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ARE TARGETED WAGE SUBSIDIES HARMFUL? EVIDENCE FROM A WAGE VOUCHER EXPERIMENT

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This paper presents the results of a controlled experiment designed to test the effectiveness of a targeted wage subsidy program. Such programs are intended to increase the employment of target group workers by offering employers reimbursement for part of those workers' wages. In the Dayton, Ohio, experiment, however, job seekers given experimental vouchers identifying them to employers as eligible for a generous wage subsidy were significantly less likely to find employment than were job seekers without vouchers. The author speculates that the vouchers had a stigmatizing effect and provided a screening device with which employers discriminated against economically disadvantaged workers.

SINCE the mid-1970s the federal government has increasingly relied on targeted wage subsidies to improve the employment prospects of disadvantaged workers. In this paper I examine surprising experimental evidence casting doubt on the effectiveness of this policy. The theory behind wage subsidies is simple in the extreme. The government offers to reimburse employers for a fraction of the wages paid to certified workers in selected target groups. The reimbursement is ordinarily limited to a certain maximum amount and to a restricted period, for example, the first one or two years of a worker's employment.

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Because wage subsidies make the net cost of hiring target-group workers lower than it otherwise would be, employers are expected to increase their hiring of such workers. This may occur either because overall employment is expanded as a result of the lower marginal cost of labor or because employers replace unsubsidized workers with subsidized workers on their payrolls.¹

In the recent past the federal government has established three wage subsidy programs, although one was quickly phased out after failing to attain much popularity among employers. Under the JOBS (Job Opportunities in the Business Sector) program of the late 1960s, private firms were offered contractual agreements with the Labor Department that provided reim-

¹For a collection of essays that generally favor targeted subsidies, see Robert H. Haveman and John L. Palmer, *Jobs for Disadvantaged Workers: The Economics of Employment Subsidies* (Washington, D.C.: Brookings Institution, 1982).

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bursment for special on-the-job training costs associated with hiring disadvantaged workers. The program did not achieve its employment goals in its early years and was largely unutilized after the 1970 recession. It was abandoned in the early 1970s.

The WIN (Work Incentive Program) tax credit and the Targeted Jobs Tax Credit (TJTC), enacted in 1971 and 1978, respectively, were longer-lived wage subsidy programs; the TJTC remains in effect. Both programs offered tax credits rather than direct payments to employers hiring certified target group workers. The WIN tax credit provided tax benefits to employers hiring participants in the Aid to Families with Dependent Children Work Incentive Program. The target groups in the TJTC program have varied over time but have included economically disadvantaged youth, veterans, ex-convicts, and public assistance and Supplemental Security Income recipients. Tax expenditures on TJTC currently amount to about \$365 million per year. Unless Congress extends the program, as it did in 1981, TJTC is scheduled to expire in December 1985.

In addition to these programs, new subsidy schemes are frequently proposed. For example, targeted wage subsidies are a popular feature in various proposals for urban enterprise zones. In 1984 the Reagan administration proposed a wage subsidy for unemployed workers who exhaust regular unemployment insurance benefits. (Congress failed to enact the measure.) The idea behind these proposals is to encourage private firms to create jobs that the government might otherwise be tempted at great expense to create itself.

Faith in wage subsidies is based on the belief that employers must find the offer of subsidized workers irresistible. This faith is difficult to reconcile with the low take-up rates that have historically plagued targeted subsidy programs.² Only a small frac-

tion of potentially subsidized job seekers typically finds employment under these programs; in fact, many more job seekers find work with employers who do not bother to file for subsidy payments.

The apparent unpopularity of subsidies among employers is often attributed to ignorance of the programs, high costs of participation, or fear of paperwork burden. Even if each of these factors were present, none of them implies that subsidies would actually harm the employment chances of job seekers known to be eligible for the subsidy. But an experiment conducted by the U.S. Department of Labor in Dayton, Ohio, in 1980–81 demonstrates that under certain conditions targeted wage subsidies may actually have such an effect. Although the Dayton experiment was small in scale, the results show conclusively that workers known to be eligible for targeted wage subsidies were significantly less likely to find jobs than were otherwise identical workers whose eligibility for subsidies was not advertised.

