage subsidy, if the employer has not hired the trainee for a regular job, the trainee is allowed to use one paid day a week to engage in job search. Participating employers are required only to sign relatively simple forms outlining their agreement to these terms; in particular, they are not subject to any requirement to hire trainees permanently, although this happens in about one-quarter of all placements.

Based on Roberts and Padden's (1998a, 1998b) process observations of JOBS Plus, the following observations are warranted:

- Despite the deep subsidies provided under the program and the great effort to make the program employer-friendly, the program has operated on a modest scale, owing to both difficulties in recruiting employers and difficulties in finding suitable welfare recipients and other unemployed for the program. Only 7 percent of the Oregonians engaged in welfare-to-work programs are in JOBS Plus. From July 1996 to October 1997, about 3,500 participants left JOBS Plus.
- The program does appear to be more attractive to small businesses than are wage subsidies such as TJTC/WOTC and the JTPA OJT program, possibly because of the simple and limited program requirements.

*SIDEBAR 8.2      Continued*

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- In interviews, some employers questioned aspects of the program. Specifically, some felt that the subsidies were unnecessarily large. In addition, some employers apparently felt that pre-job preparation training would sometimes have been a more effective way of developing human capital.
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*Source:* This description is largely based on Roberts and Padden (1998a, 1998b), as well as on the American Institute for Full Employment's web site, *www.fullemployment.org*.

TABLE 8.1 *Brief Description and History of Large-Scale National Wage Subsidy Programs*

Program	Years	Brief Description	Number of Annual Participants at Peak (Millions per Year)	Annual Funding at Peak (Billions of 1998 Dollars)	Participation of Businesses
OJT component of MDTA (Manpower Development and Training Act)	1962 to 1971	Training contracts with employers to hire the unemployed or upgrade the underemployed.	0.115 (1967)	\$0.2	
JOBS (Job Opportunities in the Business Sector)	1968 to 1973	Training contracts with employers for hiring young, less-educated black males from poverty backgrounds.	0.093 (1971)	0.7 (1969)	Subsidy claimed for one-third of eligible hires
WIN tax credit program	1972 to 1981	Tax credit for hiring welfare recipients participating in WIN welfare-to-work program; initially 20 percent credit, with maximum credit of \$3,900 (1998 dollars) per hire.	0.028 (1976)	0.033	Credit claimed for 15 percent of eligible hires
OJT component of CETA (Comprehensive Employment and Training Act)	1974 to 1982	Training contracts with employers for hiring the disadvantaged.	0.230 (1978)	0.7	
NJTC (New Jobs Tax Credit)	1977 to 1978	Tax credit of 50 percent for the first \$10,500 (1998 dollars) of wages per employee for increases in employment of more than 2 percent over previous year. No targeting.	2.15 (1978)	9.5	1.1 million firms claimed credit in 1978, over half of eligible firms

TJTC (Targeted Jobs Tax Credit)	1978 to 1994	Tax credit of various sizes over the years offered for hiring various target groups. By 1994, credit had been scaled back to 40 percent of first-year wages, up to wage of \$6,600 annually (1998 dollars). Although wide variety of disadvantaged groups were targeted, disadvantaged youth were most frequently claimed.	0.622 (1985)	0.8	Of eligible youth hired, firms claimed TJTC for only 5 to 9 percent (O'Neill 1982; Katz 1998)
OJT component of JTPA (Job Training Partnership Act)	1983 to 1999	Training contracts with businesses to hire and train the disadvantaged, in exchange for 50 percent wage subsidy for six months. Continued under WIA.	0.253 (1985 to 1986)	\$0.2 billion (wage subsidies); \$0.7 billion including services	
WOTC (Work Opportunity Tax Credit)	1997 to present	Tax credit for hiring target groups, most recently credit of 40 percent of wages up to a \$2,400 credit for individuals who retained at least 400 work hours. Eligibility simplified by tying it to receipt of food stamps or welfare rather than family income. Compared to TJTC, much more emphasis on welfare recipients: 55 percent welfare recipients in 1999 (Beverly 1999).	0.335 (1999)	0.2	
WWTC (Welfare-to-Work Tax Credit)	1998 to present	Similar to WOTC, but more generous, long-term, and targeted. Eligible group is long-term welfare recipients. Credit: 35 percent of first-year wages, 50 percent of second-year's wages, with maximum credit of \$3,500 (first year) and \$5,000 (second year).	0.105 (1999)	0.035	

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Source: Author's compilation.

TABLE 8.2 *Small-Scale, Local, and Experimental Wage Subsidy Programs for the Disadvantaged*

Program	Years and Places of Operation	Description	Magnitude of Subsidies	Scale of Program
Work supplementation/ grant diversion and other wage subsidies	1967 to present; various states (thirty-two at present)	Welfare department enters into contract to pay welfare grant to firm hiring welfare recipient.	Median: \$2.50 per hour, typ- ically for six to nine months.	Very small. Usually only a few thousand nationally.
State tax credits for hiring wel- fare recipients or other disad- vantaged	Years unknown; currently fif- teen states have such tax incentives	State provides credit against cor- porate income tax for hiring disadvantaged or welfare recipient.	Typically 20 to 30 percent of wages, with cap of less than \$2,000 in credit per year per hire, but often lasts for two to three years.	No data, but probably low take- up rate.
Supported Work	Experimental program 1975 to 1978	Experiment with providing job with graduated stress. Usually public jobs, but sometimes worked in private businesses, with private businesses paying part of costs.	Probably over half of wages.	Experiment was run on a small scale.
YIEPP	Experimental program con- ducted from 1978 to 1981 at seventeen sites	Experiment guaranteeing jobs for disadvantaged youth; 23 per- cent of jobs in private sector in 1981.	100 percent wage subsidy.	Operated at saturation level in some sites. National version would have 1.1 million partici- pants and cost \$3.6 billion an- nually (see table 7.5)

MEED	Minnesota, 1983 to 1989	Provided wage subsidy for up to six months for employers hiring unemployed workers not receiving UI benefits for newly created jobs. Over half of placements in private sector.	Provided \$6.55 per hour in wage subsidy and \$1.64 per hour in subsidy for benefits (1998 dollars). Average wage subsidy exceeded 80 percent.	At peak, program spent \$50 million per year and subsidized about 9,000 new job slots and had 10,700 participants. National version: \$5.1 billion per year, 0.591 million new job slots, and 0.703 million participants.
Maine TOPS	Maine, 1983 to 1986	Welfare-to-work experiment, with component of OJT placements, mostly in private sector.	Wage subsidy of 50 percent	Experiment.
New Jersey Grant Diversion Program	New Jersey, 1984 to 1987	Experiment in which welfare grants used to fund OJT contracts.	50 percent wage subsidy, for up to six months.	Very small-scale program, run as experiment.
Oregon JOBS Plus Program	Oregon, 1993 to present	First a demonstration, and now a statewide program, in which welfare recipients and UI recipients are placed with participating employers who receive a wage subsidy.	Effective wage subsidy of 80 percent for up to six months.	Program is operated at modest scale: less than 4,000 per year. Similar national program would have 0.2 million participants

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Source: Author's compilation.

TABLE 8.3 *Simulations of the Effects of Marginal Employment Tax Credits*

Study	Description of Subsidy Simulation	Annual Cost (Billions of 1998 Dollars)	Jobs Created (Millions)	Cost per Job (1998 Dollars)	Windfall Percentage
Kesselman, Wil- liamson, and Berndt (1977) <sup>a</sup>	Marginal employment tax credit of 4.8 percent in manufacturing, applying to employment above 90 percent of base.	\$4.5	0.101	\$44,433	94.8
Hamermesh (1978)	Ten percent wage subsidy to all employment increases above base-year employment; <sup>b</sup> to all employment increases above 85 percent of base-year employment.	Base of 100 per- cent: \$2.0	Base of 100 per- cent: 0.369	Base of 100 per- cent: 5,467	Base of 100 per- cent: 71.1
		Base of 85 per- cent: \$21.1	Base of 85 per- cent: 0.866	Base of 85 per- cent: 24,365	Base of 85 per- cent: 89.3
Bartik (1999)	Three different 10 percent wage subsidies, for three groups: single mothers, other women with less than a college education, and men with less than a college education. Wage subsidy assumed to apply to all employment above 80 percent of baseline. <sup>c</sup>	Single mothers: \$1.5	Single mothers: 0.066	Single mothers: 22,619	Single mothers: 92.5
		Other noncollege women: \$17.1	Other noncollege women: 1.630	Other noncollege women: 10,496	Other noncollege women: 83.2
		Noncollege men: \$28.4	Noncollege men: 2.536	Noncollege men: 11,189	Noncollege men: 80.6

Source: Author's compilation.

<sup>a</sup> Kesselman et al.'s (1977) estimates are derived from estimating a translog cost function for manufacturing with three factors of production: blue-collar labor, white-collar labor, and capital. Their estimates of the own price elasticity of demand for blue-collar labor is  $-0.338$ , and  $-0.192$  for white-collar labor. Thus, their estimates agree with most of the research literature that elasticities of demand for specific types of labor are modest.

<sup>b</sup> Hamermesh (1978) considers a variety of scenarios and labor demand elasticities. I consider the case where the labor demand elasticity is 0.3, which is closest to estimates later presented by Hamermesh (1993) in his book on labor demand.

<sup>c</sup> These estimates use the model of appendix 2 (see also Bartik 1999). In addition to these employment effects, the 10 percent wage subsidies would each cause the following effects on the market wages of the group targeted: single mothers, 0.4 percent; other noncollege women, 4.3 percent; other noncollege men, 6.1 percent. These are effects five years after the subsidy is introduced. All these estimates come from simulations reported in table A2.12, coupled with figures for total employment of three groups, except for deriving cost estimates. For cost estimates, I assume that a 10 percent wage subsidy applied to all employment above 80 percent of the base year has the same effect on employment as a 10 percent wage subsidy applied to all employment and costs the same as a wage subsidy applying to all new hires. The implicit assumptions are that a subsidy on all employment above 80 percent of base employment makes a subsidy relevant on the margin to almost all employment decisions; also, new hires each year are about 20 percent of total employment.



TABLE 8.4 *Estimates of Effects of New Jobs Tax Credit*

Study	Methodology	Results
Perloff and Wachter (1979)	Regression analysis with some controls that compares 1976 to 1977 job growth for firms that knew about NJTC to job growth of firms that did not know; also survey of usage.	Firms that knew about credit increased employment by 3 percent more than similar firms that did not know. This implies an economywide increase in employment in 1977 of 700,000 jobs (Eisner 1989, 70). 27 percent of firms with less than 10 employees knew about NJTC, versus 89 percent of those with over 500 employees.
McKevitt (1978), as reported in Bishop (1981)	Survey of members of National Federation for Independent Businesses, asking about knowledge of NJTC and employment increases.	Of surveyed small firms, 4.1 percent said they increased employment as a result of NJTC, by an average of 2.3 employees, suggesting an aggregate employment increase of 300,000 jobs just among NFIB members.
Bishop (1981)	Time-series regression analysis of monthly data on industry employment in construction, retail industries, wholesale trade, and trucking, and its relationship to firms' awareness of NJTC.	Estimated increase in employment in these industries due to NJTC is between 225,000 and 585,000 in various models.

Source: Author's compilation.

TABLE 8.5 *Empirical Studies of Entitlement Wage Subsidy Programs*

Study	Methodology	Results
Impact (1977), as reported in Hamermesh (1978)	Survey of employers using the WIN tax credit.	Less than 10 percent of employers using credit attributed their hiring of the WIN enrollee to the credit.
Wisconsin DHSS and Institute for Research on Poverty (1982)	Experimental study of effects of promotional efforts on firms' usage of WIN tax credit and TJTC.	Mail-only promotion slightly increased firms' usage of tax credits, but not statistically significant. Mail-plus-phone promotion approximately doubled firms' usage of tax credits, from 4.5 percent of firms to 9.0 percent. Twenty-three percent of firms with more than one hundred employees used credit, versus less than 4 percent for firms with fifty or fewer employees.
O'Neill (1982)	January 1980 survey of 720 firms.	Sixty-three percent of firms had heard of TJTC, 38 percent had used or planned to use TJTC, of whom 26 percent said use of TJTC would increase employment level, and 46 percent said they substituted target hires for similar nontarget hires. Both knowledge and usage or planned usages of TJTC were lower for small firms.
Christensen (1984)	Random survey of firms in 1980 and 1982.	Thirty-four percent of employers who used credit claimed it affected their hiring decisions significantly, 22 percent claimed it had some effect. TJTC usage was much greater among large establishments than small establishments. Probability of TJTC usage was 32 percent higher among firms contacted and asked to accept a specific referral. Firms that had received TJTC in 1980 or 1981 increased share of youth in total employment by 6 to 20 percent more than other firms. But employment growth was not statistically significantly greater in firms that used TJTC compared to firms that did not. Finally, for every increase of one in state vouchering activity in 1982, there was a 0.4 increase in employment for eligible youth as of March 1983.
Bishop, in Bishop and Hollenbeck (1985, ch. 4)	Regression analysis of employment of around 3,000 firms, December 1980 to June 1982.	Based on typical number of certifications associated with TJTC participation, for every ten TJTC-subsidized jobs at firm, there were three to four extra jobs, and six or seven displaced, with probably most of those displaced being nonsubsidized youth (four or five).

Bishop, in Bishop and Hollenbeck (1985, ch. 5)	1982 survey of stratified random sample of firms.	Eighty percent of TJTC certificates are at firms that claim they tried to select TJTC eligibles. But only 18 percent of firms reported that job applicant's eligibility influenced hiring decisions "a great amount," and only 15 percent reported that it influenced their decision "a moderate amount."
Bishop, in Bishop and Hollenbeck (1985, ch. 7)	1983 interviews in which 850 employers were asked to rate eleven imaginary job applications, with TJTC status of application randomly assigned.	On average, TJTC status only slightly increased employer's rating, by 1.9 points on average.
Copeland (1985); Hollenbeck, in Bishop and Hollenbeck (1985, ch. 8)	Case studies of thirty-five firms heavily using TJTC.	Twenty-six percent of firms reported giving hiring preference to TJTC applicants. Nine percent reported creating openings to hire TJTC applicants. Seventy-five percent of firms used consultants to screen applicants for eligibility and arrange for certification. Only 11 percent felt TJTC workers were less productive than other workers. Fifty-five percent of firms had monetary incentives to encourage local managers to hire and certify TJTC workers.
Bishop and Montgomery (1986)	Analysis of 1980 survey of 5,279 establishments.	Proportion using a program went up if firm learned of WIN tax credit from government rep: this increased WIN participation rate by 70 percent and TJTC participation rate by 53 percent.
Lorenz (1988)	Comparison of earnings of three groups: those vouchered and then hired under TJTC; those submitted for TJTC certification upon hire; and those vouchered but not hired under TJTC during first six months of 1982, in Maryland and Missouri.	Higher earnings during TJTC period for groups hired under TJTC, but earnings differentials eroded over time.

*(Table continues on p. 232.)*

TABLE 8.5 *Continued*

Study	Methodology	Results
Hollenbeck and Willke (1991); Hollenbeck, Willke and Ershadi (1986)	Comparison of change in average annual earnings and employment of vouchered TJTC eligibles in states with average voucher penetration rate of eligible population to performance of nonvouchered eligibles in states with zero penetration rate.	Vouchered youth and handicapped generally gained, with average annual real earnings gain of \$462 (1998 dollars) for youth and \$1,940 for handicapped. Most of earnings gain appeared to be due to employment increases. Among youth, white females gained most. But there was lots of displacement: losers included nonvouchered black male youth, both vouchered and nonvouchered black males, black females, and white females on welfare, and both vouchered and nonvouchered black veterans.
Bishop and Kang (1991)	Survey data on 3,412 establishments collected in 1982, with oversampling of large firms in low-wage industries.	Government offer to refer eligible workers increased expected number of TJTC hires five to twelve times, depending on year. Government contact without referral offer increased number of TJTC hires by 43 percent to 87 percent, depending on year. Prior involvement with other government wage subsidies significantly increased TJTC usage.
U.S. General Accounting Office (1991)	Interviewed sixty employers among top TJTC users in four states.	Forty-five percent of employers made special efforts to recruit, hire, or retain TJTC-eligible workers, including developing relationships with agencies supplying TJTC eligibles, giving incentives to local managers for hiring and retaining TJTC eligibles, and being more lenient in job standards with TJTC hires.
Bishop and Montgomery (1993)	Regression analysis of the percentage change in individual firms' employment from end of 1980 to 1981 as a function of change in TJTC hires as a proportion of previous level of employment.	TJTC estimated to increase total firm employment by 13 to 30 percent of certifications.
U.S. Department of Labor (1994)	Selected sample of 983 TJTC workers. Asked employers whether TJTC worker would have been hired without tax credit.	Only 8 percent of TJTC certificates were hired by their employers because of their TJTC status.

Tannery (1998)	Compared treatment group of youth and welfare recipients in Pennsylvania from 1988 to 1994 who were hired and successfully certified with controls whose certification was not processed owing to technical errors.	In general, certification was estimated to increase earnings of disadvantaged youth from \$2,000 to \$4,000 per year and to increase earnings of welfare recipients by about \$1,600 per year.
Katz (1998)	Differences-in-differences-in-differences estimator based on restriction in 1989 that made disadvantaged twenty-three- to twenty-four-year-olds no longer eligible for TJTC.	TJTC estimated to increase employment rates of disadvantaged twenty-three- to twenty-four-year-olds by 3.4 to 4.3 rate points. Suggests that 40 to 52 percent of TJTC certifications created jobs for group targeted. If this were applied to all TJTC, TJTC would have created 249,000 to 322,000 jobs for disadvantaged groups that it targeted at its peak in 1985.
Woodbury and Spiegelman (1987); Spiegelman and Woodbury (1987); Dubin and Rivers (1993)	Experimental data, seeing how weeks of unemployment vary for UI recipients for whom bonus was paid to employers hiring them within eleven-week period.	Bonus reduced initial spell of unemployment by statistically significant two-thirds of a week on average. But take-up rate was also very small: only 5 percent of sample submitted “notice of hire” to state of Illinois—less than one-fourth of 21 percent of experimental group that was eligible for bonus

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*Source:* Author's compilation.

TABLE 8.6 *Estimates of Effects of Discretionary Wage Subsidy Programs*

Study	Methodology	Results
Various studies of MDTA OJT, summarized by Perry et al. (1975)	Four different studies comparing earnings gains of MDTA trainees who participated in OJT to some comparison group.	Average earnings effects in first post-OJT year across all studies and groups was \$1,834 (1998 dollars). Effects were generally greater for women than men, blacks than whites. OJT effects were generally greater than institutional training.
Farber (1971), as reported in Perry et al. (1975)	Comparison of 1969 earnings of 1968 JOBS contract participants to matched comparison group.	Overall average effect on earnings was an increase in annual earnings of \$1,188 (1998 dollars). Effects on females were five times greater than for males, effects on blacks two or three times greater than for whites.
Ball et al. (1981)	Variation in YIEPP wage subsidy offer from 50 to 100 percent	100 percent wage subsidy: 18.2 percent agreed to participate and hire disadvantaged youth. 75 percent wage subsidy: 10.0 percent agreed to participate. 50 percent wage subsidy: 4.7 percent agreed to participate.
Ball et al. (1981)	Participating firms asked to allocate work hours of youths among those who displaced persons who would otherwise be employed and those whose hiring allowed firm to expand employment	Median reported displacement rate was 50 percent. Displacement was significantly higher at worksites whose quality was rated higher by outside observers.
Unicon (1982)	Case studies by outside observers of twenty-nine firms.	Sixty percent displacement.
Gould et al. (1982)	Regression analysis of effects on firm's employment growth of YIEPP hires.	For every ten YIEPP workers hired by for-profit firms, net employment of firms went up by about eight jobs, but about three of these were extra supervisors. Net employment of nonsupervisory personnel went up only by around five.

