Volume Title: Seeking a Premier Economy: The Economic Effects of British Economic Reforms, 1980-2000

Volume Author/Editor: David Card, Richard Blundell and Richard B. Freeman, editors

Volume Publisher: University of Chicago Press

Volume ISBN: 0-226-09284-4

Volume URL: http://www.nber.org/books/card04-1

Conference Date: December 8-9, 2000

Publication Date: June 2004

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Author: John Van Reenen

URL: http://www.nber.org/chapters/c6754

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Active Labor Market Policies and the British New Deal for the Young Unemployed in Context

John Van Reenen

11.1 Introduction

On March 14, 2001 the number of British people claiming unemployment benefits fell below 1 million for the first time in twenty-five years. To celebrate the event, the prime minister gave a speech on the New Deal. The March 15, 2001 edition of the *Evening Standard* quoted the prime minister as saying "Nobody says to me they're on a skivvy[1] scheme. The sort of language used about employment programs in the 1980s is not used about the New Deal."

This paper addresses two questions. Does New Labour's flagship employment policy represent a significant break from the past—and has it worked? In the 1980s and 1990s U.K. governments introduced major changes in the levels and conditions for receipt of unemployment benefits. I examine the effects of a large labor market program that was introduced (initially in pilot form) in January 1998, the year after the election of the new government. The New Deal involves a cluster of different policies designed to getting the jobless (especially the young unemployed) back to work.

Since April 1998 *all* individuals aged between eighteen and twenty-four

John Van Reenen is professor of economics at the London School of Economics and the Centre for Economic Performance.

This paper draws extensively on joint work with Richard Blundell, Costas Meghir, and Monica Costa Dias. Michal Myck and Tom Clark have both helped with the calculations. Finance came from Leverhulme Trust. The author is very grateful to comments by all participants, especially David Card, David Grubb, Paul Gregg, Marty Feldstein, Richard Freeman, Richard Layard, Howard Reed, and two anonymous referees. The usual disclaimer applies. On-line information about the New Deal is available at www.newdeal.gov.uk.

1. A "skivvy" is a low status, low quality occupation. *Oxford English Dictionary* gives the following example of its usage: "I never thought myself capable of such strenuosities as to do a skivvy's drudgery."

who have claimed unemployment benefits (called *Job Seekers Allowance* or *JSA*) enter the New Deal program. There are two stages. First, there is a "Gateway" period, where the claimant is given intensive help with job search. Those who do not secure an unsubsidized job during this stage go on to the second stage of New Deal options that include subsidized full-time training or education, a wage subsidy paid to the employer, voluntary work, or placement with the Environmental Task Force (government-provided employment). It is a mandatory program—there is no fifth option for remaining on benefits.

In this paper I focus on evaluating the success of the New Deal program in moving people into jobs. In the short run this occurs in two main ways. First, there is an enhanced job search monitored by a meeting every two weeks with a personal advisor. The job search could be increased by (1) the more credible threat of benefit sanctions, (2) the provision of a greater quality and quantity of information on vacancies, or (3) the psychological effect of being connected again with the labor market. Second, the wage subsidy reduces the cost to the employer of taking on an unemployed person (by about 40–50 percent).

The job search aspect has many antecedents in benefit reforms initiated under the previous Conservative administration. In particular, the Restart initiative in 1986 began a new era of increased monitoring of the unemployed (see section 11.3). The New Deal has continued this tightening up of the work-search rules, but has combined it with much more generous funding of job search assistance and subsidized options. For young people there is now effectively a time limit on benefit receipt. The wage-subsidy element also has antecedents in Britain (and elsewhere) that I discuss briefly in section 11.3.

I draw on results using a simple difference-in-differences approach exploiting two sources of identification. The eligibility for the New Deal is age related, so we can compare outflows by different age groups before and after the New Deal was introduced. Additionally, the New Deal was introduced earlier in some areas, so we can compare young people in these pilot areas to young people in nonpilot areas. There are numerous factors that may bias these estimates that are discussed, including selectivity, differential macro trends, job quality, substitution, and general equilibrium effects.

I have some things to say about the other parts of the New Deal program (such as training), but the truth is that it is still in its early days. The long-run success of the program will in large part depend on its ability to enhance the productivity and employability of people going through the options. Publicly available data at the time of writing ends in 1999, so we are only just starting to observe the labor market performance of those leaving the twelve-month education and training options.

The results suggest that the reforms have successfully increased net employment for the target group. Young unemployed men are about 20 per-

cent more likely per period to gain jobs as a result of the New Deal (i.e., the probability of a young man unemployed for six months or more obtaining a job rose from about 5 percent a month to 6 percent a month). A substantial part of this effect is attributed to the wage subsidy option, but there is also some job assistance effect. An initial cost benefit analysis suggests that the program is worth continuing on efficiency grounds alone.