Research Issues and Design of the Experiment

Origin of the Experiment. In 1978, when Congress reauthorized the Comprehensive Employment and Training Act (CETA), it directed the Labor Department to undertake a test of the effectiveness of direct wage subsidy vouchers. A wage voucher is simply a certificate provided to job seekers to assist them in their search for employment. If a vouchered job seeker is hired by a qualified employer, the voucher entitles the employer to claim a direct cash subsidy for some specified fraction of wages paid to the newly hired worker. A distinctive feature of this wage voucher approach is the responsibility of job seekers themselves to inform employers of their eligibility for subsidy benefits. Congress directed the Labor Department to provide the vouchers to economically disadvantaged job seekers eligible for other CETA services.

Coincidentally, the TJTC tax credit program was also authorized in 1978. This program permitted employers to claim tax credit wage subsidies for wages paid to certified workers in designated target groups. The TJTC was also to be publicized through

²See Dave M. O'Neill, "Employment Tax Credit Programs: The Effects of Socioeconomic Targeting Provisions," *Journal of Human Resources*, Vol. 17, No. 3 (Summer 1982), pp. 449–59. For a study of the implementation of TJTC in several local sites and a partial explanation of its low utilization, see Mershon Center, "The Implementation of the Targeted Jobs Tax Credit, Report Nos. 1, 2, and 3," mimeo (Columbus: Ohio State University, 1980 and 1981).

vouchers issued to eligible job seekers. The enactment of TJTC complicated the design problem faced by the Labor Department in establishing a test of direct wage subsidy vouchers.

Research issues. It is useful to consider some research questions about targeted subsidies. The important issue is, of course, the impact of subsidies on target group employment, but other issues are also significant.

Those who propose targeted subsidy programs usually express the pious hope that unsubsidized workers will not be displaced as a consequence of the subsidy, but such a wish is delusory if the subsidy is to achieve its intended effect. Even if employers do not discharge unsubsidized employees in order to replace them with subsidized workers, they must certainly favor potentially subsidized over unsubsidized job applicants for the subsidy to raise target group employment. Hence, one major issue in evaluating targeted subsidies is the amount of harm done to unsubsidized groups as well as the identity of those groups. Ordinarily, the degree of harm will be directly proportional to the amount of benefit conferred on the subsidized group.³ If the group aided is perceived as deserving, while the groups harmed are only slightly affected and are perceived as less deserving, the displacement issue will be unimportant. This would be the case, for example, if the target group consisted of welfare mothers and the harmed group of affluent teenagers.

Another important issue is the amount of subsidy windfall enjoyed by employers. If employers do not change their hiring or employee retention policies in response to the subsidy, but collect reimbursement for target group workers who would have been hired in the absence of the program, the subsidy simply provides a transfer from taxpayers to employers with no benefit for target group members. The portion of the subsidy that is windfall declines as employers raise the number of target group work-

ers in response to partial government reimbursement. Nonetheless, even a successful program will involve some windfall for employers, though sensible program design can reduce the amount of windfall.⁴

The desirability of a particular wage subsidy program will therefore depend on three factors: the amount of gain in target-group employment; the amount of harm to employment in unsubsidized groups, as well as the identity of those groups; and the fraction of total subsidy payments consisting of windfalls to employers.

The voucher experiment requested by Congress raises one additional issue, namely, the efficacy of vouchers as a mechanism for "marketing" job seekers eligible for the subsidy. If vouchers could be distributed to a high fraction of eligible job seekers, they might provide an efficient means of alerting employers both to the advantages of participating in the program and to the identity of eligible job seekers. The necessity for this kind of test was diminished, however, once Congress decided to enact the TJTC, a subsidy that also was supposed to be marketed using vouchers. The existence of the TJTC program also made it difficult to test the special advantages of the voucher marketing approach.

One difference between the experimental voucher requested by Congress and the TJTC program was the mechanism for reimbursing employers for wages paid to certified workers. The experimental voucher was to provide direct cash payments, whereas the TJTC offered reductions in tax liabilities through tax credits. In theory, the difference in payment mechanism should have little significance for firms with positive tax liabilities. (Firms with no tax liabilities, however, do not qualify for TJTC credits since they have no tax that can be reduced by the credit. Such firms would qualify for reimbursement under the direct cash payment plan.) Because of the similarity of the wage voucher requested by Congress to the existing TJTC voucher, the Labor Department

³The harm to the unsubsidized group will be less than the benefit conferred on the subsidized group to the extent that overall employment is raised by the subsidy. For a variety of reasons, however, it seems unlikely that total employment is much affected by a narrowly targeted wage subsidy.