Farkas et al. (1984)	Non-experimental data comparing youth at sites participating in YIEPP to youth at previously similar sites not participating in YIEPP.	Annual earnings effect a few months after YIEPP program terminated (although it had been mostly phased out a year earlier): \$1,306 (1998 dollars) for young black cohort. This effect is overall YIEPP effect; subsidized private for-profit jobs were only 23 percent of YIEPP jobs.
Bishop, in Bishop and Hollenbeck (1985, ch. 4)	Regression analysis of employment of around 3,000 firms, December 1980 to June 1982 as a function of dummy variable for whether firm participated in CETA-OJT.	For every ten OJT slots at a firm, there will be three extra jobs at the firm.
Barnow (1987), summary of various studies	Regression analyses of postprogram annual earnings of CETA-OJT participants, compared to matched individuals.	Adult women: \$1,572 Adult men: \$1,851 Female youth: \$1,636 Male youth: -\$172
Bishop and Montgomery (1986)	Analysis of 1980 survey of 5,279 establishments.	Among establishments familiar with OJT, the proportion using OJT was increased by 85 percent if firm first learned of WIN tax credit from government representative.
Rangan (1984), Rode (1988)	Survey data on participating MEED employers.	In 1988, 56 percent claimed they would not have expanded to current size without MEED subsidy; in 1984, 59 percent claimed they would not have expanded to current size without MEED subsidy.
Auspos et al. (1988)	Experiment in which welfare recipients randomly assigned to treatment sequence of prevocational training, then work experience, then OJT wage subsidy.	Net annual earnings impact of entire program sequence per experimental was \$1,243 (in 1998 dollars). Program statistics indicate that about seven out of ten OJT placements roll over into regular positions with same employer.

*(Table continues on p. 238.)*

TABLE 8.6 *Continued*

Study	Methodology	Results
Freedman et al. (1988)	Experiment in which single-mother welfare recipients were randomly assigned to group eligible for OJT wage subsidies.	Annual earnings impact (1998 dollars) during immediate postprogram period was \$929 per experimental. Since only 43 percent of all experimentals participated in OJT, and other differences in services received between experimentals and groups were small, the average earnings impact per OJT participant was \$2,160. During the main period in which experimentals received OJT subsidies, employment effects on experimentals were from 38 to 46 percent of the OJT-subsidized employment, suggesting that OJT led to increased employment for a little less than half of OJT sample. Fifty-six percent of OJT participants completed their trial employment period, and all but one of those who completed the trial employment period rolled over into a regular job with same employer.
Orr et al. (1996)	Random assignment experiment in which individuals were randomly assigned to treatment and control after having been divided into one of three service strategies: OJT, classroom training, or other services.	Effects (in 1998 dollars) in months 19 through 30 after enrollment in JTPA were \$1,342 for adult females, \$1,479 for adult males, -\$1,182 for female youth, and -\$1,575 for male youth. Youth effects were not statistically significant. Effects for adult females on welfare at enrollment appeared to be about twice as great as for average adult female. OJT effects were generally more positive than classroom training results for adults.

*Source:* Author's compilation.



Until recently, JobNet in Portland, Oregon, was still active and providing one of the most sophisticated programs linking economic development with jobs for disadvantaged persons. Companies receiving economic development aid from the Portland Development Commission (PDC), the leading regional economic development agency, were required to sign a "first-source" agreement with PDC under which the company had to use PDC as a first source of applicants for entry-level jobs. The first-source agreements were administered by JobNet, a division of PDC.

From 1978 to 1998, JobNet worked with local training agencies, the employment service, local community colleges, and neighborhood agencies to identify disadvantaged persons who were suitable for entry-level jobs. JobNet would ensure that disadvantaged persons were screened and/or trained for entry-level jobs in firms receiving economic development aid. Firms were required to consider these candidates but were not required to hire any particular candidate or any fixed number of candidates from JobNet. However, if a firm was not making a good-faith effort to hire JobNet referrals, incentives for that firm could be revoked.

JobNet's design combines a stick with a carrot. In addition to penalizing uncooperative firms, JobNet was rewarding cooperative firms with easier access to high-quality job applicants. JobNet saw itself as having two sets of clients: disadvantaged job seekers and firms locating or expanding in Portland. JobNet's services met the needs of both sets of clients.

JobNet's design combined labor supply with labor demand programs. To offer disadvantaged people meaningful access to higher-skill jobs, the program provided customized training. For example, JobNet worked with Portland semiconductor firms to set up specialized training institutes.

JobNet operated at a high activity level. From 1989 to 1996, it placed 5,123 job seekers, or around 700 persons a year. In 1994 to 1995, one of JobNet's peak years, 1,235 job seekers were placed. On a national scale, this would have been equivalent to 541,000 placements per year.<sup>6</sup>

JobNet's placements helped many disadvantaged persons to get jobs that paid above-average wages. Forty-two percent of JobNet's placements in the fiscal year 1995 were minorities, and 52 percent had low income. The median hourly wage of those placed was \$9.35 per hour (in 1998 dollars).

These results were achieved at a modest cost to JobNet itself of about \$353 per placement (in fiscal year 1995). However, JobNet's partners (community colleges, neighborhood groups, training agencies, the employment service) also had costs, which are unknown. On a national scale, the costs of a JobNet program would be around \$191 million annually.

A case study suggests that firms were favorably impressed with JobNet. One manufacturer had "initial doubts about working with JobNet staff and . . . concerns about the flexibility he would lose in advertising for covered positions. . . . [But] the company's human resource manager found JobNet to be very responsive and communicative throughout the firm's hiring process" (Molina 1998, 41). In addition, some

## *SIDEBAR 9.1      Continued*

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firms used JobNet's services voluntarily; indeed, 42 percent of JobNet's 1995 placements were at such firms.

However, JobNet in the end proved politically vulnerable because it lacked grassroots support. As Oregon moved to a "one-stop" consolidated approach to workforce development in 1997 and 1998, JobNet as a training program was removed from the PDC and has not yet been re-created at the new workforce development board. Although some neighborhood groups and education and training agencies protested JobNet's demise, no one was willing to make JobNet a key issue. This lack of support may reflect JobNet's weak ties to most neighborhood groups. Early in JobNet's history, it was thought that neighborhood groups would be a key source of job candidates. However, most neighborhood groups did not have the resources needed to identify or train qualified job candidates and therefore had little organizational investment in JobNet.

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*Sources:* Molina (1998); Lisa Nisenfeld, former head of JobNet division of PDC, interview with the author, December 1999.

"This is based on extrapolating JobNet's 1994 to 1995 placements per capita in Multnomah County (the PDC was responsible for business recruitment throughout the Portland area, and thus JobNet to some extent was active throughout the local labor market, not just the city of Portland) to the U.S. population in 1998.

## *SIDEBAR 9.2 North Carolina's Community College Programs for Business and Industry*

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North Carolina's industrial training programs help achieve two goals: orienting the state's community colleges toward the needs of employers, and encouraging firms to hire a more diverse workforce. Overall, North Carolina spent \$44 million on industrial training programs in 1998, and 319,000 workers received such training in 1997. On a national scale, this would have been equivalent to spending \$654 million annually and training 4.8 million workers."

The business and industry program in North Carolina's fifty-eight community colleges offers three training programs: New and Expanding Industry Training (NEIT), Occupational Continuing Education (OCE), and Focused Industry Training (FIT).

NEIT goes back to the late 1950s, when North Carolina became the first state to use customized training to attract new branch plants. The state set up industrial education centers to deliver this training. These centers became the nucleus for many community colleges when North Carolina in 1963 consolidated its twenty industrial education centers and six community colleges.

Firms are eligible for NEIT if they are creating at least twelve jobs in some industry that exports goods or services outside the state. Eighty percent of NEIT clients are in manufacturing. Half are firms new to the state, and half are expanding firms. NEIT is offered to new firms by development agencies and marketed to existing firms by community colleges. Under NEIT, the college places ads for employers and screens trainees, often subcontracting to the local employment service office the tasks of reviewing applications and administering tests. The firm chooses trainees from among those screened. The firm typically provides the equipment, while the community college provides the facilities and the trainers. NEIT typically provides eighty to five hundred hours of training. The firm decides which trainees will be hired.

The NEIT program is totally funded by state dollars. Its 1998 budget was \$10.1 million. Twenty-five thousand workers, almost all new hires, were trained in 1997, at an average cost (in 1998 dollars) of \$391 per trainee.

OCE provides more short-term and limited training, but with fewer eligibility requirements. For a fee of \$35 per course, the community colleges offer courses that provide occupationally relevant training. Sixty percent of the courses are arranged by employers, and 40 percent by individual workers. Courses are customized to the needs of an individual firm if there are at least ten to twelve trainees from the firm. Forty percent of those trained under OCE programs are new hires.

OCE's budget in 1998 was \$31 million. In 1997, 285,000 workers were trained under OCE, at an average cost per trainee of only \$86 (in 1998 dollars). Community colleges are reimbursed the next fiscal year, including overhead, for the number of OCE trainees they enroll.

FIT was set up to provide customized training to smaller manufacturers that do not have sufficient numbers of potential trainees to justify customized training under

## SIDEBAR 9.2 *Continued*

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either of the other two programs. Only manufacturers are eligible, and the training must be arranged by the employer. The state funds FIT up front. Spending on FIT in 1998 was \$3.1 million. Nine thousand workers were trained by FIT in 1997, at an average cost of \$384 each (in 1998 dollars).

North Carolina's industrial training programs are not explicitly targeted at the disadvantaged. However, by using community colleges so extensively in the screening and training process, many firms end up hiring a somewhat more diverse workforce. Enrollment in North Carolina's community college system is 20 percent black. Community colleges often administer local job training agencies, and half of North Carolina's "one-stop shop" training centers under the Workforce Investment Act are located at community colleges. Batt and Osterman's (1993, 34) case study argues that community colleges can sometimes use their multiple roles to give some disadvantaged persons access to better jobs. For example, "because [Durham Technical Institute] administers JTPA and state welfare-to-work programs, it has the opportunity to mainstream more disadvantaged students in these [industrial] training programs and has done so. Mothers on welfare, for example, have received training and placement in electronic assembly jobs through [NEIT] without being stigmatized or isolated for special treatment."

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*SOURCES:* Regional Technology Strategies (1999); Batt and Osterman (1993); Osterman and Batt (1993).

"This is calculated by simply extrapolating North Carolina's per capita numbers based on total U.S. population in 1998.

### *SIDEBAR 9.3 Florida's Performance-Based Incentive Funding Program*

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Florida's Performance-Based Incentive Funding (PBIF) program is an interesting experiment in incentives to motivate educational institutions to pursue economic development and workforce development goals, with special attention to the needs of the disadvantaged. So far the program would have to be given a grade of "incomplete, but promising."

PBIF was established by the Florida legislature in 1994. The program is administered by Enterprise Florida, the public-private entity that administers Florida's economic development efforts. Under PBIF, extra funds are allocated to local community colleges and school districts that enroll students in occupationally specific training programs and place them in these occupations, if these occupations are designated as high-demand and high-wage. High-demand occupations are designated annually by Enterprise Florida. High-wage is defined as \$9 per hour in general, but \$7.50 per hour for welfare recipients. Extra incentives are provided for those in various disadvantaged groups (economically disadvantaged, disabled, displaced workers, limited English proficiency). Incentives are allocated based on *increases* since a base year in program enrollment, program completion, and occupational placement.

In the 1995 to 1996 program year, the PBIF program handed out \$10 million in incentive grants. Overall placements in PBIF-designated occupations of those who completed the program increased from 10,213 in the 1992 to 1993 baseline year to 16,851 in the 1995 to 1996 program year. Placements for disadvantaged groups increased from 2,118 in the baseline year to 5,077 in the 1995 to 1996 program year. However, only 240 of these PBIF placements were welfare recipients.

Has this program been successful? The program has increased educational activity in the designated occupations. However, it is unclear whether the program has led to changes in educational institutions to better meet the needs of disadvantaged students. For example, it is unclear whether placements of disadvantaged students have really increased or schools are just keeping better track of whether a student is in a target group. The small number of welfare recipients placed is distressing. Some community colleges have set up special courses for welfare recipients. However, there have been no major changes in outreach, support services, entrance requirements, or course design.

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*Sources:* Roberts and Padden (1998b); Florida Office of Program Policy Analysis and Government Accountability (1998); Switzer (1999).

## *SIDEBAR 9.4 The Minneapolis Neighborhood Employment Network*

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The Minneapolis Neighborhood Employment Network (NET) was started in 1981 as a first-source program: firms that received assistance from the Minneapolis city government were required to use this program as a source for entry-level job hiring. However, over time NET has come to rely little on requirements placed on employers. Rather, NET appeals to employers by offering an easier source of high-quality labor supply for new hires, in the context of a high-aggregate-demand local labor market. NET appeals to its affiliates by offering access to jobs throughout the local labor markets and providing cost savings by having many neighborhood groups share services, ideas, and peer support.

The NET director or job broker is located in the Minneapolis mayor's offices. NET has eleven neighborhood affiliates. It employs two job developers who aggressively search out new job listings. Information on these job listings is distributed through a computer network with links to each neighborhood. After receiving a job listing, neighborhood affiliates recruit and screen appropriate job candidates and directly contact the employer. NET affiliates regularly meet with the director to discuss common problems. According to a case study of NET (Molina 1998, 28), "there is considerable peer pressure among the partners to refer only good matches in order to retain employers' credibility in the whole network." Funding for the NET affiliates comes from the Job Training Partnership Act (JTPA) and the federal Community Development Block Grant program. The NET director's position is funded by private funds.

No formal evaluation has been done of NET, but the program has succeeded in achieving a significant number of job placements for relatively disadvantaged persons, at modest costs. In the fiscal year 1996, the program placed 1,706 persons. This is equivalent to making 1.3 million job placements nationally in 1998.<sup>a</sup> All those placed have household income less than 80 percent of median household income in the Twin Cities metropolitan area. Thirty-five percent of those placed received some type of welfare assistance before being placed. The cost of the NET director and the NET affiliates per placement is \$790 (in 1998 dollars). The total cost of an equivalent national program would be \$1.0 billion (in 1998 dollars).

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*Source:* Molina 1998.

<sup>a</sup> This is calculated by extrapolating Minneapolis's per capita job placement rate to the U.S. population in 1998.

## *SIDEBAR 9.5 The Milwaukee Careers Cooperative*

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The Milwaukee Careers Cooperative (MCC) is a nonprofit, temporary-to-permanent placement agency that specializes in placing workers in light industrial work and primarily serves young African American male workers in five neighborhoods in central Milwaukee. MCC was founded in 1987 by a group of church congregations and community organizations.

MCC's staff of six includes one developer-account executive who solicits job orders from area employers for MCC, just as is done by ordinary temp agencies. Job seekers go through job retention training and some screening. MCC places these workers with employers under thirty- to ninety-day contracts, but only if a permanent job is available at the end for those who perform satisfactorily. As with a regular temp agency, the employer pays MCC a fee, and MCC pays the worker during the contract period. No extra fee is charged for a permanent placement. One week after workers are placed, MCC runs a Saturday morning workshop for workers providing advice on job retention and an opportunity to discuss problems that might have developed on the job. MCC workers are provided with transportation to work via a van service for up to six months after being permanently placed. The van service allows MCC workers to be available at odd shift times, giving them an important comparative advantage with suburban industrial employers.

MCC has achieved fairly high placement levels and has provided services valuable enough that employers have been willing to pay substantial fees. In the 1996 fiscal year, MCC placed 1,132 persons, or 61 percent of those who came to MCC seeking jobs. This is equivalent to making 516,000 placements nationally in 1998.<sup>a</sup> Most MCC clients are twenty to twenty-four years old, two-thirds are males, and 86 percent are African American. The costs of MCC, including paying wages to its temp workers, are about \$1,468 per placement (in 1998 dollars). Seventy-five percent of MCC's costs are covered by employer fees, and the remaining costs are covered by government and foundation grants, area congregations, and individual contributions.

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*Source:* Seavey (1998).

<sup>a</sup> This is calculated by extrapolating MCC's placement rate as a percentage of Milwaukee's population in 1996 to the national population in 1998.

TABLE 9.1 *A Typology of State and Local Economic Development Policies*

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<p>Traditional Economic Development Policies (Targeted at Recruitment and Expansion of Larger Businesses)</p>	
<hr/>	
1.	Marketing area as branch plant location <ul style="list-style-type: none"> <li>• Industrial development advertising</li> <li>• Marketing trips to corporate headquarters</li> <li>• Provision of site information to prospects</li> </ul>
2.	Financial incentives <ul style="list-style-type: none"> <li>• Industrial revenue bonds</li> <li>• Property tax abatements</li> <li>• Other tax relief</li> <li>• Provision of land at below-market prices</li> </ul>
3.	Nonfinancial incentives <ul style="list-style-type: none"> <li>• Customized training</li> <li>• Expedited provision of site-specific roads and utilities</li> <li>• Help with regulatory problems</li> </ul>
<p>“New Wave” Economic Development Policies (Targeted at Small or Start-Up Businesses)</p>	
<hr/>	
1.	Capital market programs <ul style="list-style-type: none"> <li>• Predominantly government-financed loan or equity programs</li> <li>• Government support for predominantly privately financed loan or equity programs</li> </ul>
2.	Information/education for small businesses <ul style="list-style-type: none"> <li>• Small business ombudsman and information office</li> <li>• Community college classes in starting a business</li> <li>• Small business development centers</li> <li>• Entrepreneurial training programs</li> <li>• Small business incubators</li> </ul>
3.	Research and high technology <ul style="list-style-type: none"> <li>• Centers of excellence in business-related research at public universities</li> <li>• Research-oriented industrial parks</li> <li>• Applied research grants</li> <li>• Technology transfer programs and industrial extension services</li> </ul>
4.	Export assistance <ul style="list-style-type: none"> <li>• Information and training in how to export</li> <li>• Trade missions</li> <li>• Export financing</li> </ul>
5.	Networking programs <ul style="list-style-type: none"> <li>• Networks of small businesses working with local community colleges to define training needs and create specialized courses for particular industries (a different variety of “customized training”)</li> <li>• Cooperative arrangements among small firms with similar but not directly competing products to share services such as consultants, shipping, and marketing</li> </ul>

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Source: Author's compilation.



TABLE 9.2 *Share of Benefits from Minimum-Wage Increases Going to Workers at Different Levels of Family Income*

Income-to-Needs Ratio of Worker's Family	Percentage of All Workers	Percentage Share of Benefits from Minimum-Wage Increase
Less than 1.00	6.1	19.3
1.00 to 1.25	2.8	8.4
1.25 to 1.50	3.3	6.4
1.50 to 2.00	8.2	12.2
2.00 to 2.99	17.9	21.2
3.00 and above	61.7	32.5

*Source:* Burkauer et al. (1996, 550, table 2).

*Notes:* The minimum-wage hike considered is the 1990 to 1991 hike from \$3.35 per hour to \$4.25 per hour. The income-to-needs ratio is ratio of family income to poverty line for that family size. The benefits from a minimum-wage increase are calculated assuming that there are no employment or hours effects, and that wages for everyone in the range from \$3.35 to \$4.25 per hour go up to \$4.25 per hour owing to the minimum-wage hike, but that no other wages change. The underlying data are calculated by Burkauer et al. (1996) from the March 1990 CPS-ORG.