⁴For example, the government could restrict reimbursement to newly hired workers or to those newly hired workers in excess of some baseline level.

was reluctant to undertake the required experiment.

Experimental design. The Labor Department, in cooperation with the economic research firm Mathematica Policy Research, adopted an extremely simple experimental design that emphasized the contrasts between wage vouchers and no vouchers, on the one hand, and between tax credit subsidies and direct cash subsidies, on the other.⁵ The experiment was based on a three-cell design in which two alternative voucher treatments were contrasted with a null treatment.

The first experimental treatment was the *tax credit voucher*. It was provided to a randomly selected group of eligible job seekers who were instructed to use the voucher as an important part of their job search efforts. Voucher holders in this group were eligible for either the WIN/welfare tax credit or the TJTC credit. To obtain certifications of eligibility for tax-credit-vouchered job applicants, employers simply indicated their intention to hire the applicant by completing the voucher form and returning it to the program operator. Tax certification forms were then sent to qualified employers for use in filing their federal tax returns. In addition to the wage vouchers, clients enrolled in the tax credit group were given written explanatory material about the program, which they were encouraged to distribute to employers they contacted during their job search. The main goal of this first experimental treatment was to inform employers of clients' eligibility for *existing* tax credit wage subsidies.

The second experimental treatment was virtually identical to the first except that employers hiring vouchered job applicants were eligible to receive *direct cash rebate subsidies* rather than tax credits for wages paid to vouchered workers. The subsidies were paid by the Dayton program operator and were computed to be equal in value to the tax credit subsidies provided by the WIN/welfare and TJTC credits. As in the tax credit programs, employers were eligible to receive subsidies equal to 50 percent of wages paid during the first year of employment, up to a wage limit of \$6,000 (or a subsidy limit of \$3,000), and 25 percent of wages paid during the second year, up to the same \$6,000 wage limit (or subsidy limit of \$1,500). The direct wage rebate payments were taxable as ordinary corporate income. Because of this tax treatment, the direct rebate subsidy was exactly equal in value to the tax credits for firms owing positive tax liabilities.⁶ For firms owing no federal income taxes, however, WIN/welfare and TJTC credits would be valueless, whereas the direct rebate subsidies would obviously be worth claiming.

To obtain certification of eligibility for the direct rebate voucher holder, employers simply completed the voucher form and returned it to the program operator. The operator then sent earnings declaration forms to qualifying employers. Firms mailed in an earnings declaration for certified workers every three months and received prompt wage subsidy payments for reported wages. Direct rebate voucher holders, like tax credit voucher holders, were provided with explanatory material about their voucher treatment, which they

⁵Additional information about the design and administration of the experiment can be found in the following sources: Harold Watts, "Design for Wage Voucher Experiment in EOPP," mimeo (Princeton, N.J.: Mathematica, 1980); Office of the Assistant Secretary for Policy, Evaluation and Research, Department of Labor, "Draft Guidelines for a Wage Voucher Experiment in Montgomery County," mimeo (Washington, D.C.: U.S. Department of Labor, November 1980); John Burghardt, David Delessandro, and Harold Watts, "Report on the Design and Early Operation of the Wage Voucher Experiment in Montgomery County, Ohio," mimeo (Princeton, N.J.: Mathematica, February 1981); and Gary Burtless and John Cheston, "The Montgomery County, Ohio, Wage-Subsidy Voucher Experiment: Initial Findings," mimeo (Washington, D.C.: U.S. Department of Labor, July 1980).

⁶It should be noted that firms claiming the WIN/welfare or TJTC credits are not eligible to claim an ordinary deduction for covered wages paid to workers for whom they are claiming the credit. For that reason, the direct rebate payment was made taxable in order for the two vouchers to have identical value. To protect employers against the possible expiration of TJTC in December 1981, firms hiring TJTC-covered workers under the tax credit plan were guaranteed that scheduled subsidies after January 1, 1982, would be paid as direct cash subsidies if the TJTC program were not reauthorized. In the event, TJTC was reauthorized in slightly modified form so the backup subsidy plan did not have to be used.

were encouraged to supply to prospective employers during job search.

A third group of randomly selected clients became the control or null treatment group. Although identical in every respect to job seekers in the two experimental voucher groups, members of the control group neither received subsidy vouchers nor were informed of their eligibility for the WIN/welfare or TJTC tax credit subsidies. It was believed that in Dayton, Ohio, control group members were not likely to be informed of their eligibility for the tax credit subsidies by the CETA prime sponsor, the welfare department, or the local Employment Service office. The tax credit programs were not widely advertised through the use of vouchers before the experiment. Hence, members of the control group were not being denied assistance that would ordinarily have been available to them in the absence of the experiment.⁷

The wage voucher experiment was conducted by the Montgomery County CETA prime sponsor, with the technical assistance of the Office of the Secretary of Labor and Mathematica. The prime sponsor was a participant in the 14-site Employment Opportunity Pilot Project, of which the voucher experiment was a part. The principal goal of the Pilot Project in Dayton was to aid public assistance recipients in their search for jobs.

General assistance and AFDC recipients who participated in the program were provided with two weeks of job search training, which focused on the development of specific job search skills, such as resumé preparation, rehearsal for job interviews, and telephoning for interview appointments. During one day of the two-week job search training period, clients in the two experimental voucher groups were taught how to use their vouchers in job search. The idea behind the vouchers was explained, and vouchered clients were encouraged to present their vouchers as well as supporting explanatory material to each prospective employer contacted during their job hunt.

As previously mentioned, members of the

control group were not given vouchers or instructed in their use. A day was spent, however, in teaching control group clients how to use job bank listings in the local Employment Service. Since this service was also mentioned to experimental group members and since it was available to all job seekers irrespective of experimental status, the designers of the experiment believed that the instruction represented a placebo treatment.

After the two weeks of job search training, all participants in the Dayton experiment were expected to spend six weeks in intensive, structured job search.⁸ At the end of that period the wage voucher expired. The Montgomery County prime sponsor used the job club approach to structure job search activities. This is an approach in which peer group support and pressure are important factors in motivating participants to actively seek work. In contrast to the job development and job referral approaches, which were more common elsewhere in CETA, the job club method relies on the skills and initiative of job seekers themselves to locate potential jobs and secure job interviews. For that reason, the wage subsidy voucher was seen as an appropriate addition to the search strategy used in Dayton.

The three treatments (two experimental vouchers and the null treatment) were randomly assigned to the participants in the experiment in order to assure that there would be no systematic differences among the populations receiving the different treatments. The random assignment assures that, except for chance error, the difference in outcomes observed for the treatments is attributable to genuine treatments effects rather than to the differing composition of the populations eligible for treatment. People enrolled in each treatment received their job search training in classes consisting entirely of clients assigned to the same treatment. To assure that no participant or administrator influenced the treatment assigned to a particular individ-

⁷For that reason there does not appear to be any ethical objection to the design of the control treatment.

⁸Some participants who were unsuccessful in finding a job during the six-week search period were later assigned to a CETA work or training position, which could last up to one year.

ual, Mathematica made the experimental assignments and did not permit participants to alter their voucher treatment after it was initially assigned. Teachers in the job search classes were rotated among treatment groups to ensure there would be no systematic difference due to teacher effects.

The experimental design can shed light on three of the issues mentioned earlier, although in view of the unexpected result two of these issues are not particularly relevant in evaluating the treatments. By comparing the job finding success of vouchered versus unvouchered clients, we can establish how much potential help wage subsidies provide in raising the employment of target group workers. By comparing the amount of subsidy payment made in the vouchered group to the amount of subsidy that *would be owed* on wages paid to control group members, we can estimate the fraction of subsidy payments that represents windfalls to employers. Finally, by comparing the relative employment rates in the tax credit voucher and direct rebate voucher groups, we can infer the relative effectiveness of these two payment mechanisms. Since the experiment only collected employment information on target group members, it would obviously be impossible to deduce the impact of the subsidies outside the target group.

Potential biases. The Dayton experimental design is not without drawbacks. The two most important sources of potential bias are control group contamination and queuing bias. Control group contamination might arise in this experiment if the control group did not receive a true null treatment, that is, a treatment accurately reflecting the environment faced by job seekers in the absence of a wage subsidy voucher. This might be the case, for example, if the “placebo” received by control subjects turned out to be an especially effective or ineffective aid to job finding. (I consider this unlikely.)