TABLE A1.1 *Implications of Demand and Supply Elasticities for the Effects of Demand and Supply Policies on the Wages and Employment of Less-Educated Workers*

Scenario	Assumptions About Demand and Supply Behavior <sup>a</sup>	Employment and Wage Elasticities of Various Policies <sup>b</sup>							
		Quantity Increase in Labor Demand		Quantity Increase in Labor Supply		Demand-Side Wage Subsidy to Employer		Supply-Side Wage Subsidy to Worker	
		Employment	Wage	Employment	Wage	Employment	Wage	Employment	Wage
Baseline	Elasticity of demand with respect to wage of $-0.5$ , elasticity of demand with respect to unemployment rate of $1.5$ , elasticity of wages with respect to one-point increase in unemployment rate of $(-0.6)$ .	0.50	0.16	0.50	$-0.16$	0.25	0.08	0.20	$-0.07$
Short run	Same as baseline, but elasticity of demand with respect to one-point increase in unemployment rate of $0.75$ .	0.64	0.21	0.36	$-0.21$	0.32	0.10	0.15	$-0.08$
Greater response of demand to unemployment	Same as baseline, but elasticity of demand with respect to one-point increase in unemployment rate of $3.0$ .	0.36	0.12	0.64	$-0.12$	0.18	0.06	0.26	$-0.05$

Greater response of wages to unemployment	Same as baseline, but elasticity of wages with respect to one-point increase in unemployment rate of (-2.0).	0.49	0.41	0.51	-0.41	0.24	0.20	0.20	-0.16
Greater response of labor demand to wages	Same as baseline, but elasticity of labor demand with respect to wages of (-1.5).	0.43	0.14	0.57	-0.14	0.65	0.21	0.23	-0.06
Long-run effects with unemployment returning to long-run frictional level	Same as baseline, but elasticity of wages with respect to one-point increase in unemployment rate of (-1,000).	0.44	1.11	0.56	-1.11	0.22	0.55	0.22	-0.44
Long-run market-clearing model with high elasticity of demand	Same as long-run model, but with elasticity of labor demand with respect to wages of (-1.5).	0.21	0.53	0.79	-0.53	0.32	0.79	0.32	-0.21

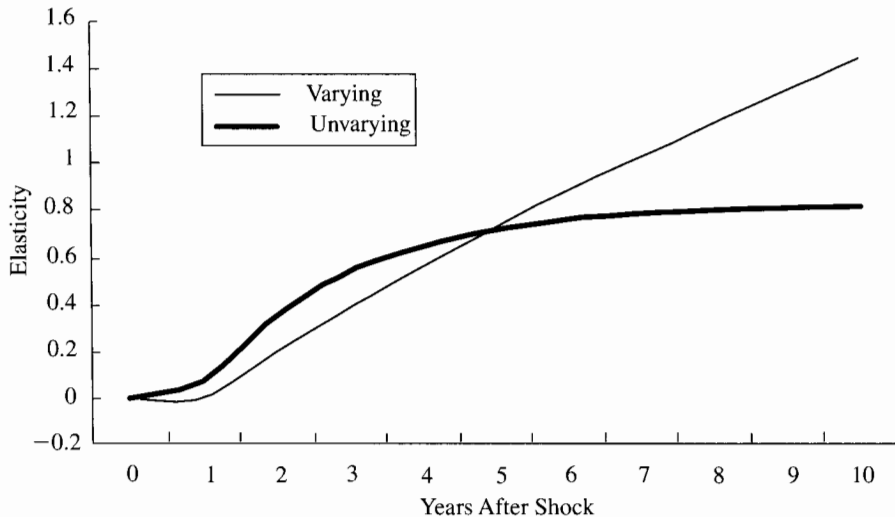
*Source:* Author's calculations.

*Note:* See appendix for model and equations used to produce these numbers.

<sup>a</sup> All simulations assume unemployment of eight percent, elasticity of labor supply with respect to wages of 0.4, and elasticity of labor supply with respect to a one-point change in unemployment of -0.5.

<sup>b</sup> Percentage change in employment or wages of less-educated workers for a 1 percent increase in labor quantity or a 1 percent wage subsidy.

FIGURE A2.1 *(-1) Times Elasticity of Overall Employment Demand with Respect to Overall Wages, with and Without Income Varying*

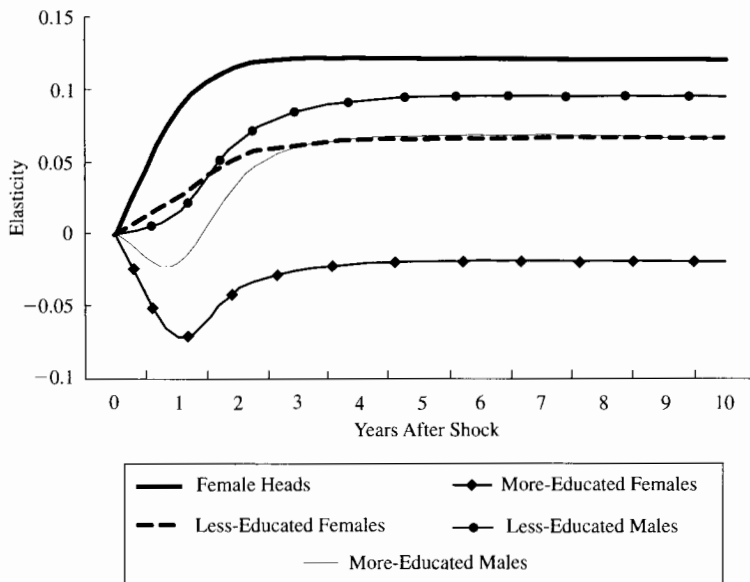


	Income Not Allowed to Vary	Income Allowed to Vary
Five years	0.717 (11.22)	0.722 (5.84)
Ten years	0.808 (12.43)	1.431 (4.48)

Source: Author's estimates.

Notes: Pseudo *t*-statistics, derived from 1,000 Monte Carlo repetitions of simulations, are in parentheses. Elasticities are derived from decreasing overall wages by 10 percent. The "income not allowed to vary" elasticities are based on simulations using only the overall labor demand evaluation of the model. Simulations with "income allowed to vary" also allow the income equation in the model to become operational, with feedback between labor demand and income equations.

FIGURE A2.2 *(-1) Times Elasticity of Relative Labor Demand for Each Group with Respect to That Group's Relative Wage*

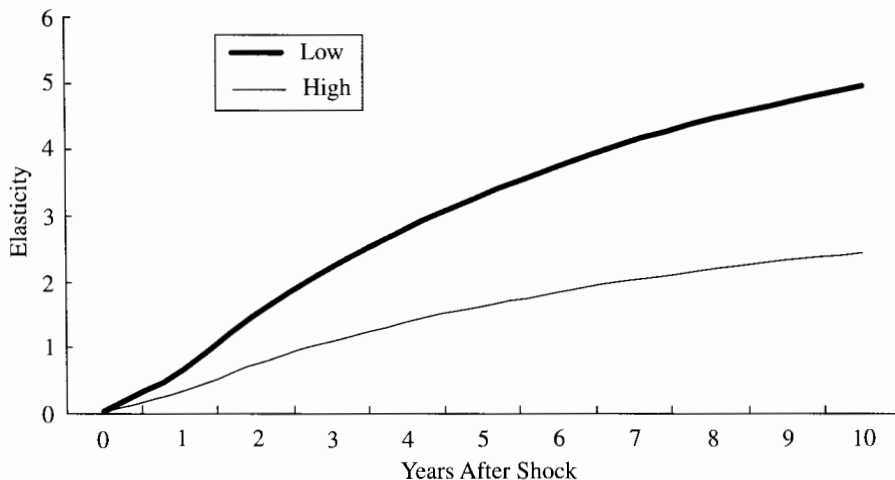


	Female Heads	Other Less-Educated Females	More-Educated Females	Less-Educated Males	More-Educated Males
Five-year elasticity	0.122 (1.29)	0.067 (4.24)	-0.019 (-0.26)	0.096 (3.58)	0.068 (1.79)

Source: Author's estimates.

Note: Pseudo *t*-statistics are in parentheses, derived from 1,000 Monte Carlo repetitions of effects of shock. Effects are  $(-1)$  times elasticity of  $\ln(\text{employment in group}/\text{employment overall})$  with respect to  $\ln(\text{wage of group}/\text{overall wage})$ . Elasticities are derived from decreasing the wage of the group by .10, with resulting effects multiplied by ten to generate table and figure. Each simulation allows only the group's relative demand equation to become operational.

FIGURE A2.3 *Elasticity of Overall Wages with Respect to Unemployment Under Conditions of High and Low Initial Unemployment*

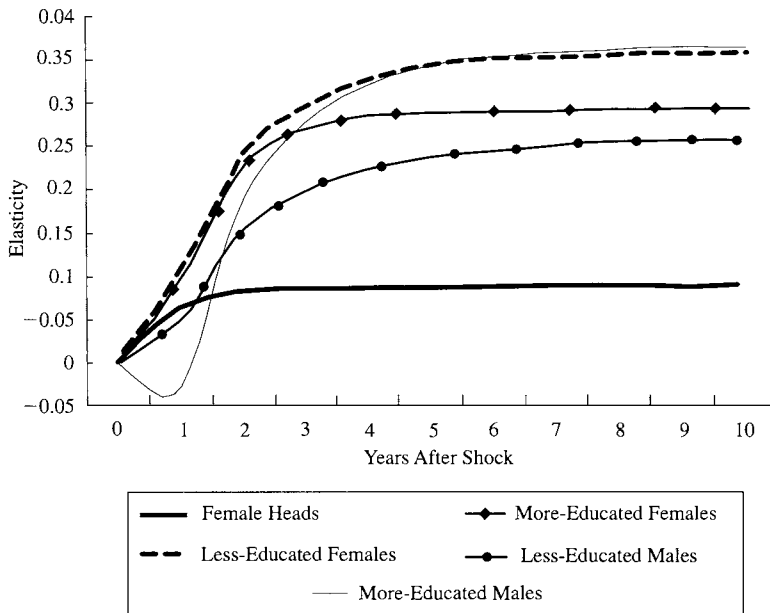


Elasticity of Wages with Respect to 1 Percentage Point Reduction in Unemployment Rate	Low Unemployment at Baseline	High Unemployment at Baseline
After five years	3.317 (15.13)	1.629 (15.13)
After ten years	4.959 (12.40)	2.435 (12.40)

Source: Author's estimates.

Notes: Results are change in  $\ln(\text{wage}) \times 100$ , for 1 percent reduction in unemployment rate of all five groups. A change from a 6 percent to a 5 percent unemployment rate is a 1 percent change in the unemployment rate. Base unemployment rates in low unemployment baseline are actual national unemployment rates for the five groups in 1997. For high unemployment baseline, actual national unemployment rates for the five groups as of 1982 were used. 1982 and 1997 were highest and lowest unemployment years in nation from 1979 to 1997. See table A2.2 for the actual values of unemployment rates for each group in 1982 and 1997.

FIGURE A2.4 *Group-Specific Elasticity with Respect to Unemployment for Group-Specific Wages Relative to Overall Wages Under Conditions of Low Initial Unemployment*

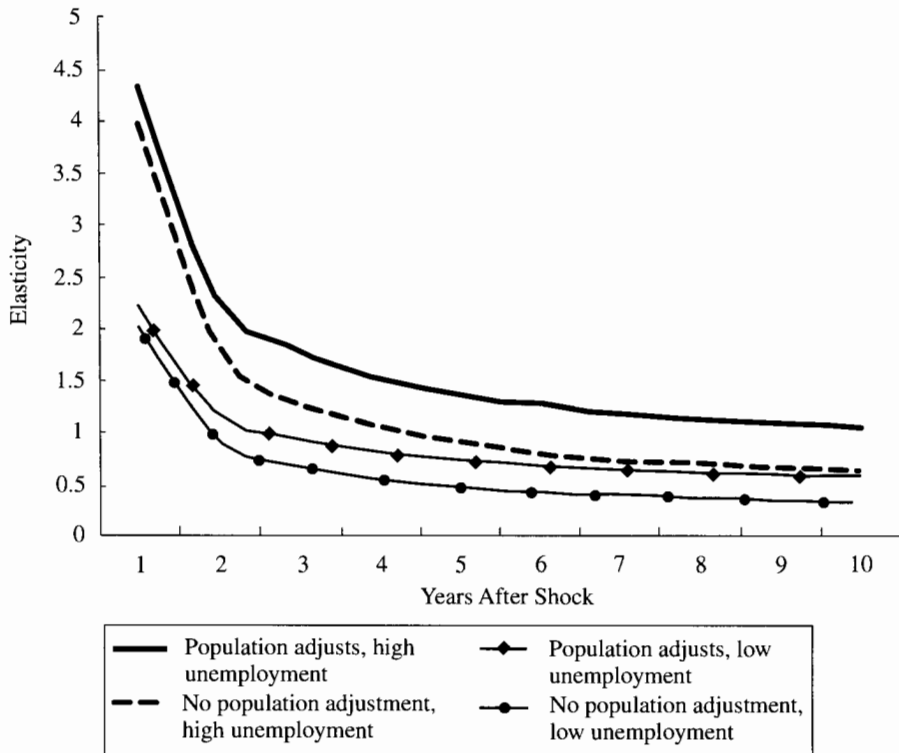


	Female Heads	Other Less-Educated Females	More-Educated Females	Less-Educated Males	More-Educated Males
Low unemployment baseline	0.085 (3.70)	0.341 (3.02)	0.287 (2.97)	0.236 (3.70)	0.342 (2.27)
High unemployment baseline	0.047 (3.84)	0.142 (3.86)	0.273 (2.83)	0.173 (3.18)	0.322 (2.15)

Source: Author's estimates.

Notes: Pseudo *t*-statistics are in parentheses, derived from 1,000 Monte Carlo repetitions of simulation. Simulation shows effect on 100 times  $\ln(\text{wage of group} / \text{average overall wage})$  of reduction of 1 percent in unemployment of that group, with unemployment rates for other groups staying unchanged.

FIGURE A2.5 Complete Labor Supply Elasticities with and Without Population Adjustment Under Conditions of High (Bold Lines) and Low (Regular Lines) Initial Unemployment



	Low Initial Unemployment Rate, No Population Adjustment Allowed	Low Initial Unemployment Rate, with Population Adjustment	High Initial Unemployment Rate, No Population Adjustment Allowed	High Initial Unemployment Rate, with Population Adjustment
Effective labor supply elasticity after five years	0.473 (12.71)	0.725 (10.14)	0.912 (12.94)	1.351 (10.12)

Source: Author's estimates.

Notes: These numbers are derived from a simulation that specified an exogenous 1 percent reduction in the unemployment rate for all groups and used wage curve equations, labor force participation rate equations, and population migration equations to simulate the effects of this change on wages and the labor force. The labor demand side of the model is suppressed for this exercise and is not allowed to adjust. The effective labor supply elasticity is the ratio of the change in the natural logarithm of the overall labor force to the change in the average log wage. Pseudo *t*-statistics from 1,000 Monte Carlo repetitions of simulation are in parentheses. Low initial unemployment rates are national unemployment rates of 1997 (overall average: 5.0 percent), and high initial unemployment rates are national unemployment rates of 1982 (overall average: 9.9 percent).



TABLE A2.2 *National Means of Labor Market Outcomes for Five Demographic Groups and Overall Population Ages Sixteen to Sixty-Four, 1997*

	Female			Male		
	Household Heads Ages Sixteen to Forty-Four, Other Relatives Present, Less Than College Education	Ages Sixteen to Sixty-Four, Less Than College Education, Excluding First Group	College Graduates, Ages Sixteen to Sixty-Four	Less Than College Education, Ages Sixteen to Sixty-Four	College Graduates, Ages Sixteen to Sixty-Four	Overall Population, Ages Sixteen to Sixty-Four
Proportion of population	0.0399	0.3569	0.1091	0.3782	0.1159	1.0000
Wage rate	\$ 8.49	\$ 8.80	\$15.19	\$10.85	\$18.85	\$11.29
Unemployment rate (1982 rate in parentheses)	11.0 (15.4)	5.4 (10.2)	2.2 (4.2)	6.0 (12.0)	1.9 (3.1)	5.0 (9.9)
Labor force participation rate	75.7	66.5	82.8	81.5	93.1	77.4

*Source:* All data are taken from 1997 Merged Outgoing Rotation Group data tape of the CPS, with the exception of the unemployment rate data for 1982.

*Notes:* All means are weighted national means using appropriate weights from tape. Mean wage rate is actually  $\exp(\text{mean } \ln[\text{hourly wage}])$ . Over sample period of 1979 to 1997, 1997 is the year with the lowest national unemployment rate for sixteen- to sixty-four-year-olds, and 1982 is the year with the highest national unemployment rate. The patterns of unemployment in these two years are used as alternative baselines in simulating the effects of supply and demand shocks.

TABLE A2.3 Overall Labor Demand Equation (Dependent Variable:  $\ln(\text{Employment})$ )

Independent Variable	Coefficient ( <i>t</i> -statistic)
Lag $\ln(\text{employment})$	0.6900 (19.49)
Second lag	-0.0567 (-1.72)
Lag $\ln(\text{wage})$	-0.0779 (-1.74)
Second lag	-0.2210 (-4.85)
Lag $\ln(\text{personal income})$	0.4377 (9.50)
Second lag	-0.1524 (-3.12)

Source: Author's estimates.

Note: The estimation is based on 867 observations, for fifty states plus D.C., times seventeen years (1981 to 1997). Independent variables also include complete vectors of year dummies (sixteen variables) and state dummies (fifty variables) to control for fixed time period and state effects; coefficients on these variables are not reported to save space. The estimation uses 1979 state population as weight for each state-year cell observation. R-squared is 0.9996.

TABLE A2.4 *Relative Labor Demand Equations (Dependent Variables: ln(Employment of Group/Total Employment))*

Independent Variable	Female Heads	Other Less-Educated Females	More-Educated Females	Less-Educated Males	More-Educated Males
Lag ln(employment share of group)	0.2713 (2.66)	0.0719 (2.34)	0.3549 (4.40)	0.3999 (5.15)	0.3864 (5.62)
Second lag	-0.0434 (-1.64)	0.0032 (0.25)	-0.0227 (-0.92)	-0.0291 (-1.27)	-0.0685 (-2.83)
Lag ln(relative wage of group)	-0.0915 (-1.41)	-0.0502 (-2.50)	0.0923 (1.93)	-0.0333 (-0.99)	0.0340 (1.03)
Second lag	0.0015 (0.02)	-0.0124 (-0.65)	-0.0647 (-1.26)	-0.1170 (-3.29)	-0.0810 (-2.52)
Current ln(labor force share of group)	0.3752 (1.56)	0.8503 (14.00)	0.3008 (1.96)	0.4277 (3.44)	0.4009 (3.98)

Source: Author's estimates.

Notes: A *t*-statistic is in parentheses below each coefficient estimate. Each column shows estimates for one equation; this equation's dependent variable is the ln(employment share) for the group given in the column headings. The estimations in each of five equations presented here are based on 867 observations, for fifty states plus D.C.,  $\times$  seventeen years (1981 to 1997). Independent variables also include complete vectors of year dummies (sixteen variables) and state dummies (fifty variables) to control for fixed time period and state effects; coefficients on these variables are not reported to save space. The estimation uses 1979 state population as weight for each state-year cell observation. Estimation is by 2SLS, with current labor force share treated as endogenous and lagged labor force share used as instrument. R-squared not reported because this statistic is not meaningful for 2SLS. In the first stage of 2SLS, the *t*-statistic on the lagged labor force share instrument for each of the five equations is as follows: female head, 2.98; other less-educated females, 5.62; more-educated females, 4.65; less-educated males, 5.36; more-educated males, 6.05.

TABLE A2.5 Overall Wage Equation (Dependent Variable:  
 $\ln(\text{Wage})$ )

Independent Variable	Coefficient ( <i>t</i> -statistic)
Lag $\ln(\text{wage})$	0.7318 (20.29)
Second lag	0.1030 (2.96)
Lag $\ln(\text{unemployment rate plus } 0.005)$	-0.0314 (-6.71)
Second lag	-0.0217 (-4.52)

Source: Author's compilation.