A more likely source of control group contamination is the possibility that local employers became aware of the availability of wage subsidies as a result of the experiment. This may have either helped or harmed the control group’s employment rate, depending on whether control group

members were perceived by employers as eligible or ineligible for subsidies. The potential extent of this bias might be uncovered through a survey of local employers, and such a survey, though never conducted, was originally planned as part of the experimental design. The very short duration of the experiment, however, makes it unlikely that employers could have learned enough about the availability of subsidies to either request them for unvouchered control group members or discriminate systematically against unvouchered job applicants.

A second source of experimental error might arise from queuing bias, a problem that is common in small-scale manpower experiments and demonstrations. Suppose there is a well-ordered queue of disadvantaged job seekers waiting to fill a fixed number of job slots. The experimental treatment may give an advantage to vouchered job seekers, moving them toward the front of the job queue and increasing their chances of employment. By contrast, job seekers not offered the treatment (for example, control group members) will be pushed toward the rear of the queue and will be less likely to obtain employment than they would be in the absence of the experiment.

Now consider what would happen if the wage voucher were offered in the context of a national program rather than of a small-scale experiment. If a high fraction of job seekers in the queue were then covered by a subsidy, the same rearrangement of the queue would take place, but the advantage to each potentially subsidized job applicant would be much smaller. Simply stated, the advantage to each subsidized applicant is reduced as the number of subsidized applicants rises, as of course it would do in a national program. By implication, queuing bias will cause a small-scale experiment to overstate the amount of employment change that is due to the experimental treatment.

There is no easy solution to this problem. Experimental findings subject to queuing bias nonetheless retain some value for policy making. Even if the findings do not provide a valid estimate of the ultimate impact of a program, they still may inform policy

makers about the relative attractiveness of different policy options. In this experiment, the findings can reasonably be taken to suggest whether employers find subsidized workers more attractive than unsubsidized workers, whether vouchering is a useful mechanism for marketing disadvantaged workers, and whether employers prefer direct rebates to tax credit subsidies. As it happens, the results demonstrate that employers find vouchered workers to be *less* attractive than unvouchered workers. Even if queuing bias leads to a slight overstatement of what the size of this effect would be in a heavily advertised national program, the result appears to be very strongly supported by the evidence. Queuing bias is therefore not a significant issue in this experiment.

Experimental Findings

Participants in the experiment. The Dayton voucher project began enrolling participants in experimental classes in December 1980. By the beginning of May 1981, shortly before enrollments ceased, the experiment had enrolled 916 participants, of whom 46 percent were AFDC recipients and the remainder general assistance recipients. General assistance recipients were required to participate in the project as a condition for receiving assistance payments in Dayton. These people were typically young and members of one- or two-person families in which no dependent children were present. Many were only temporarily destitute and probably expected to receive general assistance payments for only one or two months.

The AFDC recipients enrolled in the project were typically single women in their twenties who were responsible for the support of one or more dependent children. Unlike general assistance recipients, AFDC beneficiaries in Dayton were not required to participate in the voucher experiment, although many of them were mandatory participants in the WIN program. Some WIN participants were referred to the project because they had been unsuccessful in other WIN-sponsored activities or because no other suitable WIN position was available. For these reasons, the AFDC

recipients participating in the Dayton experiment may have faced poorer job prospects than typical women enrolled in WIN.

Of the 916 voucher project participants, roughly equal numbers were assigned to the three treatments. The program design originally called for a greater number of enrollees. The designers had also planned to conduct detailed interviews with participants and local employers in order to measure the impact of the tested programs. These plans were frustrated when the Reagan administration canceled the experiment along with other Labor Department research projects in the spring of 1981. The analysis sample here is consequently restricted to 808 program participants who had substantially completed the two-week job search training and six-week job search period by the time the experiment was canceled.⁹

Only two indicators of program success are available for analysis. The first is placement in a job (other than a CETA public service employment job) during the eight-week training and job search period. For persons who succeeded in finding a job during that period, we also know the initial wage on the job, which provides a second indicator of success.¹⁰