Notes: The estimation is based on 867 observations, for fifty states plus D.C.,  $\times$  seventeen years (1981 to 1997). Independent variables also include complete vectors of year dummies (sixteen variables) and state dummies (fifty variables) to control for fixed time period and state effects; coefficients on these variables are not reported to save space. The estimation uses 1979 state population as weight for each state-year cell observation. R-squared is 0.9816. The unemployment rate is defined as  $\ln(\text{labor force} - \ln(\text{employment}))$ . The specification then takes the logarithm of this unemployment rate definition. 0.005 is added to avoid problems with occasional zeros for some smaller groups. The logarithmic specification was chosen by Akaike Information Criterion over linear specification, specification with  $1 / \text{unemployment rate}$ , and specification with unemployment rate and unemployment rate squared.

TABLE A2.6 *Relative Wage Curves (Dependent Variables:  
ln(Wage of Group/Average Wages Overall))*

Independent Variable	Female Heads	Other Less-Educated Females	More-Educated Females	Less-Educated Males	More-Educated Males
Lag ln(relative wage)	0.2332 (6.47)	0.3227 (9.05)	0.3090 (8.81)	0.3592 (10.31)	0.4426 (12.67)
Second lag	-0.0463 (-1.28)	0.0862 (2.44)	-0.0139 (-0.39)	0.1555 (4.48)	0.0146 (0.42)
Lag ln(relative unemployment variable)	0.001035 (4.35)	0.000567 (1.75)	-0.1150 (-1.54)	-0.0419 (-0.61)	0.0539 (0.49)
Second lag	-0.000045 (-0.22)	0.000496 (1.51)	-0.1021 (-1.37)	-0.1422 (-2.06)	-0.2779 (-2.51)

Source: Author's estimates.

Notes: A *t*-statistic is in parentheses below each coefficient estimate. Each column shows results from estimating a different equation; the dependent variable for each equation is the ln(relative wage) for the group named in the column heading. The estimation of each of the five equations presented in this table is based on 867 observations, for fifty states plus D.C.,  $\times$  seventeen years (1981 to 1997). Independent variables also include complete vectors of year dummies (sixteen variables) and state dummies (fifty variables) to control for fixed time period and state effects; coefficients on these variables are not reported to save space. Estimation uses 1979 state population as weight for each state-year cell observation. R-squared for each estimated equation is as follows: female heads, 0.5334; other less-educated females, 0.8279; more-educated females, 0.8906; less-educated males, 0.9261; more-educated males, 0.7549. The relative unemployment variable specification in each equation was chosen separately, based on Akaike Information Criterion, among the following possible unemployment rate transformations: linear ( $UR_g - UR$ ); log-linear ( $\ln[UR_g] - \ln[UR]$ ); one over UR specification ( $1/UR_g - 1/UR$ ); and quadratic. Optimal specification was  $1/UR$  for female heads and other less-educated females, linear for all others.

TABLE A2.7 *Labor Force Participation Rate Equations (Dependent Variables:  $\ln(\text{Labor Force Participation Rate for That Group})$ )*

Independent Variable	Female Heads	Other Less-Educated Females	More-Educated Females	Less-Educated Males	More-Educated Males
Lag $\ln(\text{lfpr for group})$	0.4140 (11.62)	0.4923 (13.88)	0.3246 (9.53)	0.4324 (12.14)	0.2730 (7.58)
Second lag	-0.0227 (-0.63)	0.1131 (3.80)	-0.0808 (-2.39)	-0.0044 (-0.12)	-0.0802 (-2.24)
Lag $\ln(\text{wage rate for group})$	0.0169 (0.39)	0.0188 (0.48)	0.0160 (0.64)	-0.0149 (-0.77)	0.0177 (1.45)
Second lag	0.0448 (1.05)	-0.0323 (-0.83)	-0.0595 (-2.40)	0.0332 (1.76)	0.0013 (0.11)
Lag $\ln(\text{unemployment rate for group})$	-0.1482 (-3.04)	-0.2527 (-3.85)	-0.1990 (-2.47)	-0.1124 (-3.46)	-0.1136 (-2.20)
Second lag	-0.0351 (-0.72)	-0.0387 (-0.58)	0.0876 (1.10)	-0.1068 (-3.18)	-0.0270 (-0.52)
Lag $\ln(\text{AFDC benefit level})$	-0.1201 (-2.17)	—	—	—	—
Second lag	0.0153 (0.30)	—	—	—	—

Source: Author's estimates.

Notes: A *t*-statistic is in parentheses below each coefficient estimate. Each column shows empirical estimates for a different equation; the dependent variable for this equation is the  $\ln(\text{labor force participation rate})$  for the group named in the column heading. The estimate of each of the five equations presented in this table is based on 867 observations, for fifty states plus D.C.,  $\times$  seventeen years (1981 to 1997). Independent variables also include complete vectors of year dummies (sixteen variables) and state dummies (fifty variables) to control for fixed time period and state effects; coefficients on these variables are not reported to save space. Estimation uses 1979 state population as weight for each state-year cell observation. R-squared for each equation is as follows: female heads, 0.8370; other less-educated females, 0.9533; more-educated females, 0.7449; less-educated males, 0.9063; more-educated males, 0.5608. AFDC benefits allowed to affect only labor force participation of female-head group, on argument that this is the only group of the five that is very likely to be eligible for welfare benefits. Estimated welfare receipt rate for female-head group is 28.6 percent; the estimated receipt rate for other groups is: other less-educated women, 2.5 percent; more-educated women, 0.3 percent; less-educated men, 0.5 percent; more-educated men, 0.1 percent. These are based on welfare receipt rates from March 1997 CPS, blown up by 1.33 based on Blank's (1997) estimates of how much of welfare receipt is underreported.

TABLE A2.8 *Migration Equations (Dependent Variables:  
ln(Population for That Group))*

Independent Variable	Other				
	Female Heads	Less- Educated Females	More- Educated Females	Less- Educated Males	More- Educated Males
Lag ln(population for group)	0.5839 (16.40)	0.7782 (22.13)	0.6112 (17.54)	0.8547 (23.95)	0.7084 (20.06)
Second lag	-0.0935 (-2.61)	0.0872 (2.56)	-0.0348 (-1.03)	0.0400 (1.14)	-0.0604 (-1.77)
Lag ln(wage rate for group)	-0.1098 (-1.63)	0.0427 (1.02)	0.1628 (3.01)	0.06451 (1.72)	0.1664 (3.39)
Second lag	0.0379 (0.56)	-0.0629 (-1.53)	-0.0595 (-1.10)	-0.0739 (-2.00)	-0.0147 (-0.29)
Lag ln(unemployment rate for group)	-0.0509 (-0.66)	-0.0153 (-0.22)	-0.1422 (-0.81)	-0.0583 (-0.89)	-0.7644 (-3.64)
Second lag	-0.0104 (-0.14)	-0.0664 (-0.94)	-0.0624 (-0.36)	-0.0842 (-1.30)	-0.3288 (-1.56)
Lag ln(AFDC benefit level)	0.0255 (0.30)	—	—	—	—
Second lag	0.0989 (1.24)	—	—	—	—

*Source:* Author's estimates.

*Notes:* A *t*-statistic is in parentheses below each coefficient estimate. Each column shows coefficient estimates for a different equation; the dependent variable for the equation is the ln(state population) for the group named in the column heading. The estimate of each of the five equations presented in this table is based on 867 observations, for fifty states plus D.C.,  $\times$  seventeen years (1981 to 1997). Independent variables also include complete vectors of year dummies (sixteen variables) and state dummies (fifty variables) to control for fixed time period and state effects; coefficients on these variables are not reported to save space. Estimation uses 1979 state population as weight for each state-year cell observation. R-squared for each equation is as follows: female heads, 0.9925; other less-educated females, 0.9995; more-educated females, 0.9975; less-educated males, 0.9994; more-educated males, 0.9978.

TABLE A2.9 *Personal Income Equation: (Dependent Variables:  
ln(State Personal Income))*

Independent Variable	Coefficient (t-statistic)
ln(Wage rate)	0.1862 (5.30)
Lag	-0.0903 (-2.12)
Second lag	-0.1126 (-3.37)
ln(Employment)	0.2670 (7.31)
Lag	-0.0053 (-0.12)
Second lag	-0.1350 (-3.62)
ln(Population)	-0.1539 (-3.18)
Lag	-0.0600 (-1.01)
Second lag	0.1492 (3.28)
Lag ln(Personal income)	1.0651 (28.72)
Second lag	-0.1688 (-4.20)

*Source:* Author's estimates.

*Notes:* The estimation is based on 867 observations, for fifty states plus D.C.,  $\times$  seventeen years (1981 to 1997). Independent variables also include complete vectors of year dummies (sixteen variables) and state dummies (fifty variables) to control for fixed time period and state effects; coefficients on these variables are not reported to save space. Estimation uses 1979 state population as weight for each state-year cell observation. R-squared is 0.9999.



TABLE A2.1 *A Wage Curve Model of State Labor Markets*

Type of Equation	Dependent Variable	Independent Variables <sup>a</sup>
Overall labor demand (one equation)	ln(state employment)	ln(wage) ln(personal income)
Employment share demand (five equations)	ln(share of employment in group)	ln(wage of group/overall wage) <i>Current</i> value only of ln(labor force share of group) [endogenous, lagged labor force share used as instrument]
Overall wage curve (one equation)	ln(wage)	ln(unemployment rate)
Relative wage curves (five equations)	ln(wage of group/overall wage)	Some function of relative unemployment of group, with functional form chosen for each group after preliminary testing
Labor force participation rate (five equations)	ln(labor force participation rate of group)	ln(wage of group) Unemployment rate of group ln(AFDC benefits for female-head group)
Migration (five equations)	ln(population of group)	Same as for labor force participation rate
Income (one equation)	ln(state personal income)	ln(wage) ln(employment) ln(population) Includes current as well as lagged values of these variables

*Source:* Author's compilation.

*Notes:* All estimates are based on pooled annual time-series cross-section data for all states, 1979 to 1997. All estimates are weighted by 1979 state population and use weighted least squares except employment share demand, which is weighted 2SLS.

<sup>a</sup> These are independent variables in addition to year and state dummies, and two lags of dependent variable. All independent variables are included with two lags and no current values unless otherwise noted.

TABLE A2.10      *Sensitivity of Relative Labor Demand to Relative Labor Supply, Holding Relative Wages Constant*

Group	Short-Run (Immediate) Elasticity	Implied Long-Run Elasticity
Female heads	0.375 (1.56)	0.486
Other less-educated females	0.850 (14.00)	0.919
More-educated females	0.301 (1.96)	0.451
Less-educated males	0.428 (3.44)	0.680
More-educated males	0.401 (3.98)	0.588

*Source:* Author's estimates.

*Notes:* Effects are derived directly from parameter estimates in relative labor demand elasticities. Short-run effect is from equation explaining  $\ln(\text{employment share of group})$  and is coefficient on  $\ln(\text{labor force share of group})$ . A  $t$ -statistic is in parentheses below each coefficient estimate. Long-run effect is this short-run effect  $\div 1 - (\text{sum of coefficients on two lagged dependent variables in equation})$ .

TABLE A2.11 *Medium-Run Elasticities of Labor Supply with Respect to Wages and Unemployment*

	Female Heads	Other Less- Educated Females	More- Educated Females	Less- Educated Males	More- Educated Males	All Five Groups
Wage elasticities after five years						
Labor force participation rate	0.098 (1.38)	-0.025 (0.51)	-0.057 (1.89)	0.030 (1.64)	0.024 (1.58)	0.021 (1.05)
Population	-0.140 (1.06)	-0.039 (0.45)	0.236 (2.24)	0.012 (0.15)	0.397 (3.29)	0.048 (0.98)
Total labor force	-0.042 (0.28)	-0.064 (0.66)	0.178 (1.63)	0.042 (0.49)	0.421 (3.44)	0.069 (1.33)
(- 1) times unemployment elasticities after five years						
Labor force participation rate	0.298 (3.18)	0.661 (5.15)	0.149 (1.07)	0.374 (8.02)	0.173 (2.04)	0.492 (8.92)
Population	0.121 (0.66)	0.262 (1.17)	0.447 (0.89)	0.508 (2.60)	2.769 (3.83)	0.632 (4.25)
Total labor force	0.420 (1.98)	0.923 (3.60)	0.596 (1.14)	0.882 (4.33)	2.943 (4.05)	1.124 (6.93)

Source: Author's compilation.

Notes: Estimates are derived from 1,000 Monte Carlo repetitions of two different simulations: one where all  $\ln(\text{wages})$  are increased by 0.10, the other where all unemployment rates are reduced by 0.01. Each simulation allows the labor force participation and population equation for that group only to become operational. Elasticities are change in natural logarithm of the three labor force variables divided by change in the wage or unemployment rate variable, and, for unemployment, multiplied by (-1). Absolute values of pseudo  $t$ -statistics are in parentheses below elasticity estimates and are equal to the absolute value of the mean elasticity from 1,000 repetitions divided by the standard deviation of elasticity in 1,000 repetitions. Estimates are elasticities after five years; elasticities slowly change only after five years. For example, the elasticity of the total overall labor force after ten years with respect to wages is 0.039 ( $0.52 = t$ ), from 0.069 ( $t = 1.33$ ) after five years. The elasticity of the overall total labor force after ten years with respect to unemployment is 1.408 ( $t = 6.15$ ), compared to 1.124 ( $t = 6.93$ ) after five years.

TABLE A2.12 *Simulations, Based on an Empirically Estimated Dynamic Model, of Elasticities After Five Years of Wages and Employment of Various Less-Educated Groups in Response to a Supply or Demand Policy Targeting That Group*

Group	Employment and Wage Elasticities for Group from Policy Targeting That Group <sup>a</sup>							
	Quantity Increase in Labor Demand		Quantity Increase in Labor Supply		Demand-Side Wage Subsidy to Employer		Supply-Side Wage Subsidy to Worker	
	Employment	Wage	Employment	Wage	Employment	Wage	Employment	Wage
Less-educated female heads of household	0.65 (3.21)	0.14 (4.89)	0.42 (1.75)	-0.11 (-5.61)	0.16 (1.57)	0.04 (3.60)	0.04 (0.99)	-0.01 (-1.32)
Other less-educated females	0.58 (7.84)	0.94 (11.97)	0.69 (10.56)	-0.67 (-11.72)	0.40 (11.12)	0.43 (6.15)	-0.01 (-0.46)	0.01 (0.27)
Less-educated males	0.64 (7.28)	1.27 (12.44)	0.66 (8.04)	-0.91 (-5.67)	0.47 (10.93)	0.61 (5.88)	0.01 (1.14)	-0.02 (-1.07)

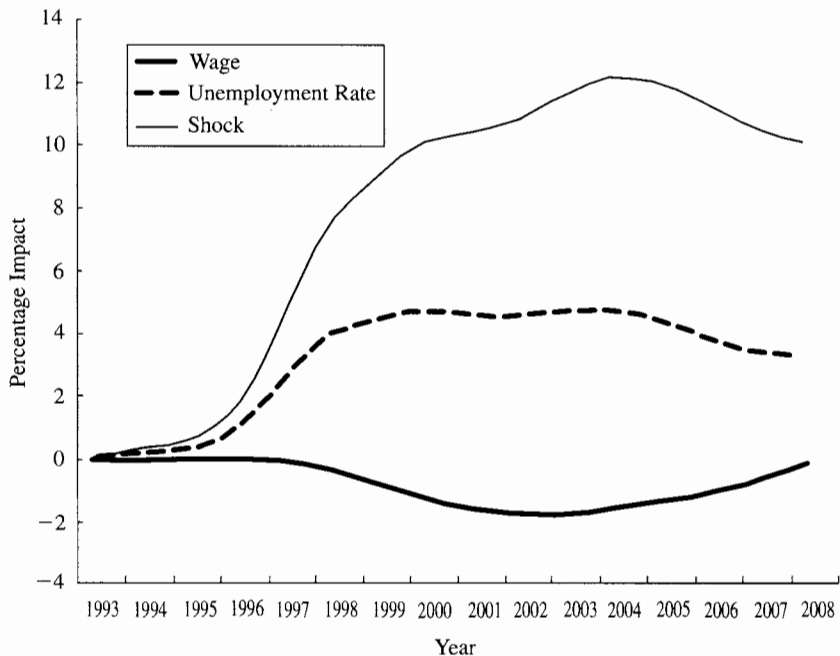
Source: Author's estimates.

Notes: See appendix 2 for the model and equations used to produce these numbers. Elasticities are calculated as change in  $\ln(\text{employment})$  or  $\ln(\text{market wage})$  of group, divided by the effect that the policy initially has on the natural log of the appropriate variable for the policy for that group. The divisor for elasticities is the initial shock due to policy to  $\ln(\text{employment of group})$  for quantity increase in labor demand, initial shock due to policy to  $\ln(\text{wage paid by employers for group})$  for wage subsidy to employers, initial shock due to policy to  $\ln(\text{labor force of group})$  for quantity increase in labor supply, and initial shock due to policy to  $\ln(\text{wage received by workers in group})$  for wage subsidy to workers. Estimated elasticities are effects after five years of running models for each of these twelve shocks (four different types of policies, each of three groups). Population-migration equations in the model are suppressed to approximate the "national" effects of implementing the policy in all states; the implicit assumption is that if all states pursue the same policy, there would be no incentives for in- or out-migration. Pseudo *t*-statistics in parentheses are derived from 1,000 Monte Carlo repetitions of simulation, with each repetition randomly choosing a set of parameter estimates from the estimated distribution of the model's parameter estimates. Pseudo *t*-statistics are ratio of mean results from 1,000 Monte Carlo repetitions to standard deviation of results in these 1,000 repetitions. Estimates are based on initially low-unemployment rate scenario, where overall unemployment situation for 1997 is used, with overall unemployment of 5 percent, and unemployment rate for various groups as given in table A2.2.

<sup>a</sup> Percentage change in employment or wages of each group for a 1 percent increase in labor demand or supply for that group, or a 1 percent wage subsidy to workers in that group or to employers of that group.

FIGURE A3.1 *Percentage Impacts of Welfare Reform on Wage Rate and Unemployment Rate of Female Heads*

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*Source:* Estimates derived by simulating model of appendix 2 and Bartik (1999) for welfare reform shock, as described in Bartik (2000a).

*Notes:* Effects on logarithmic variables are multiplied by 100 to get effects in percentage terms. For comparison, figure also reports labor supply shock on females heads as percentage of baseline labor force of female heads. “Female heads” are female heads of household, with other relatives present, with less than a college degree.

TABLE A3.1 *Displacement Effects of Welfare Reform*

Group	2000	2004	2008
Female heads	0.29 (2.69)	0.26 (2.41)	0.21 (2.03)
Other women with less than a college degree	0.31 (6.89)	0.01 (0.10)	-0.34 (2.96)
Women with college education	0.03 (1.49)	-0.10 (2.96)	-0.26 (4.73)
Men with less than college education	0.21 (3.95)	-0.14 (1.64)	-0.50 (3.96)
Men with college education	0.05 (2.46)	-0.08 (2.35)	-0.23 (4.32)
Overall average	0.89 (18.14)	-0.06 (0.32)	-1.10 (3.43)

*Source:* Estimates derived by using the model presented in Bartik (1999) and appendix 2 to simulate the effects of welfare reform, as described by Bartik (2000a).

*Notes:* Displacement is defined in this table as:  $[(-1) \times \text{change in employment of original members of group}] / (\text{total change in employment of those entering labor force due to welfare reform})$ . That is, the denominator is the same for all five groups, and the sum of the displacement numbers for each group is equal to overall average displacement. Numbers in parentheses are absolute values of pseudo *t*-statistics for testing whether estimated displacement is zero, and are derived by dividing the estimates by the standard deviations from 1,000 Monte Carlo simulations of model.

TABLE A4.1 *Effects of Education on Earnings of Different Groups*

	Low Unemployment Rate, Effects After Five Years	Low Unemployment Rate, Effects After Ten Years	High Unemployment Rate, Effects After Five Years	High Unemployment Rate, Effects After Ten Years
<b>Female heads</b>				
Ratio: (earnings effects on group left)/(earnings gains for group educated)	0.596 (2.28)	0.615 (2.35)	0.550 (2.30)	0.557 (2.33)
Ratio: (earnings effects on group joined)/(earnings gains for group educated)	-1.555 (3.30)	-1.445 (3.09)	-1.486 (3.31)	-1.445 (3.21)
Ratio: (total earnings effects for population)/(earnings gains for group educated)	-0.280 (0.91)	0.635 (1.90)	-0.051 (0.27)	0.259 (1.38)
<b>Other less-educated females</b>				
Ratio: (earnings effects on group left)/(earnings gains for group educated)	0.113 (0.98)	0.481 (3.19)	0.133 (1.42)	0.265 (2.62)
Ratio: (earnings effects on group joined)/(earnings gains for group educated)	-1.443 (3.31)	-1.275 (2.95)	-1.355 (3.27)	-1.295 (3.13)
Ratio: (total earnings effects for population)/(earnings gains for group educated)	-0.438 (1.53)	1.001 (2.37)	-0.072 (0.43)	0.413 (1.96)
<b>Less-educated males</b>				
Ratio: (earnings effects on group left)/(earnings gains for group educated)	0.420 (3.02)	0.673 (4.57)	0.424 (3.46)	0.496 (4.16)
Ratio: (earnings effects on group joined)/(earnings gains for group educated)	-1.320 (4.55)	-1.165 (4.17)	-1.224 (4.54)	-1.163 (4.44)
Ratio: (total earnings effects for population)/(earnings gains for group educated)	-0.007 (0.04)	0.668 (3.07)	0.139 (0.99)	0.365 (2.58)

Source: Author's estimates.

Notes: These results are estimated from the five-group model described in appendix 2 and Bartik (1999). The table presents ratios of the effects on "earnings" (dollar effects on earnings, defined as the change in the product of wage times employment rate) of different groups to effects on earnings of individuals who are "educated" by leaving a low-education group and joining a higher-education group. Absolute values of pseudo *t*-statistics are in parentheses and are derived from 1,000 Monte Carlo repetitions of simulations. The earnings effects analyzed are for the low-education group that loses some population, the high-education group that gains some population, and for everyone. The earnings effects calculated for the group receiving education assume that this group before and after being switched from one population to another has wages and an employment rate similar to whatever group it is in at the moment. Three different education switches are considered: switching some female heads to the college-educated female group; switching some less-educated females to the more-educated female group; switching some less-educated men to the more-educated male group. Simulations are all for switch of one-tenth of 1 percent of overall population. Generally, the group that is educated approximately doubles its average earnings, mostly owing to gaining wages from switching to college-educated group. Initial unemployment rates that are low and high correspond to 5.0 percent and 9.9 percent overall unemployment, the lowest and highest unemployment rates in the 1979 to 1997 sample period for which this model is estimated. See Bartik (1999) and appendix 2 for more details.

TABLE A6.1 *Effects of Education on Employment of Low-Education Groups*

Group	Employment Effects per Person Educated of Educating Single Mothers	Employment Effects per Person Educated of Educating Other Women	Employment Effects per Person Educated of Educating Men
Employment effects on group being educated	0.165 (109.4)	0.192 (129.6)	0.139 (187.2)
Employment effects on group "left be- hind"	0.362 (2.40)	0.320 (3.61)	0.405 (5.02)
Sum of employment effects on all less- educated groups (including those who gain educa- tion)	0.709 (3.88)	0.817 (4.47)	0.643 (6.08)
Sum of employment effects on entire labor force (in- cluding those who are more educated)	0.305 (2.10)	0.507 (3.47)	0.300 (3.74)

Source: Author's estimates.

Notes: Employment effects stated are *ratio* of net increase in jobs for indicated group or groups to number of people who gain education. The columns consider the effects of providing a college education to some number of individuals from three groups: single mothers ages sixteen to forty-four with less than a college education (column 1); other women ages sixteen to sixty-four with less than a college education (column 2); men with less than a college education (column 3). Rows consider effect on: employment of group that is educated (row 1); employment of those who do *not* receive a college education in the group that is educated (row 2); total employment of all those who originally, before the education program, had less than a college education (row 3); total employment of everyone (row 4). Estimates are derived from three simulations, in each of which 0.1 percent of the population in some less-educated group is switched to a more-educated group. Simulation assumes that initially unemployment is low, at 5.0 percent (1997 actual level). The estimated employment effects are effects ten years after the education "shock" occurs. Numbers in parentheses are pseudo *t*-statistics on estimates, derived from 1,000 Monte Carlo repetitions of simulation.



TABLE A7.1 *Long-Run Displacement Effects of Increase in Labor Demand for Less-Educated Males Under Various Assumptions*

Scenario	Displacement of Other Less-Educated Males (Pseudo <i>t</i> -Statistic for Displacement Estimate)	Overall Displacement Rate (Pseudo <i>t</i> -Statistic for Displacement Estimate)
Baseline scenario: Low unemployment rate, jobs created are permanent and pay market wages, hiring is done from unemployed less-educated males	0.974 (7.07)	1.545 (4.74)
High unemployment scenario: Baseline except high unemployment	0.484 (4.15)	0.386 (1.40)
Public-service job scenario with continued availability of PSE workers for regular jobs: Baseline except jobs created are assumed to not reduce supply of labor to regular jobs	0.526 (3.70)	1.616 (4.77)
Targeting nonparticipants scenario: Baseline, except jobs created go to less-educated males who otherwise would not be in the labor force	-0.128 (-1.44)	-0.295 (-1.36)

*Source:* Author's estimates.

*Notes:* All estimates are taken from simulations of the model described in appendix 2 and in Bartik (1999). Pseudo *t*-statistics are derived as ratios of average effects to standard deviations of effects from 1,000 Monte Carlo simulations of model. These long-run effects are the effects occurring ten years after the shock to labor demand. Less-educated males are males with less than a college degree. Estimates are all derived from a shock to labor demand of less-educated males of 0.001 of overall population. Displacement rate of less-educated males is loss of employment to less-educated males not directly hired by the shock, divided by the number of jobs created; if this number is negative, it indicates a gain in employment. Overall displacement is loss of employment to all in population other than those directly hired by the policy, divided by the number of jobs created. Low unemployment is the unemployment rate that prevailed in 1997 (5.0 percent overall). High unemployment is unemployment rate that prevailed in 1982 (9.9 percent overall). Baseline scenario assumes that those directly hired by the policy effectively decrease available labor supply to regular employers. The third scenario (row 3) assumes that created jobs do not reduce available labor supply to regular employers, perhaps by design features of the policy that encourage transition to regular jobs. The scenario in row 4 assumes that for each job created, labor supply of less-educated males increases by one person.

TABLE A8.1 *Descriptive Statistics for Key Variables for Different Groups Used in Estimation*

Group	Number of Individuals in Sample (Number of Panel Observations on Person-Year Cells)	Mean Annual Real Earnings	Mean Annual Work Hours Five Years Before	Mean Wage Five Years Before, Real Dollars per Hour [Twenty-Fifth Percentile, Seventy-Fifth Percentile]
Female high school dropouts	407 (6,732)	\$6,752	834	6.93 [5.24, 9.51]
Female high school graduates	665 (10,940)	13,523	1,215	9.50 [6.82, 12.83]
Female college graduates	177 (2,895)	23,986	1,310	16.14 [10.81, 21.26]
Male high school dropouts	264 (4,332)	25,574	1,789	12.77 [8.73, 18.15]
Male high school graduates	364 (5,967)	38,548	2,071	17.17 [12.64, 22.10]
Male college graduates	217 (3,530)	63,994	2,228	21.69 [16.11, 29.85]

*Source:* Author's estimates.

*Notes:* Real wages are calculated as annual real earnings / annual real hours. All real earnings and real wage figures are in 1995 dollars.

TABLE A8.2 *Parameter Estimates from Two Models of Earnings, for Six Demographic Groups (Dependent Variable: ln(Real Annual Earnings))*

(a) Models Without Individual Fixed Effects						
Independent Variable	Female			Male		
	High School Dropouts	High School Graduates	College Graduates	High School Dropouts	High School Graduates	College Graduates
Five years before: dummy for whether worked during year	-4,218 (511)	-8,079 (609)	-14,857 (1887)	-14,825 (1905)	-33,171 (2979)	-67,673 (12840)
Five years before: annual work hours	2.491 (0.179)	3.641 (0.186)	4.593 (0.533)	2.848 (0.389)	5.010 (0.456)	6.004 (1.678)
Five years before: ln(average real wage = annual real earnings/annual work hours)	2.621 (246)	4,598 (261)	6,686 (663)	5,881 (629)	10,607 (775)	20,468 (2487)
F-test (probability) for three five-years-before variables	140.7783 (0.0001)	307.8882 (0.0001)	58.5700 (0.0001)	35.5826 (0.0001)	72.4473 (0.0001)	22.7189 (0.0001)

(b) Models with Individual Fixed Effects						
Independent Variable	Female			Male		
	High School Dropouts	High School Graduates	College Graduates	High School Dropouts	High School Graduates	College Graduates
Five years before: dummy for whether worked during year	-865 (415)	-2,460 (496)	-3,354 (1601)	1,038 (1842)	-4,898 (2571)	-18,402 (12210)
Five years before: annual work hours	0.627 (0.148)	1,954 (0.153)	0.905 (0.453)	0.263 (0.351)	2.129 (0.406)	1.524 (1.760)
Five years before: ln(average real wage = annual real earnings/annual work hours)	634 (197)	1,433 (209)	1,725 (567)	639 (569)	1,828 (694)	8,559 (2568)
F-test (probability) for three five-years-before variables	14.7935 (0.0001)	101.3081 (0.0001)	5.3637 (0.0011)	3.197 (0.0225)	10.6008 (0.0001)	3.8943 (0.0086)

Source: Author's estimates.

Notes: Standard errors are in parentheses below coefficient estimates. All models also include year dummies and state dummies; dummy variables for whether married, whether have kids, whether black, whether Hispanic; continuous variables for "experience" and "experience squared," years of education, and average real earnings from years 6 through 8. Coefficients and standard errors for these variables are not reported to save space and focus on variables of interest. Panel b models also include fixed effects for each individual. Each panel is reporting results from six regressions, one for each of six groups. Dependent variable in each regression is annual real earnings (1995 dollars).

TABLE A8.3 *Estimated Real Earnings Effects Five Years Later of Working Full-Time at Various Wage Rates*

(a) Models Without Individual Fixed Effects

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Group	Twenty-Fifth Percentile	Mean Wage for That Group	Seventy-Fifth Percentile
Female high school dropouts	\$5,107 (269) [0.487]	\$5,839 (288) [0.421]	\$6,669 (326) [0.351]
Female high school graduates	8,031 (293) [0.589]	9,555 (320) [0.503]	10,937 (361) [0.426]
Female college graduates	10,246 (952) [0.474]	12,926 (1,050) [0.401]	14,768 (1,149) [0.347]
Male high school dropouts	3,614 (951) [0.207]	5,851 (1,014) [0.229]	7,919 (1,115) [0.218]
Male high school graduates	3,758 (1,902) [0.149]	7,007 (1,942) [0.204]	9,684 (1,997) [0.219]
Male college graduates	1,224 (9,700) [0.038]	7,312 (9,752) [0.169]	13,848 (9,869) [0.232]

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(b) Models with Individual Fixed Effects

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Group	Twenty-Fifth Percentile	Mean Wage for That Group	Seventy-Fifth Percentile
Female high school dropouts	\$1,440 (247) [0.137]	\$1,617 (257) [0.117]	\$1,818 (282) [0.096]
Female high school graduates	4,199 (268) [0.308]	4,674 (283) [0.246]	5,104 (311) [0.199]
Female college graduates	2,561 (871) [0.118]	3,253 (936) [0.101]	3,728 (1,010) [0.088]
Male high school dropouts	2,948 (1,053) [0.169]	3,191 (1,077) [0.125]	3,416 (1,139) [0.094]

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TABLE A8.3 *Continued*

(b) Models with Individual Fixed Effects			
Group	Twenty-Fifth Percentile	Mean Wage for That Group	Seventy-Fifth Percentile
Male high school graduates	3,999 (1,735) [0.158]	4,559 (1,775) [0.133]	5,020 (1,828) [0.114]
Male college graduates	8,435 (9,315) [0.262]	10,981 (9,406) [0.253]	13,714 (9,576) [0.230]

*Source:* Author's estimates.

*Notes:* Table reports effects on real earnings (in 1995 dollars) in a given year of working full-time five years before at various wage rates, compared to not working at all. The estimates are based on the models reported in table A8.2. The estimates are based on switching the values of three variables: the employment dummy (from zero to one), the annual hours worked variable (from zero to 2,000), and the ln(wage) variable (from one to whatever value is assumed). Standard errors are in parentheses and are based on estimated variance-covariance matrix for the estimates in table A8.2. Brackets show the estimated dollar effects on real earnings as a proportion of the initial shock to earnings. The various percentiles of wages used in the simulations are calculated separately for each of the six groups and are based on the wage distribution of each group; the actual wage numbers used are reported in table A8.1.

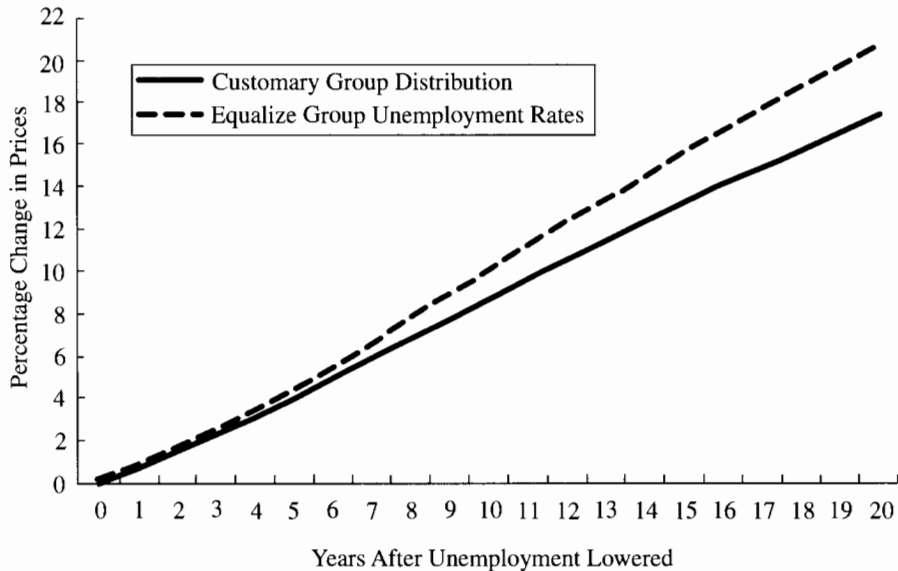
TABLE A8.4 *Elasticities of Current Real Earnings with Respect to Wage of Job Taken Five Years Before*

Group	Nonfixed Effect Estimates	Fixed Effect Estimates
Female high school dropouts	0.39	0.09
Female high school graduates	0.34	0.11
Female college graduates	0.28	0.07
Male high school dropouts	0.23	0.02
Male high school graduates	0.28	0.05
Male college graduates	0.32	0.13

*Source:* Author's estimates.

*Notes:* These elasticities are derived by dividing the coefficient estimates on five-year lagged wages, from table A8.2, by the mean value of current real earnings for that group, reported in table A8.1.

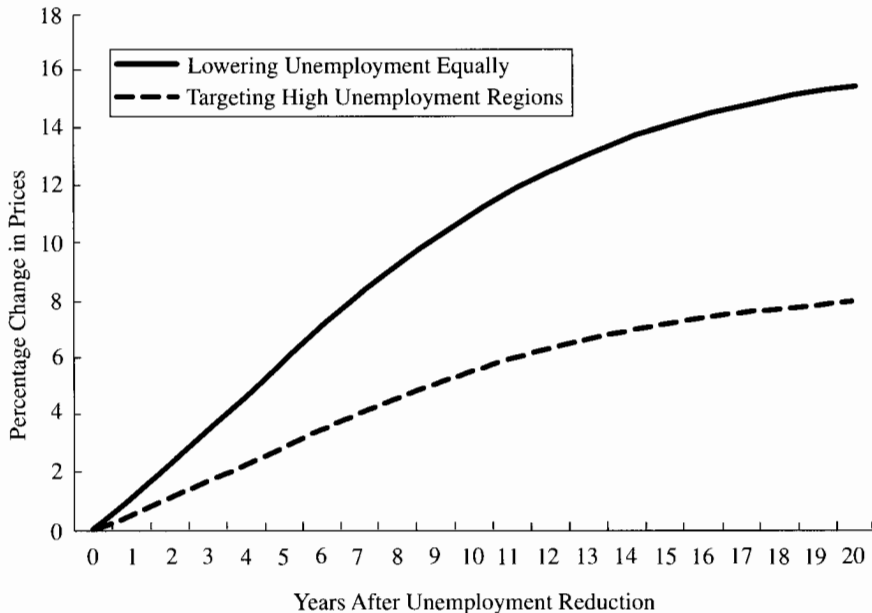
FIGURE A9.1 *Price Effects of Lowering Overall Unemployment from 4.5 to 3.0 Percent, with Two Different Ways of Distributing Unemployment Reduction: Lowering Unemployment Among Both College Graduates and Nongraduates According to Customary Patterns, and Equalizing Both Groups at 3.0 Percent*



Source: Author's estimates.

Note: Both simulations start out with a college graduate unemployment rate of 2.1 percent, and a nongraduate unemployment rate of 5.4 percent. Simulation using customary distribution lowers graduate unemployment rate to 1.5 percent, and the nongraduate unemployment rate to 3.6 percent. Other simulation equalizes both groups' unemployment rates at 3 percent.

FIGURE A9.2 *Price Effects of Lowering Unemployment from 4.5 to 3.0 Percent, Under Two Different Methods of Distributing Unemployment Reduction Among Regions: Lowering Unemployment Equally Everywhere, and Focusing Unemployment Reduction in High-Unemployment Regions*



Source: Author's estimates.

Notes: In both simulations, the economy is initially divided into two equal-sized regions, one with unemployment of 6 percent, the other with unemployment of 3 percent. In one simulation, overall unemployment is lowered to 3.0 percent by lowering unemployment in both regions by 1.5 percent. In other simulation, overall unemployment is lowered to 3 percent by lowering unemployment in high-unemployment regions only.



TABLE A9.1 *Descriptive Statistics on Local Unemployment and Wage Data for Twenty-Five Metropolitan Areas, 1979 to 1998*

(a) Means and Standard Deviations			
	Proportion of Labor Force, Average Across All Years and MSAs	Mean of MSA Unemployment Rates, All Years and MSAs	Mean of MSA ln(Adjusted Real Wage) (1979 Dollars)
Overall	1.000	6.4 (2.2)	1.639 (.064)
Noncollege graduate	0.723	7.6 (2.5)	1.555 (.067)
College graduate	0.277	2.8 (1.2)	1.943 (.080)

*Notes:* The means here are “means” of means; that is, they are unweighted means and standard deviations across observations on MSA-year means for twenty-five MSAs and all years from 1979 to 1998. The underlying data are calculated from the CPS-ORG, as described in the text. The unadjusted means for unemployment for each MSA-year are calculated using CPS sampling weights. The regression-adjusted means for ln(wage rate) are based on preliminary regression of ln(real wage) on a variety of wage predictors. Standard deviations are in parentheses.