⁹Although this sample is smaller than originally planned, it is not especially small in comparison to the analysis samples in some other noted social experiments. For example, the Dayton sample is slightly larger than the sample of husbands analyzed in the New Jersey Negative Income Tax (NIT) experiment (799) and approximately twice the size of the male sample analyzed in the Gary NIT experiment (380). For the New Jersey sample size see Harold W. Watts and David Horner, "Labor-Supply Response of Husbands," in Harold W. Watts and Albert Rees, eds., *The New Jersey Income-Maintenance Experiment*, Vol. 2, *Labor-Supply Responses* (New York: Academic Press, 1977), p. 68; for the Gary sample size see Gary Burtless and Jerry A. Hausman, "The Effect of Taxation on Labor Supply: Evaluating the Gary NIT Experiment," *Journal of Political Economy*, Vol. 86, No. 6 (December 1978), p. 1123.

¹⁰As a result of the premature cancellation of the experiment, the Labor Department was able to collect only limited data on participants. This prevents an analysis of experimental response within racial and other subgroups assigned to the treatments and also prevents inclusion of statistical controls for the main effects of demographic characteristics. Although these extensions would be desirable, they are not necessary

Table 1. Job Placement Rates
in Dayton Treatment Groups.

Group	Sample Size Enrolled	Number Placed in Jobs	Percentage Placed in Jobs
Tax Credit Voucher	247	32	13.0
Direct Rebate Voucher	299	38	12.7
Control	262	54	20.6
Total	808	124	15.3

Findings. The Dayton project was not conspicuously successful in helping clients find work. Of the sample of 808 public assistance recipients, only 124—or about 15 percent—landed a job during the eight-week training and search period. Job-finding success was approximately equal in the general assistance and AFDC groups. The low placement rate is partly attributable to rising unemployment in Dayton during the course of the experiment as well as to the labor market handicaps of the participants. But it may also be that the experiment’s job search training and supervision were poor.

The most striking feature of the results is the marked difference in job-finding success between participants in the wage voucher groups and those in the control group. (See Table 1.) Surprisingly, the two vouchered groups attained a significantly lower average placement rate than did the control group (12.8 percent versus 20.6 percent).¹¹ This finding suggests that

vouchers hurt rather than helped welfare clients’ chances of obtaining jobs. An implication is that employers used the information provided by the vouchers to discriminate against target-group workers.¹² Employers may have used the vouchers to screen out job applicants known to be public assistance recipients. Alternatively, some may have reasoned that if the job applicants were so disadvantaged that they required a wage subsidy to find work, they were probably poor prospects for hiring and training.

A less surprising result is the finding that tax credit vouchers and direct rebate vouchers were about equally damaging to the employment chances of job applicants. (The placement rates are 13.0 percent and 12.7 percent, respectively, in the tax credit and direct rebate voucher groups.) This result is consistent with the hypothesis that employers used vouchers primarily as a labor market signal indicating potentially poor job performance, because in that case employers would have regarded the subsidy payment mechanism as of little or no account.

It should be noted that employers hiring clients in the direct rebate group were more likely to request subsidies than were employers of tax credit voucher holders. Payment requests were made for 14 of the 38 direct rebate job placements, but tax credits were requested for only 5 of the 32 tax credit job placements. There is thus slight evidence that employers prefer direct rebates to tax credit wage subsidies, though that preference has no effect on the likelihood of hiring a worker.

A more interesting aspect of the figures just cited is the high proportion of employers who failed to request payment of wage

for the unbiased estimation of the average experimental effect. In an unstratified experimental design, such as the one used in Dayton, demographic control variables are typically included to increase the precision of the estimate of the average experimental effect. If assignment to treatment is strictly random, inclusion of such control variables does not usually improve (or harm) the unbiasedness of the estimate.

¹¹The Chi-square test statistic for Table 1 is 8.28 (2 degrees of freedom), indicating statistically significant differences in placement rates for the three treatments. The null hypothesis—that placement rates in the three groups are identical—can be rejected at the 2 percent level.

¹²It is also possible that the vouchers caused workers in the two voucher groups to reduce the intensity of their job search, affecting their chances of finding a job. If some voucher holders thought their vouchers gave them a special advantage, this response to the treatment might be understandable. However, as a result of the job club strategy used in Dayton, the vouchered clients must have become quickly aware of the fact that possession of a voucher did not automatically result in a job offer. For that reason, it is extremely unlikely that voucher holders searched less actively than control group members.

subsidies. Of the 70 voucher holders who found employment, only 19, or little better than one-quarter, worked for firms that requested certification for wage subsidies. Presumably the 73 percent of employers who did not request payments considered the subsidies too small to justify the expense of filing for them. The filing forms themselves were quite simple, and are not likely to have discouraged employers from filing, but it may be that tracking the employment and earnings of selected workers is an expensive proposition.

Another explanation for the low utilization of vouchers is that many applicants who succeeded in finding jobs may have refrained from telling prospective employers that they were covered by a wage subsidy scheme. (Informal reports by the Dayton experiment staff suggest that at least a few applicants purposely refrained from mentioning the voucher.) The applicants may have correctly reasoned that the information contained in the voucher would not aid their chances of obtaining work. Although some applicants may thus have avoided the possible stigma arising from their vouchers, their employers were not given information that would have permitted a claim of the subsidy either. Since job applicants were not asked whether they mentioned their voucher to prospective employers, we cannot establish the true reason for employers' failure to request subsidy payment.

Although the job-finding success of vouchered clients was substantially below that of unvouchered clients, there is no evidence indicating that the two groups obtained different types of jobs. The average hourly wages of new hires in the vouchered groups and control group were nearly identical (\$4.07 and \$4.06, respectively). A high proportion of jobs in all three treatment groups reportedly paid the minimum wage or less.

Caveats. The Reagan administration's decision to abort the Dayton experiment after only six months' operation raises important issues in the interpretation of the results. As we have already seen, the failure to collect follow-up information from participants and local employers prevents a comprehensive analysis of reasons

for the subsidy's failure. The limited duration of the study may also have affected employer acceptance of the program. There was not enough time for word-of-mouth contacts to establish the credibility of the subsidy offer. The experiment did not counteract employers' lack of familiarity with the program with a publicity campaign aimed at gaining their confidence. One reason for failing to mount such a campaign was the fear that it might give rise to purposeful employer discrimination against unvouchered control group job applicants, thus biasing the experimental results. In the event, this fear appears to have been not only groundless, but faintly ridiculous.

Conclusion

Notwithstanding reasonable objections that might be raised to the experimental results, the finding that economically disadvantaged workers who are clearly identified to potential employers as disadvantaged have reduced chances of employment is both striking and plausible. In the Dayton experiment, the amount of harm done by the voucher must have been considerable. The same document that described the government's offer to pay up to one-half of the applicant's first-year wages and one-quarter of his or her second-year wages also informed employers that the applicant was a welfare recipient. Although the vouchered worker was offered at steep discount, employers appeared to interpret the voucher as implying "damaged goods." Given this interpretation, job applicants using the voucher were placed at a disadvantage in comparison to identical job applicants who refrained from identifying themselves as welfare recipients. As mentioned earlier, some participants in the experimental voucher plans reportedly refused to use their vouchers out of fear the vouchers would have a stigmatizing effect. The results imply that this fear was justified.

Whether or not readers accept the view that targeted wage subsidies can be harmful, the findings of the Dayton experiment contain disquieting implications for current and proposed wage subsidy policies.

The TJTC program has occasionally been criticized for low utilization rates and questionable effectiveness.¹³ The Dayton findings lend support to critics of TJTC and suggest that the program may be largely ineffective in raising the employment and earnings of target group members. In view of this conclusion, the wisdom of continuing the present program or initiating new targeted subsidy programs in the future must be seriously questioned.

In addition, the findings may have important implications for the design of other employment and training programs aimed at disadvantaged workers. Such pro-

grams should be designed to minimize the stigmatizing effects that might be conveyed by participation. Graduates of a training program should be perceived by employers as being well-trained workers rather than economically disadvantaged workers. This can be accomplished if disadvantaged workers are trained in institutions that also serve the nondisadvantaged, such as local public schools, proprietary training institutions, and community colleges. Graduates of these institutions do not typically suffer the stigma that is attached to job applicants who are specifically labeled "economically and socially disadvantaged." As the results from this experiment make clear, the adverse effects of such stigma can be severe.

¹³See O'Neill, "Employment Tax Credit Programs."