(b) 1998 MSA Means for Overall Unemployment Rate	
MSA	1998 Unemployment Rate
Atlanta	2.9
Baltimore	5.3
Boston	2.6
Buffalo	2.9
Chicago	5.0
Cincinnati	3.8
Cleveland	3.1
Dallas	3.8
Denver	3.1
Detroit	4.2
Houston	4.2
Kansas City	3.4
Los Angeles	6.8
Miami	7.0
Milwaukee	3.4
Minneapolis-St. Paul	1.8
New York	6.2
Philadelphia	5.2
Pittsburgh	4.2
Portland (Oregon)	4.8
St. Louis	5.0
San Diego	5.5
San Francisco	3.9

(Appendix continues on p. 366.)

TABLE A9.1      *Continued*

(b) 1998 MSA Means for Overall Unemployment Rate

MSA	1998 Unemployment Rate
Seattle	2.8
Washington, D.C.	3.2

*Source:* Author's estimates.

*Note:* The official national unemployment rate in 1998 was 4.5 percent. The simple mean across these twenty-five MSAs in 1998 was 4.2 percent.

TABLE A9.2 *Selected Coefficient Estimates from Wage and Price Equation Models*

	Mean ln(Wage) of Persons with Less Than College Education in That MSA and Year	Mean ln(Wage) of College Graduates in That MSA and Year	ln(Average Consumer Prices) in That MSA and Year
Functional form of unem- ployment on right-hand side	ln(UR)	UR squared	1/UR
Coefficient on:			
Lagged national variable	—	0.382 (2.82)	0.121 (3.11)
Current unemployment rate, group	-0.0278 (-1.44)	—	—
Lag unemployment rate, group	-0.0058 (-0.29)	2.464 (0.96)	—
Current local unemploy- ment rate	0.0107 (0.53)	—	—
Lag local unemployment rate	-0.0235 (-1.11)	-3.492 (-4.30)	0.000472 (3.91)
Lag ln(wage group)	0.229 (2.38)	0.188 (3.14)	—
Second lag ln(wage group)	0.237 (2.46)	-0.202 (-3.41)	—
Current overall wage	—	—	0.0644 (4.31)
Lag ln(overall wage)	0.449 (4.30)	0.305 (2.76)	—
Second lag ln(overall wage)	-0.129 (-1.20)	0.233 (2.13)	—
Lag 1 ln(price)	0.371 (3.51)	0.037 (0.20)	0.978 (22.11)
Lag 2 ln(price)	-0.092 (-0.61)	0.322 (1.30)	-0.246 (-3.99)
Lag 3 ln(price)	-0.061 (-0.41)	-0.089 (-0.37)	0.074 (1.19)
Lag 4 ln(price)	-0.028 (-0.19)	0.133 (0.57)	0.082 (1.37)
Lag 5 ln(price)	-0.064 (-0.75)	-0.262 (-1.73)	-0.125 (-3.22)
F-test probability:			
All unemployment rate variables	0.0001	0.0001	0.0001
Unemployment rate for group	0.3468	0.3381	—
Unemployment rate overall	0.4793	0.0001	0.0001

(Appendix continues on p. 368.)

TABLE A9.2 *Continued*

	Mean ln(Wage) of Persons with Less Than College Education in That MSA and Year	Mean ln(Wage) of College Graduates in That MSA and Year	ln(Average Consumer Prices) in That MSA and Year
Wage for group	0.0002	0.0002	—
Wage overall	0.0001	0.0001	0.0001
Prices	0.0002	0.0712	0.0001

*Source:* Author's estimates.

*Notes:* Observations are on MSA-year cell means for labor market variables. Each column corresponds to one of the three equations estimated. The dependent variable for the equation is listed at the top. The second row lists the functional form for unemployment used on the right-hand side of that equation. Unemployment rate variables are defined as proportions, not percentages. Subsequent rows list various right-hand-side variables and give the estimated coefficients on these variables in each equation, with *t*-statistics in parentheses. National variable included is lagged group wage for college graduate wage equation, lagged national price for price equation. All equations also include dummies for each MSA and random effects for each year. Last six rows list relevant F-test probabilities in each equation for variables of a particular type.

TABLE A9.3 *Estimated Effects on Prices and Real Wages After Ten Years of Two Different Patterns of Lowering Unemployment Among Different Groups*

Variable	Customary Pattern of Lowering Group Unemployment Rates (College Grads to 1.5 percent, Non-Grads to 3.6 percent)	Equalizing Group Unemployment Rates at 3 percent
ln(price)	0.0868 (4.62)	0.0999 (4.04)
ln(overall real wage)	0.0606 (2.36)	0.0867 (2.15)
ln(real wage non-graduates)	0.0641 (2.39)	0.0915 (2.12)
ln(real wage college graduates)	0.0517 (1.52)	0.0746 (1.57)

*Source:* Author's estimates.

*Notes:* Initial unemployment rates: college graduates, 2.1 percent, nongraduates, 5.4 percent, overall unemployment, 4.5 percent. Both simulations lower overall unemployment from 4.5 to 3 percent. Pseudo *t*-statistics are in parentheses, from 1,000 Monte Carlo repetitions of simulation.

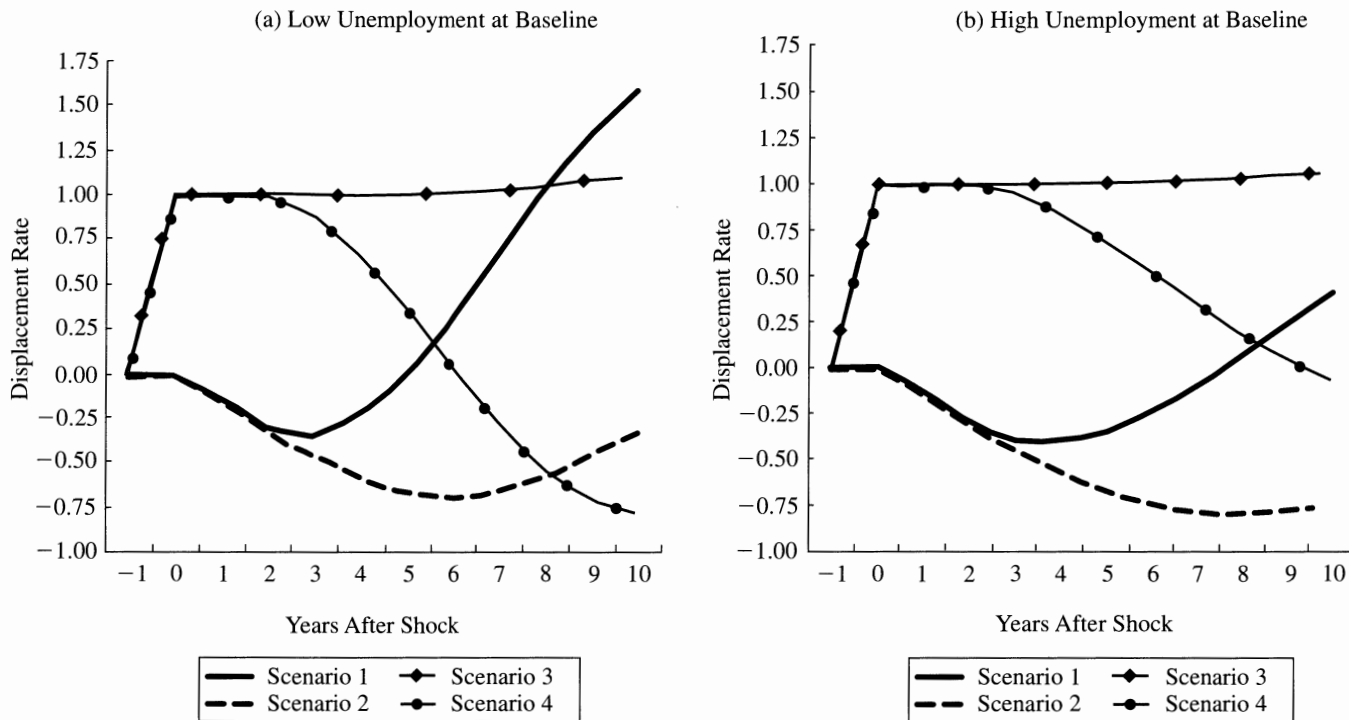
TABLE A9.4 *Estimated Effects on Prices After Ten Years of Two Different Patterns of Lowering Unemployment Across Geographic Areas*

Variable	Lowering Unemployment in Both Regions by 1.5 Percent	Lowering Unemployment in High-Unemployment Region to 3 Percent
ln(National prices)	0.1114 (4.40)	0.0563 (4.83)
ln(Prices in high-unemployment region)	0.0709 (3.38)	0.0897 (5.70)
ln(Prices in low-unemployment region)	0.1519 (4.77)	0.0230 (2.33)

*Source:* Author's estimates.

*Notes:* Initial unemployment rates: high-unemployment region, 6 percent; low-unemployment region, 3 percent; overall national unemployment 4.5 percent. Both simulations lower overall unemployment from 4.5 to 3 percent. Pseudo *t*-statistics are in parentheses, from 1,000 Monte Carlo repetitions of simulations.

FIGURE A10.1 *Displacement Effects of Publicly Funded Jobs for Single Mothers Under Four Scenarios*



Source: Author's estimates.

Notes: The figure is derived from simulating the model of appendix 2 under four scenarios times two different baselines for overall unemployment, a low-unemployment baseline and a high-unemployment baseline. Each scenario estimates the effects of policy (publicly funded jobs) that increases labor demand for female heads of household with less than a college degree. In each scenario, what is estimated is the displacement rate: how much employment of all others (except those employed directly in these jobs) is affected by policy. Four scenarios are described in table A10.1, which also gives actual numbers for displacement effects five years after the policy is implemented.

TABLE A10.1 *Displacement Effects After Five Years of Publicly-Funded Jobs for Single Mothers Under Four Scenarios*

Demand Shock Scenario	Displacement Rate for Overall Employment, Low Baseline Unemployment Rate	Displacement Rate for Overall Employment, High Baseline Unemployment Rate
Scenario 1: increase in labor demand for female heads	- 0.012 (- 0.06)	- 0.349 (- 2.61)
Scenario 2: increase in labor demand for female heads, targeted at female heads who otherwise would be out of the labor force	- 0.652 (- 5.32)	- 0.675 (- 5.32)
Scenario 3: increase in labor demand for female heads, offset by equal-sized reduction in labor demand for other less-educated females	1.008 (64.01)	1.007 (114.07)
Scenario 4: increase in labor demand for female heads, offset by equal-sized reduction in labor demand for other less-educated females, but increased demand for female heads is targeted at persons who otherwise would be out of the labor force	0.383 (2.58)	0.686 (8.97)

Source: Author's estimates.

Notes: Pseudo *t*-statistics on estimated overall displacement rates are in parentheses, derived from 1,000 Monte Carlo repetitions of simulations. Reported displacement rates are equal to 1 - (actual change in overall employment in five years due to scenario / initial change in employment of female heads brought about by demand shock). A displacement rate of one implies that overall employment did not go up at all owing to the demand shock, and hence that each job gained by a female head owing to the demand shock is offset completely by a job lost to someone else. A displacement rate of zero implies that the actual change in overall employment is equal to the initial demand shock to female heads, so that there is no net gain or loss to others from the initial demand shock to female heads. A negative displacement rate implies that there is actually a net gain to others from the initial demand shock to female heads. To generate the scenarios, the following assumptions are made: in scenario 1, added demand for female heads reduces effective supply to private demanders; in scenario 2, the added demand also generates an added supply of female heads to the overall labor market; in scenario 3, two demand shocks occur, a positive demand shock to female heads and a negative demand shock of equal size to other less-educated females, with corresponding changes in the effective supply of each type of labor to private demanders of labor; in scenario 4, in addition to the two demand shocks, there is a positive supply shock for female heads.



TABLE A11.1 *Description and History of Large-Scale National Wage Subsidy Programs*

Program	Years	Description
OJT component of MDTA (Manpower Development and Training Act) <sup>a</sup>	1962 to 1971	Training contracts entered into with employers to hire the unemployed or upgrade the underemployed, with wages paid by employers and training costs paid by the government. Generally administered by the Department of Labor's Bureau of Apprenticeship, which was not enthusiastic about the program given its ties to established union apprenticeship programs. Grew rapidly to 1967. Eventually most OJT was ceded to the JOBS program in 1968, and OJT components of both programs were formally merged in 1971.
JOBS (Job Opportunities in the Business Sector) <sup>b</sup>	1968 to 1973	Sponsored by federal government in cooperation with National Alliance of Business; targeted at young and less-educated workers from poverty backgrounds; most were black males; federal government provided contracts to private business with up to \$12,600 per placement (1998 dollars). Grew rapidly from 1968 to 1971, then declined.
WIN tax credit program <sup>c</sup>	1972 to 1981	Tax credit for hiring welfare recipients participating in WIN welfare-to-work program; initially provided 20 percent wage credit, with maximum credit of \$3,900 (1998 dollars) per hire, and credit cut in half for firm whose annual credit exceeded

TABLE A11.1 *Continued*

Program	Years	Description
OJT component of CETA (Comprehensive Employment and Training Act) <sup>d</sup>	1974 to 1982	<p>\$98,000. Initially only for hires placed by WIN offices, the program was expanded in 1973 to include WIN hires who found jobs by themselves. Initially required two-year retention, which was reduced to ninety days in 1975. The credit amounts were liberalized in 1978 to be the same as those adopted for TJTC in that year. Folded into TJTC in 1981.</p> <p>Training contracts with employers, ostensibly to reimburse for extra training and supervision costs associated with high-risk hires. Typically about 12 percent of CETA training participants received OJT (Levitan and Gallo 1988). Decline in OJT in latter years of MDTA, early years of CETA, before increasing until 1978 and then declining with rest of program.</p>
NJTC (New Jobs Tax Credit) <sup>e</sup>	1977 to 1978	<p>Tax credit of 50 percent for the first \$10,500 (1998 dollars) of wages per employee for increases in employment of more than 2 percent over previous year. Total credit could not be claimed for more than about forty-seven employees per firms, so there were no marginal incentives for large growing firms. Replaced by TJTC.</p>
TJTC (Targeted Jobs Tax Credit) <sup>f</sup>	1978 to 1994	<p>Tax credit of various sizes over the years offered for hiring various target groups. Originally offered tax credit of 50 percent of first-year wages and 25 percent of second-year wages, up to wage of \$15,000 annually (1998 dollars). By 1994, credit had been scaled back to 40 percent of first-year wages, up to wage of \$6,600 annually (1998 dollars). Among target groups usually included under the program were economically disadvantaged youth, veterans, and ex-offenders; disabled individuals undergoing vocational rehabilitation; and recipients of various forms of welfare assistance. Youth in cooperative education programs were originally included but then cut out in 1981. Generally more than half of those certified have been disadvantaged youth.</p>

(Appendix continues on p. 378.)

TABLE A11.1 *Continued*

Program	Years	Description
OJT component of JTPA (Job Training Partnership Act)*	1983 to 1999	<p>Originally employers could request retroactive certification of hires, but over the years the credit was amended so that certification had to be requested prior to first date of employment and employer was supposed to make good-faith effort to determine eligibility prior to hiring. Also, over the years, requirements were added that credit was paid only for employees who lasted for at least 120 hours of work. About 70 percent of employers participating in TJTC have job hires screened by consultants who handle paperwork, usually screening after hiring decision is made. Characteristics of TJTC jobs (U.S. Department of Labor 1994): occupations: 39 percent clerical/sales, 38 percent service; industries: 66 percent wholesale/retail. Thirty-seven percent of jobs pay minimum wage, 65 percent no fringes, 61 percent part-time. Seventy-six percent no longer with same firm five quarters after hire. From U.S. GAO (1991): 59 percent retail store and restaurants; 75 percent service or clerical/sales occupations; 75 percent of jobs pay within \$1 of minimum wage. Expired, replaced after lapse by WOTC.</p> <p>Under the Job Training Partnership Act, one service option, in addition to various job search and training options, was to provide on-the-job-training. This typically involved job developers from JTPA locating employers for JTPA clients and entering into contracts with them to provide OJT for six months, in exchange for a wage subsidy for 50 percent of wages. Employers entering into OJT contracts were generally expected to retain trainees at the end of the six months if their performance was satisfactory. Employers who were felt to be abusing the system to get wage subsidies could be excluded from future OJT placements. An important point is that these wage subsidies under OJT, unlike most of the tax-based wage subsidies, were discretionary in that there</p>

TABLE A11.1 *Continued*

Program	Years	Description
		<p>was no entitlement by any employer to receiving an OJT subsidy unless the local JTPA service provider voluntarily decided to enter into a contract with that provider. Over time, regulations were tightened to require more substantive training and more documentation by employers, causing OJT to decrease in importance. Initially OJT was larger under JTPA than under CETA. Over time, JTPA shrank in size, and OJT declined as a proportion of the program. For example, the percentage of OJT participants was 22 to 23 percent for most of the 1980s but went down to 15 percent by 1991 to 1992. Will probably be continued under WIA.</p>
WOTC (Work Opportunity Tax Credit) <sup>a</sup>	1997 to present	<p>Generally similar to TJTC, but with differences in potentially crucial design details. Slight changes from 1996 to 1998, but in most recent design, provides credit of 40 percent of wages up to a \$2,400 credit for individuals retained at least 400 work hours, 25 percent up to \$1,500 for individuals employed 125 to 400 hours. Good-faith effort is more specifically defined as employer completing a prescreening eligibility form on or before date job offer is made, basically stating that employer believes hire is eligible. Eligibility is also simplified by tying it to receipt of food stamps or welfare rather than family income. Eligible groups include welfare and SSI recipients; eighteen- to twenty-four-year-olds in families receiving food stamps or living in empowerment zones or enterprise communities; vocational rehabilitation referrals; veterans receiving food stamps; and ex-felons who are members of low-income families. Compared to TJTC, WOTC puts much more emphasis on welfare recipients: 55 percent welfare recipients in 1999 (Beverly 1999).</p>
WWTC (Welfare-to-Work Tax Credit) <sup>a</sup>	1998 to present	<p>Similar to WOTC, but much more generous, long-term, and targeted. Eligible group is</p>

(Appendix continues on p. 380.)

TABLE A11.1 *Continued*

Program	Years	Description
		welfare recipients who received welfare at least eighteen months. Credit is 35 percent of first-year wages, 50 percent of second-year wages, with maximum credit of \$3,500 of first year wages and \$5,000 of second year wages. Benefits can be counted in wage base. Has same requirements as WOTC for employer to do written eligibility screen on or before hiring date

*Source:* Author's estimates.

*Note:* This appendix presents more detailed program descriptions corresponding to table 8.1. It also gives the sources and methods used to derive the costs and size figures used in table 8.1.

<sup>a</sup> Perry et al. (1975, 152–53, table VII–I). Total cost figures are derived from costs per enrollee figures in Mangum (1968, 121) for fiscal year 1967.

<sup>b</sup> Hamermesh (1978) and Perry et al. (1975).

<sup>c</sup> Hamermesh (1978) and O'Neill (1982).

<sup>d</sup> OJT enrollment figures are derived from U.S. Department of Labor (1979, 32). Total cost per OJT participant is derived using Taggart (1981, 25, 46), a calculation that implies fiscal year 1978 costs per participant of \$3,180 (1998 dollars). This cost figure takes service year costs and adjusts them to per participant costs by average length of training and ratio of participants to slots.

<sup>e</sup> Katz (1998), Perloff and Wachter (1979), and Bishop (1981). In addition to what is noted in the table, the credit could be no larger than 50 percent of excess total wages over 105 percent of the previous year's wages, to limit the ability of firms to maximize the credit by getting rid of full-timers and replacing them with part-timers.

<sup>f</sup> Katz (1998), U.S. House of Representatives (1998), and Bishop and Montgomery (1993).

<sup>g</sup> Various issues of the *Training and Employment Report* of the U.S. Department of Labor, in particular the report for program year 1986 (U.S. Department of Labor 1990). Figures on costs are extra wage subsidy costs of OJT. This is derived from Orr et al. (1996). I take costs per OJT participant from the different groups and weight each group by the proportion of OJT participants. In addition, I looked at the costs of the OJT strategy versus other strategies. It is difficult to determine true relative OJT costs with associated services, since the OJT strategy in Orr et al. (1996) has only 30 percent of that group actually using OJT. However, in looking at the OJT strategy, the average cost of incremental training services across all four groups for this strategy appears to be \$1,235, whereas the average cost across all four groups of classroom training is \$1,195. OJT is expensive, but so is classroom training. So I assume that on average OJT is only slightly more expensive than average service, and hence might cost 30 percent of total JTPA spending on Title II-A.

<sup>h</sup> Participation figures come from U.S. Department of Labor, (2000). Budget figures for tax expenditures come from U.S. Office of Management and Budget (1998, 333). Note that some WOTC costs are delayed, and because program is being phased in, the ongoing costs of running a program at 1999 size would be higher than \$0.2 billion, perhaps between \$0.3 billion and \$0.4 billion based on OMB projections.

<sup>i</sup> as for WOTC; see note *h*.

TABLE A12.1 *Small-Scale, Local, and Experimental Wage Subsidy Programs for the Disadvantaged*

Program	Years and Places of Operation	Description	Magnitude of Subsidies	Scale of Program
Work supplementation/ grant diversion and other wage subsidies <sup>a</sup>	1967 to the present, various states. At present, thirty-two states have some sort of wage subsidy program. Some recent interesting programs are listed below separately.	Welfare department pays welfare grant, or sometimes welfare grant plus food stamps, as wage subsidy to private employer that hires welfare recipient for some period of time. This is a discretionary program in that the employer receives subsidy only if it enters into a contractual arrangement with the state welfare department.	Quite variable across programs, typically in range of \$1 per hour to \$5 per hour. Across thirteen states for which explicit wage subsidy can currently be calculated, median is \$2.50 per hour. Length of subsidy is typically a maximum of six or nine months. Of the twenty-two states that report this feature of their program, eleven have a maximum of six months, and seven have a maximum of nine months.	Although these programs have commonly been created in various waves of welfare reform, they usually are run at a very small scale. For example, under the JOBS program in 1994, sixteen states ran the Work Supplementation Program, but total participation across all states was only 1,011. <sup>b</sup>
State tax credits for hiring welfare recipients or other disadvantaged	Years unknown; currently fifteen states have such tax incentives. (Five offer only in enterprise zone areas.)	State provides credit against corporate income tax for hiring disadvantaged or welfare recipient. Such tax credits are an entitlement in that any firm that meets the requirements of the tax credit is entitled to it.	Quite variable, but typically the subsidy is less per hour of work but longer-lasting than for grant diversion. Across five states that calculate incentives as percentages, percentage is 20 to 30 percent of wages in first year. Credit is typically capped during the first year, with a median cap of \$1,500. Of ten states	There is little data on the take-up rate of these programs, but experience with TJTC and WOTC would suggest that the take-up rate is modest.

Supported Work <sup>c</sup>	Experimental program, 1975 to 1978.	Although in most cases SW workers were directly employed by SW agency in public-service or nonprofit-type jobs, some SW workers were employed either as subcontractors to private business or directly under the supervision of private business, with private business reimbursing for part of the costs.	with incentives, three have them for the first three years of employment, two for the first two years, and five for just the first year.	On average Supported Work covered 16 percent of its costs by income from selling goods or services, implying extremely large wage subsidy when contracting with private businesses.	Experiment was run on a small scale.
YIEPP	Experimental program conducted from 1978 to 1981 at seventeen sites.	Guaranteed part-time job during school year, and full-time job during summer, for disadvantaged youth ages sixteen to nineteen, living in eligible areas, who stayed in school or returned to school. The proportion of jobs in the private sector grew over time, reaching 23 percent by the last year of the experiment.	An essentially 100 percent wage subsidy was provided for the duration of a youth's eligibility, although some experiments during the program tested using 50 percent and 75 percent wage subsidies.	Operated at saturation level in some sites. Response to program suggests that a national program would have about 1.1 million participants and cost about \$3.6 billion annually (see table 7.5).	

*(Appendix continues on p. 384.)*

TABLE A12.1 *Continued*

Program	Years and Places of Operation	Description	Magnitude of Subsidies	Scale of Program
MEED <sup>d</sup>	Minnesota program conducted from 1983 to 1989.	<p>Provided wage subsidy for up to six months for employers hiring unemployed workers not receiving UI benefits for newly created jobs. Initially 60 percent of placements were in the public sector, but over time 75 percent were in the private sector. Private-sector employers were required to retain subsidized workers for at least one year after the subsidy expired; if not, they had to hire another subsidized worker or repay a portion of the subsidy. Workers and employers were recruited and selected by local JTPA agencies. Priority was placed on employers that were nonretail small businesses and businesses that exported outside Minnesota. Priority was placed on unemployed persons receiving public assistance.</p>	<p>Provided \$4 per hour in subsidy for wages and \$1 per hour in subsidy for benefits (equivalent to \$6.55 per hour for wages and \$1.64 per hour for benefits in 1998 dollars). Actual wage paid averaged slightly less than \$1 more than the subsidized wage, so the average wage subsidy exceeded 80 percent.</p>	<p>At peak, program spent \$50 million per year and subsidized about 9,000 new job slots and had 10,700 participants. A national program of the same scale relative to population would spend \$5.1 billion per year (1998 dollars), be associated with 0.591 million new job slots, and have 0.703 million participants.<sup>e</sup></p>



Maine TOPS<sup>f</sup>

Maine program conducted from 1983 to 1986.

Random assignment experiment in which individuals who had received welfare for at least six months, had volunteered for the program, were thought by program staff to be suitable for employment, and were randomly selected into the treatment group were offered sequenced "treatment" of (1) two to five weeks of prevocational training in job search and job-holding skills; (2) half-time work experience positions in the public and nonprofit sectors for up to twelve weeks; (3) OJT positions in private for-profit (73 percent) or public or nonprofit sector (27 percent) for up to six months.

OJT period provided wage subsidy of 50 percent to employers, partly financed by welfare grant diversion and partly by JTPA funds. MDRC evaluation comments that those hired seem to receive no more training than most workers receive, and seem as productive as the average new worker, so OJT subsidy is really a hiring subsidy, not compensation for extra costs. Average OJT subsidy per OJT participant was \$1,900 (1998 dollars). The total cost of the program per participant in any program component was \$4,481, \$626 of which was OJT wage subsidy costs (reduced from \$1,900 because less than half of those participating in the program used OJT).

The program was run as an experiment but ended up being smaller-scale than anticipated when it was set up, in part because the program was thought to be suitable for only a portion of the caseload, in part because of the complexity of delivering all the services in the prescribed sequence and recruiting employers. Only 30 percent of the treatment group members actually received the OJT wage subsidy.

*(Appendix continues on p. 386.)*

TABLE A12.1 *Continued*

Program	Years and Places of Operation	Description	Magnitude of Subsidies	Scale of Program
New Jersey Grant Diversion Program <sup>a</sup>	New Jersey program conducted from 1984 to 1987.	Random assignment experiment in which welfare recipients who were single parents, volunteered for OJT, and were believed to be suitable for OJT were randomly assigned either to a control group, which received normal welfare-to-work services, or to a treatment group, which also was eligible for OJT.	50 percent wage subsidy, for up to six months. Average subsidy lasted ten weeks. Forty-three percent of experimental sample actually received OJT subsidy. Costs per OJT participant (per experimental) of OJT wage subsidy itself was \$1,269 (\$518) in 1998 dollars. Administrative costs of OJT program per OJT participant (per experimental) were \$1,823 (\$744) in 1998 dollars. Administrative costs included the costs of hiring job developers to find jobs for OJT participants.	Very small-scale program run as an experiment. In the first year, there was an average eight-week waiting period for locating OJT positions; it declined in the second year to four weeks.
Oregon JOBS Plus Program <sup>b</sup>	Oregon program conducted from 1993 to the present.	First a demonstration and now a statewide program in which welfare recipients and UI recipients are placed with participating employers, which receive a wage subsidy for hiring them for up to six months.	Wage subsidy is \$6 per hour plus payroll taxes and workers' compensation costs. Firm is supposed to pay prevailing wage for the job; on average, firms pay a bit less than \$1 more, making the effective average	Program is operated on a modest scale. Seven percent of Oregonians engaged in welfare-to-work programs are in JOBS Plus. From July 1996 to October 1997, 3,500 persons left JOBS Plus. A similar program in per

wage subsidy over 80 percent. Wage subsidy is up to six months. After four months on the job, if employer has not hired the participant, employer must allow that person to search for a job one day a week, thus reducing effective subsidy.

capita size, run on a national scale, would have had about 0.220 million graduates in 1998.

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*Source:* Author's estimates.

*Note:* This table expands on table 8.2 and lists the sources for its figures.

<sup>a</sup> American Public Welfare Association (1998), on both state wage subsidies and state tax credits for hiring the disadvantaged.

<sup>b</sup> Savner and Greenberg(undated).

<sup>c</sup> Ball (1984); the 16 percent figure is from page 75.

<sup>d</sup> Rangan (1984) and Rode (1988).

<sup>e</sup> These calculations are based on projecting Minnesota's per capita spending levels and participation levels in 1984 to the nation for 1998.

<sup>f</sup> Auspos, Cave, and Long (1988).

<sup>g</sup> Freedman, Bryant, and Cave (1988).

<sup>h</sup> Roberts and Padden (1998a, 1998b).

TABLE A13.1 *Detailed Description of Empirical Studies of Entitlement Wage Subsidy Programs*

Study	Methodology	Results
Impact (1977), as reported in Hamermesh (1978)	Survey of employers using the WIN tax credit.	Less than 10 percent of employers using credit attributed their hiring of the WIN enrollee to the credit.
Wisconsin DHSS and Institute for Research on Poverty (1982)	Experimental study of the effects of promotional efforts on firms' usage of WIN tax credit and TJTC. 1,405 firms were involved with test of mail-only promotion (sending brochure), and about 284 firms were involved with test of mail-plus-phone promotion (mailing brochure, plus follow-up phone call). About half of the firms were randomly assigned to treatment in each case, half to controls. The brochure emphasized money savings to the firm, a lack of red tape, and who to contact. Phone contact emphasized these features plus survey findings that most employers had been pleased with the quality of the subsidized workers. The dependent variable was whether the firm made a certified hire for tax credit during the five months after promotional efforts were	Mail-only promotion slightly increased firms' usage of tax credits, but not to a statistically significant extent. Mail-plus phone promotion approximately doubled firms' usage of tax credits, from 4.5 percent of firms to 9.0 percent. This increase is statistically significant at the 5 percent level in an analysis that adds in other firm characteristics. Because of the study design, it is unclear whether the extra effect of the mail-plus-phone promotion was due to the personal nature of the phone contact or the extra message of the phone contact (that most firms found that workers hired under these tax credit programs were of good quality). Firm size was highly correlated with the use of tax credits: 23 percent of firms with more than one hundred employees used the credit, versus less than 4

TABLE A13.1 *Continued*

Study	Methodology	Results
O'Neil (1982)	undertaken (March 1 to July 31, 1981). January 1980 survey of 720 firms.	percent for firms with fifty or fewer employees. Sixty-three percent of firms had heard of TJTC, 38 percent had used or planned to use TJTC, of whom 26 percent said that using TJTC would increase employment level, and 46 percent said they substituted target hires for similar nontarget hires. Both knowledge and usage or planned usages of TJTC are highly correlated with firm size, with usage particularly low among firms with twenty or fewer employees; usage or planned usage was 22 percent in this group, versus 37 percent for firms with twenty-one to sixty employees, 42 percent for firms with sixty-one to one hundred, and 48 percent for firms with more than one hundred employees.
Christensen (1984)	Random survey of 5,301 firms in 1980 and 3,710 firms in 1982. Underlying data come from various studies led by Bishop and his colleagues (cited later in this table), although statistical analysis is different. The survey both directly asked firms how TJTC affected their employment decisions and ran regression models to estimate determinants of TJTC usage and how credit affected employment growth and change in share of youth employment. Survey also analyzed CPS data on employment in March 1983 of youth eligible for TJTC in 1982 and looked at how these youth were affected by state vouchering activity in 1982. Because there were no controls for endogeneity of state vouchering activity in this approach, doubts are raised about the results.	Thirty-four percent of employers who use credit claim it affects their hiring decisions significantly, and 22 percent claim it has some effect on hiring decisions. TJTC usage is much greater among large establishments than small establishments, with 45 percent of those above two hundred in employment size using TJTC, versus 16 percent for those with five to nineteen employees. Probability of TJTC usage was 21 percent greater among firms contacted by the government about TJTC, and 32 percent higher among firms contacted and asked to accept a specific referral. Firms that had received TJTC in 1980 or 1981 increased their share of youth in total employment by 6 percent to 20 percent more than other firms, even when we instrument for TJTC usage using a variable for whether firm had been contacted by government employment agencies about TJTC. But employment

*(Appendix continues on p. 390.)*

TABLE A13.1 *Continued*

Study	Methodology	Results
Bishop, in Bishop and Hollenbeck (1985, ch. 4)	Regression analysis of employment of around 3,000 firms, December 1980 to June 1982, and the proportion of employment of youth as a function of a dummy variable for whether the firm participated in TJTC. Attempts to instrument for TJTC participation were unsuccessful.	growth is not statistically significantly greater in firms that use TJTC compared to firms that do not. Finally, for every increase of one in state vouchering activity in 1982, there was a 0.4 increase in employment for eligible youth as of March 1983. Because there were about 45 percent as many certificates as vouchers, one could argue that each certificate for a youth was associated with about one job for each youth. No sign that TJTC had a significantly negative effect on employment for nondisadvantaged youth.
Bishop and Hollenbeck (1985, ch. 5)	1982 survey of stratified random sample of firms.	Based on typical number of certifications associated with TJTC participation, for every ten TJTC-subsidized jobs at a firm, there will be three to four extra jobs, and six or seven displaced, with probably most of those displaced being nonsubsidized youth (four or five).  Eighty percent of TJTC certificates are at firms that claim they tried to select TJTC eligibles. Eighty percent of TJTC certificates are at firms that claimed they had contacted ES or other government agencies to request referrals of TJTC eligibles. But only 18 percent of firms reported that job applicant's eligibility influenced hiring decisions "a great amount," and only 15 percent reported that it influenced their decision "a moderate amount."
Bishop, in Bishop and Hollenbeck (1985, ch. 7)	1983 interviews in which 850 employers were asked to rate 11 imaginary job applications on a scale from 0 to 200, in which 50 was the worst applicant ever hired, 100 was average, and 150 was the best applicant. TJTC status of application was randomly assigned.	On average, TJTC status only slightly increased employer's rating, by 1.9 points on average. TJTC eligibility helped much more for firms that provided more general training and paid low wages. TJTC was more helpful for applicants with low schooling.

TABLE A13.1 *Continued*

Study	Methodology	Results
Copeland (1985); Hollenbeck, in Bishop and Hol- lenbeck (1985, ch. 8)	Case studies of nonrandom sample of thirty-five firms in industries that heavily use TJTC, with oversupply of firms that heavily use TJTC.	Twenty-six percent of firms reported giving hiring preference to TJTC applicants. Nine percent reported creating openings to hire TJTC applicants. Twenty-nine percent screen for TJTC eligibility before making hiring decision. Forty percent claim TJTC increased use of ES. Seventy-five percent of firms use consultants to screen applicants for eligibility and arrange for ES certification. Only 11 percent felt TJTC workers were less productive than other workers. Eight percent felt TJTC workers had higher turnover. Fifty-five percent of firms had monetary incentives to encourage local managers to hire and certify TJTC workers.
Bishop and Mont- gomery (1986)	Analysis of 1980 survey of 5,279 establishments. Particularly focused on the 20 percent familiar with TJTC and the 31 percent familiar with WIN tax credits (see table 8.6 for analysis of CETA-OJT). Focused also on models for explaining determinants of establishment's familiarity with each program and explaining determinants of usage of each program, conditional on establishment being familiar with that program.	Among establishments familiar with the particular subsidy program, the proportion using it in each case goes up significantly if the firm first learned of the WIN tax credit from a government representative. This increases the WIN participation rate by 70 percent (mean participation rate is 0.15) and increases the TJTC participation rate by 53 percent (mean participation rate is 0.07). Among all establishments, a 10 percent increase in other establishments at site that learned of WIN via government increases proportion familiar with these three programs by 8 percent for WIN and 9 percent for TJTC. Both familiarity with programs and usage conditional on familiarity were significantly greater for larger establishments.
Lorenz (1988)	Comparison of earnings of three groups: those vouchered and then hired under TJTC, those submitted for TJTC certification upon hire, and those vouchered but not hired under TJTC during first six months	The earnings of the groups were quite similar in the 1981 period. As one might expect, the groups that were hired for TJTC jobs in early 1982 had considerably higher earnings in the period from July 1, 1982 to

*(Appendix continues on p. 392.)*

TABLE A13.1 *Continued*

Study	Methodology	Results
	<p>of 1982, in Maryland and Missouri. Three groups were compared for three types of TJTC eligibles: disadvantaged youth, welfare recipients, and Vietnam veterans. Sample was followed for one year prior to hire, and five years after hire. No attempt was made to control for unobserved differences across groups that might explain why the first two groups were hired in TJTC jobs in early 1982.</p>	<p>June 30, 1983. These earnings differences tended to erode over time, although differentials tended to stay higher for the youth group. Differentials in favor of TJTC hires were concentrated on those whose employers ended up receiving full TJTC credit. This result also could be due to selection bias.</p>
<p>Hollenbeck and Willke (1991); Hollenbeck, Willke, and Ershadi (1986)</p>	<p>Comparison of change in average annual earnings and employment from two years before potential vouchering to two years after potential vouchering, for vouchered TJTC eligibles in states with average voucher penetration rate of eligible population versus nonvouchered eligibles in states with zero penetration rate. Comparisons were the same for the displacement analysis, except that it looked at nonvouchered TJTC eligibles in states with average voucher penetration rates.</p>	<p>Vouchered youth and the handicapped generally gain, with average annual real earnings gain of \$462 (1998 dollars) for youth and \$1,940 for the handicapped. Most of the earnings gain appears to be due to employment increases. Among youth, white females gain the most. But there are many indications of displacement: losers include nonvouchered black male youth, both vouchered and nonvouchered black males, black females, white females on welfare, and both vouchered and nonvouchered black veterans. These are results without correction for nonrandom nature of vouchering, but with controls for standard demographics, controls for area economic conditions, and implicit controls due to changes specification for prior economic status. Corrections for selection bias yield implausible results.</p>
<p>Bishop and Kang (1991)</p>	<p>Analysis of cross-sectional data on 3,412 establishments collected in 1982, with oversampling of large firms in low-wage industries. Asked questions about number of employees hired under TJTC in 1980, 1981, and 1982. Estimated Poisson model of determinants of number of TJTC hires by establishment, as</p>	<p>Government offer to refer eligible workers increased expected number of TJTC hires five to twelve times, depending on year. Government contact without referral offer increased number of TJTC hires by 43 percent to 87 percent depending on year. Prior involvement with other government wage subsidies significantly</p>



TABLE A13.1 *Continued*

Study	Methodology	Results
	function of characteristics of establishment and firm and interactions with government.	increased TJTC usage. An establishment that used NJTC, WIN tax credit, and CETA-OJT in the past, compared to an establishment that used none of these programs, had a three to ten times greater number of TJTC hires. In 1980 and 1981, larger establishments were much more likely to make more TJTC hires; in 1981 and 1982, larger firms were much more likely to make more TJTC hires.
U.S. General Accounting Office (1991)	Interviewed sixty employers among top TJTC users in four states.	Forty-five percent of employers made special efforts to recruit, hire, or retain TJTC-eligible workers, including developing relationships with agencies that supplied TJTC eligibles, giving incentives to local managers for hiring and retaining TJTC, and being more lenient in job standards with TJTC hires.
Bishop and Montgomery (1993)	Regression analysis of the percentage change in individual firm's employment from end of 1980 to 1981, as a function of change in TJTC hires as a proportion of previous level of employment. The marginal effect of TJTC is allowed to vary with proportion, with spline at proportion of .50. This regression overestimates the effects of TJTC because growth increases new hires, increasing TJTC in turn. Hence, the study also estimated growth as a function of the ratio of TJTC hires to all new hires, an estimate that should be biased downward. It also examined the effect of TJTC hires on youth's share of total employment.	In specification that is biased upward, TJTC was estimated to increase total firm employment by 30 percent of certifications for certifications up to 50 percent of total employment, with zero marginal effect for certifications above 50 percent. In specification that is biased downward, TJTC was estimated to increase total firm employment by 13 percent of TJTC certifications (up to 50 percent proportion, with marginal effect beyond assumed to be zero), but estimate is not significantly different from zero. TJTC was also estimated to increase the number of young workers hired, by just slightly less than TJTC effect on overall employment. This implies that TJTC has at most slight displacement effects on adults, although it may have displacement effects within youth group.

(Appendix continues on p. 394.)

TABLE A13.1 *Continued*

Study	Methodology	Results
U.S. Department of Labor (1994)	Selected sample of 983 TJTC workers. Asked employers whether TJTC worker would have been hired without tax credit. If employer did not answer, assumed that if applicant said eligibility was checked before hire, then TJTC caused hire.	Only 8 percent of TJTC certificates were hired by their employers because of their TJTC status. Average starting wage was \$4.96 (1992 dollars). Thirty-seven percent were paid minimum wage or below. Sixty-five percent of jobs had no fringe benefits.
Tannery (1998)	Compared treatment group of youth and welfare recipients in Pennsylvania who were hired from 1988 to 1994 and then management assistance companies applied for certification on their behalf and received that certification, with control group of individuals for whom certification was not processed owing to technical paperwork errors (for example, missing signatures or data). The comparison uses a sparse set of demographic controls but does not control for prior earnings or employment. Also, it is apparent from data that the control group is significantly different from the treatment group in a number of respects; the source of these differences is unknown. Finally, it is not clear what "treatment" amounts to in this analysis: both groups were employed by an employer who submitted them to be certified; the only difference was that at some later date the certification was not received. Are the differences detected due to certification yielding more job retention or to the possibility that firms that made errors were different sorts of employers?	In general, certification is estimated to increase earnings of disadvantaged youth between \$2,000 and \$4,000 per year, and it increases earnings of welfare recipients by about \$1,600 per year. Certification is also associated with more quarters of employment during the 1988 to 1994 period, increasing the quarters worked by 2 percent to 6 percent for various groups. However, as noted under "Methodology," it is not clear what "treatment" means in this study, and it is not clear that the controls do not differ from the treatment group in the types of firms in which they were employed.
Katz (1998)	Uses difference-in-difference-in-differences estimator that is based on restriction in 1989 that made disadvantaged twenty-three- to twenty-four-year-olds no longer eligible for TJTC. Looks at trends in	TJTC was estimated to increase employment rates of disadvantaged twenty-three- to twenty-four-year-olds by 3.4 to 4.3 rate points. Suggests that 40 to 52 percent of TJTC certifications create jobs for the

TABLE A13.1 *Continued*

Study	Methodology	Results
	<p>relative employment rates of disadvantaged and nondisadvantaged twenty-three- to twenty-four-year-olds versus trends in relative employment rates of disadvantaged and nondisadvantaged eighteen- to twenty-two-year-olds (disadvantaged in this group stayed eligible for TJTC) and trends in relative employment rates of disadvantaged and nondisadvantaged twenty-five- to twenty-nine-year-olds (never eligible for TJTC). Also does some regression adjustment for demographic changes in one estimate.</p>	<p>group targeted. If this were applied to all TJTC, TJTC would have created 249,000 to 322,000 jobs for the disadvantaged groups it targets at its peak in 1985. <i>Caveat:</i> there is some sign that the relative employment of disadvantaged twenty-three- to twenty-four-year-olds started declining in 1988, before TJTC eligibility was eliminated.</p>
<p>Woodbury and Spiegelman (1987); Spiegelman and Woodbury (1987); Dubin and Rivers (1993)</p>	<p>Experimental data, comparing UI benefits collected and weeks of unemployment of two groups of UI recipients: treatment group whose employers would get \$757 bonus (1998 dollars) if UI recipient was re-employed within eleven weeks and stayed employed for four months; control group who were subject only to regular UI benefits and rules.</p>	<p>Bonus reduced initial spell of unemployment by statistically significant two-thirds of a week on average, but reduction in unemployment over entire benefit year is only marginally significant (probability of 0.19) reduction of 0.36 weeks. Take-up rate is also very small: only 5 percent of sample submitted "notice of hire" to the state of Illinois, which is less than one-fourth of the 21 percent of the experimental group that was re-employed within eleven weeks. If experimental effects were concentrated within this group, this implies that experiment reduced weeks of unemployment of this group by about thirteen weeks, compared to control group mean of twenty weeks. Effects on overall benefit year unemployment are statistically significant for white women. Effects of employer bonus appear to be stronger for high school dropouts, those previously employed in clerical or sales jobs, persons with below-average prior earnings, and those who get jobs in wholesale and retail trade. Participation in program in sense of agreeing to participate is greater for those with lower prior earnings. Although</p>

(Appendix continues on p. 396.)

TABLE A13.1 *Continued*

Study	Methodology	Results
		employment bonus has effects, they are so small (or rather, the take-up rate is so small) that a universal national program would have very small effects on total employment. A national program today might increase average employment levels by 5,000 persons and cost perhaps \$17 million per year. <sup>a</sup>

*Source:* Author's estimates.

*Note:* This appendix presents a more detailed description of the results from empirical studies reviewed in table 8.5.

<sup>a</sup> National figures are derived in the following way. Based on Spiegelman and Woodbury (1987), over the sixteen-week period in which the Illinois experiment was conducted, approximately 12,000 new claimants came in who met the experimental criteria, in an area in which weekly unemployment averaged 300,000. Hence, over a fifty-two-week period, the number of persons eligible for an employer bonus-style treatment averages about 13 percent of average total unemployment. Average unemployment in the United States nationally now is about 6 million, so the number of persons to whom this treatment might be applied is 780,000. We would expect treatment to increase employment for this group by an average of .36 of a week over a year. Multiplying 780,000 times .36 over 52 results in an increase in employment of about 5,400. Also, of 780,000, only 2.8 percent would claim bonuses, which average \$757 in 1998 dollars. Multiplying this out, we have 21,840 claiming bonuses and a total bonus cost of \$16.5 million.

TABLE A14.1 *Detailed Summaries of Estimates of Effects of Discretionary Wage Subsidy Programs*

Study	Methodology	Results
Various studies of MDTA OJT, summarized by Perry et al. (1975)	Four different studies, comparing earnings gains of MDTA trainees who participated in OJT to some comparison group of nontrainees.	Average earnings effects in first post-OJT year across all studies and groups is \$1,834 (1998 dollars). Effects were generally greater for women than men, and greater for blacks than whites. OJT effects generally were greater than institutional training in studies looking at both.
Farber (1971), as reported in Perry et al. (1975) <sup>a</sup>	Comparison of 1969 earnings of 1968 JOBS contract participants to matched comparison group based on preprogram earnings from social security.	Overall average effect on earnings is increase in annual earnings of \$1,188 (1998 dollars). Effects on females are five times those on males, effects on blacks two or three times those on whites. Annual earnings effects on black females estimated to be \$3,319 (1998 dollars).
Ball et al. (1981)	During latter part of YIEPP, job developers for YIEPP in Detroit and Baltimore varied the wage subsidy offered among different private businesses. In Detroit, the wage subsidy varied between 100 percent and 75 percent based on random assignment; in Baltimore, it varied between 100 percent and 50 percent based on lo-	Regression adjusted participation rates (verbal agreement to hire youth under program; firms might have changed mind later, in either direction): under the 100 percent wage subsidy, 18.2 percent agreed to participate and hire disadvantaged youth; under the 75 percent wage subsidy, 10.0 percent agreed to par-

*(Appendix continues on p. 398.)*

TABLE A14.1 *Continued*

Study	Methodology	Results
	<p>cation of firm. Regression model estimated pooling both cities, and estimating firm's decision to participate as function of subsidy offered, and a variety of firm characteristics. Tests indicate two cities can be pooled in regression.</p>	<p>ticipate; and under the 50 percent wage subsidy, 4.7 percent agreed to participate. These differences are statistically significant. Participation is also significantly higher for younger businesses, growing businesses, and businesses with prior experience with youth. Participation did not vary significantly, controlling for other factors, with size of business, prior experience with government employment and training programs, and financial condition or profitability of firm.</p>
Ball et al. (1981)	<p>Participating firms were asked to estimate the percentage of youths' work hours that resulted in displacement of other workers, versus the percentage that represented a net increase in total work hours for the firm. These survey responses were then adjusted by researchers to be internally consistent and to add up to 100 percent. Note that YIEPP program rules are supposed to prohibit "displacement" or "substitution" in the sense of directly laying off one worker to hire a YIEPP worker, although program rules could not prevent a firm from hiring a YIEPP worker instead of another worker.</p>	<p>Mean reported displacement rate is 49.9 percent among 111 surveyed firms, and median displacement rate is 50 percent. That is, for every two youths subsidized by YIEPP at private firms, overall employment is estimated to increase by one. It is not clear whether displacement is of other youths, other disadvantaged, or adults or nondisadvantaged. Displacement is significantly higher at worksites whose quality was rated higher by outside observers. (Compared to "inadequate" worksites, displacement is higher by 14 percent at adequate sites, 18 percent at good sites, and 22 percent at "outstanding" sites.) Displacement is lower for full-time summer work than for part-time school-year work, by 17 percent.</p>
Unicon (1982)	<p>Trained observers visited twenty-nine participating firms at four different sites, interviewed managers of firms, and attempted to judge whether hiring a YIEPP worker had resulted in a net increase in employment or displaced some nonsubsidized worker.</p>	<p>Weighted mean estimate from four cities is 60 percent displacement in the private sector, implying that for every ten YIEPP jobs in the private sector, net business employment increases by four. It is unclear for whom the 60 percent displacement substitutes.</p>
Gould, Ward, and Welch (1982)	<p>Nonlinear regression analysis of employment data for 410 to 514 for-profit firms (depending on period examined), and of how their em-</p>	<p>Net displacement is estimated to average around 19 percent, but this low figure is in part due to the estimate that each YIEPP worker leads to an</p>

TABLE A14.1 *Continued*

Study	Methodology	Results
	<p>ployment during YIEPP is related to pre-YIEPP employment, revenue growth, and ratio of YIEPP workers to predicted total workers, with YIEPP to total workers allowed to have nonlinear effect on net employment. The effect of YIEPP on employment as YIEPP-to-non-YIEPP ratio approaches infinity is assumed to reflect effects of YIEPP on requiring extra supervisors. There were some problems with the quality of the employment data owing to some firms being confused about whether total employment includes YIEPP workers.</p>	<p>extra .28 of a supervisor. Hence, displacement of nonsupervisory workers is estimated to be 47 percent. So, for every ten YIEPP workers hired by for-profit firms, the firm's net employment goes up by about eight, but about three of these are extra supervisors. Net employment of nonsupervisory personnel goes up only by around five.</p>
<p>Farkas et al. (1984)</p>	<p>Non-experimental data comparing youth at sites participating in YIEPP to youth at previously similar sites not participating in YIEPP.</p>	<p>Annual earnings effect a few months after YIEPP program was terminated (although it had been mostly phased out a year earlier): \$1,306 (1998 dollars) for young black cohort. This effect is overall YIEPP effect; subsidized private for-profit jobs were only 23 percent of YIEPP jobs.</p>
<p>Bishop, in Bishop and Hollenbeck (1985, ch. 4)</p>	<p>Regression analysis of employment of around 3,000 firms from December 1980 to June 1982, and the proportion of employment of youth, as a function of dummy variable for whether firm participated in CETA-OJT. Attempts to instrument for OJT participation were unsuccessful.</p>	<p>Based on typical number of OJT slots associated with CETA-OJT participation, for every ten OJT slots at a firm there are three extra jobs at the firm. OJT participation has insignificant effects on youth share of employment.</p>
<p>Barnow (1987)</p>	<p>Summary of various studies. Generally, regression analysis of postprogram annual earnings of CETA-OJT participants compared to individuals matched on pre-OJT earnings and other characteristics.</p>	<p>Adult women:<sup>b</sup> \$1,572 Adult men: \$1,851 Female youth: \$1,636 Male youth: -\$172</p>
<p>Bishop and Montgomery (1986)</p>	<p>Analysis of 1980 survey of 5,279 establishments. Particularly focused on the 57 percent familiar with CETA-OJT (see table 8.5 for analysis of TJTC and WIN tax credits). Focus is on models for explaining</p>	<p>Among establishments familiar with OJT, the proportion using OJT was increased by 85 percent if the firm first learned of the WIN tax credit from a government representative (mean participation rate is .075).</p>

*(Appendix continues on p. 400.)*

TABLE A14.1 *Continued*

Study	Methodology	Results
	determinants of establishment's familiarity with OJT and explaining determinants of usage of OJT, conditional on establishment being familiar with OJT.	Among all establishments, a 10 percent increase in other establishments in area that learned of WIN via the government increases the proportion familiar with OJT by 11 percent. Familiarity with the program increases significantly for larger establishments, but usage conditional on familiarity increases significantly for smaller establishments. Authors speculate that this may occur because local training offices tend to spread referrals among employers.
Rode (1988) and Rangan(1984)	Survey data on participating MEED employers, asking whether they would have expanded to their present size without the MEED subsidy.	In 1988, 56 percent claimed they would not have expanded to their current size without the MEED subsidy; in 1984, 59 percent made this claim. It should be noted that the average number of MEED placements per business was 2.8 in 1984 and 1.8 in the latter period, so in most cases the issue is whether the firm would have added one worker without MEED. As noted in chapter 8, MEED was focused on small businesses: more than 80 percent of the private businesses participating had twenty or fewer full-time employees.
Auspos, Cave, and Long (1988)	Random assignment experiment in which welfare recipients who volunteered and who were felt to be suitable were randomly assigned to a treatment sequence of prevocational training, then work experience, then OJT wage subsidy.	The net annual earnings impact of the entire program sequence, per experimental, was \$1,243 in 1998 (after OJT). Only 30 percent of the treatment group participated in OJT, so if OJT causes the entire impact, then the impact of OJT would be much greater. During quarters four and five after random assignment, when prevocational training and work experience are mostly over and OJT is near its peak, the employment impact of program is between two-fifths and two-thirds of OJT employment. This is consistent with the belief that OJT in fact leads to employment for



TABLE A14.1 *Continued*

Study	Methodology	Results
Freedman, Bryant, and Cave (1988)	Random assignment experiment in which single-mother welfare recipients who volunteered and were felt to be suitable were randomly assigned to a group eligible for OJT wage subsidies.	some who otherwise would not find employment. Program statistics indicate that about seven out of ten OJT placements “roll over” into regular positions with the same employer. <sup>c</sup>  The annual earnings impact (1998 dollars) during the immediate postprogram period is \$929 per experimental. Since only 43 percent of all experimentals participated in OJT, and other differences in services received between experimentals and groups were small, the average earnings impact per OJT participant was \$2,160. During the main period in which experimentals received OJT subsidies, employment effects on experimentals were from 38 percent to 46 percent of the OJT-subsidized employment, suggesting that OJT leads to increased employment for a little less than half of OJT sample. Fifty-six percent of OJT participants completed their trial employment period, and all but one of those who completed the trial employment period rolled over into a regular job with the same employer. <sup>d</sup>
Orr et al. (1996) <sup>e</sup>	Random assignment experiment in which individuals were randomly assigned to treatment and control after they had been divided into one of three service strategies: OJT, classroom training, or other services. Each strategy received multiple services; for example, only 30 percent of the OJT strategy group received OJT, and 5 percent of the other two groups received OJT. Estimates reported here are for OJT strategy and are effects per enrollee.	Effects (in 1998 dollars) in months 19 to 30 after enrollment in JTPA are \$1,342 for adult females, \$1,479 for adult males, -\$1,182 for female youth, and -\$1,575 for male youth. Youth effects are not statistically significant. Effects for adult females on welfare at enrollment appear to be about twice as great as for average adult female. OJT effects are generally more positive than classroom training results for adults. However, for women, the “other services” category had higher earnings effects than the OJT service

(Appendix continues on p. 402.)

TABLE A14.1 *Continued*

Study	Methodology	Results
		strategy. Key services in “other services” appear to be job search and work experience.

*Source:* Author’s compilation.

*Note:* This appendix presents more detailed summaries of the studies listed in table 8.6.

<sup>a</sup> Perry et al. (1975, 198, table VIII-2), data on net changes, contract trainees versus controls.

<sup>b</sup> These figures are based on studies summarized in various studies. To combine results from different studies, I aggregated each study up to overall effects on adult women, adult men, female youth, and male youth. To aggregate, I assume, based on Taggart (1981), that CETA-OJT was 21 percent welfare recipients, 79 percent nonrecipients, 60 percent white, and 40 percent minority. The numbers reported in this table are means of the various studies in 1998 dollars of annual earnings effects. The means are based on six studies for men and women and two studies for youth. The range of effects reported in 1998 dollars are: adult women, \$88 to \$2,205; adult men, -\$908 to \$5,527; female youth, \$782 to \$2,490; and male youth, -\$870 to \$527.

<sup>c</sup> The long-run effects, in percentage terms, of the program on earnings are greater than the effects on the proportion “ever employed” in a quarter. This finding is difficult to interpret. These greater percentage earnings effects could be due to higher wages, more full-time employment, or steadier employment during the quarter for those employed.

<sup>d</sup> The impact of the program on those “ever employed” in a given quarter tends to fade over time, even though the earnings impact does not. It is difficult to know what to make of this. The earnings impact could be due to effects on wages, effects on weekly work hours, or effects on weeks employed during the quarter for those who are employed.

<sup>e</sup> For impact figures, I use months 19 to 30, because this is clearly a period after OJT would be expected to have ended, whereas months 7 to 18 may also include OJT enrollment for some participants. Impact figures are per enrollee in JTPA, not assignee. On the other hand, figures are not per enrollee in OJT. If all these effects were assumed to be due to OJT participation of 30 percent of OJT enrollees who received OJT, the numbers in the table would have to be multiplied by around three.

TABLE A15.1 *Estimated Five-Year Effects of Phelps and Haveman Proposals*

	Phelps's Proposal	Haveman's Proposal
Impact on overall employment after five years	9.002 million jobs (10.89)	4.518 million jobs (11.01)
Impact on overall wages after five years	14.9 percent (4.44)	5.0 percent (5.96)

*Source:* Author's estimates derived from simulations using model of appendix 2.

*Notes:* Numbers in parentheses are pseudo *t*-statistics, calculated as ratio of mean effects to standard deviation of these effects from Monte Carlo repetitions of simulation. Percentage impact on overall wages is actually 100 times impact on  $\ln(\text{average overall wage})$ . Wage effects for each of the five groups is quite similar in this model, within a few tenths of a percent except for the female head group, whose market wage goes up somewhat less. Most of the new jobs go to three non-college-educated groups in this model (7.1 million in Phelps model, 3.6 million in Haveman model).