The plan of the paper is as follows. Section 11.2 gives some background, placing the U.K.'s labor market in historical and comparative perspective. Section 11.3 gives the history and details of the reforms. Section 11.4 offers some results on the evaluation of the New Deal. Section 11.5 gives the cost-benefit calculations, and section 11.6 offers some concluding remarks.

11.2 General U.K. Labor Market Background

In this section I sketch out some features of the labor market of the United Kingdom in historical and comparative perspective.

Figure 11.1 displays the total unemployed claimant count since 1960, and figure 11.2 shows the standard International Labour Organization (ILO) unemployment rates from 1978 onward. In many respects the United Kingdom is similar to other European countries. There has been a steady upward drift of unemployment since 1960, with a very large increase post-1979. Until the 1990s, the trough of each recession was associated with higher unemployment than the previous downturn. The expansion since 1993 has pushed the number of unemployed below that of the previous cycle to levels not seen since the last Labour government (1974–1979).

Another feature of U.K. unemployment is its volatility. The United Kingdom has experienced sharp boom-bust cycles. There were deep recessions in the early 1980s and early 1990s and a fast boom in the mid- to late 1980s. There was a similar boom in the late 1990s and early 2000s, although with lower levels of wage and price inflation.

Currently U.K. unemployment is relatively low by Organization for Economic Cooperation and Development (OECD) standards (see column [1], table 11.1). This has been a relatively recent phenomenon, however. Between 1983 and 1996, U.K. unemployment rates have been above the OECD average, certainly higher than Germany's (which has never fully recovered from the shock of reunification in 1989), although lower than France's.[2] Furthermore, in terms of its long-term unemployment rates, the United Kingdom appears much closer to a European country than to the United States.

Across almost all OECD countries youth unemployment is higher than

2. Between 1983 and 1996 OECD average unemployment was 8.2 percent—9.7 percent in the United Kingdom, 6.2 percent in West Germany, and 10.4 percent in France (Nickell 1997).

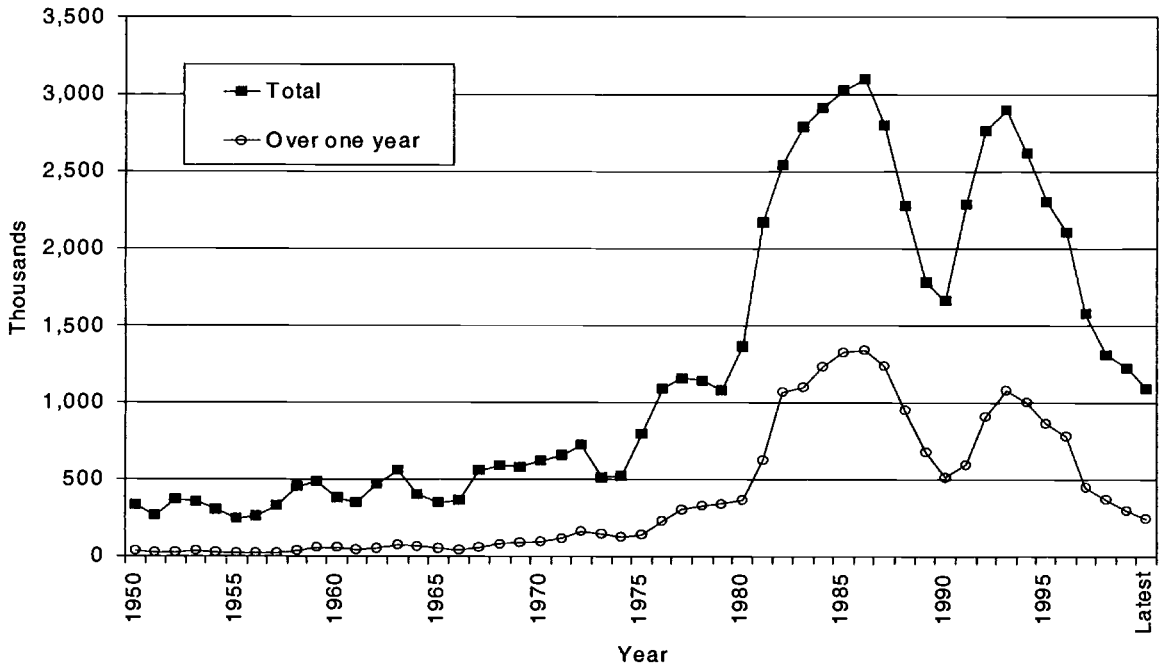

Fig. 11.1 Claimant unemployment—total and long-term

Sources: Data underlying the figure is available at http://www.statistics.gov.uk and Wells (2000).

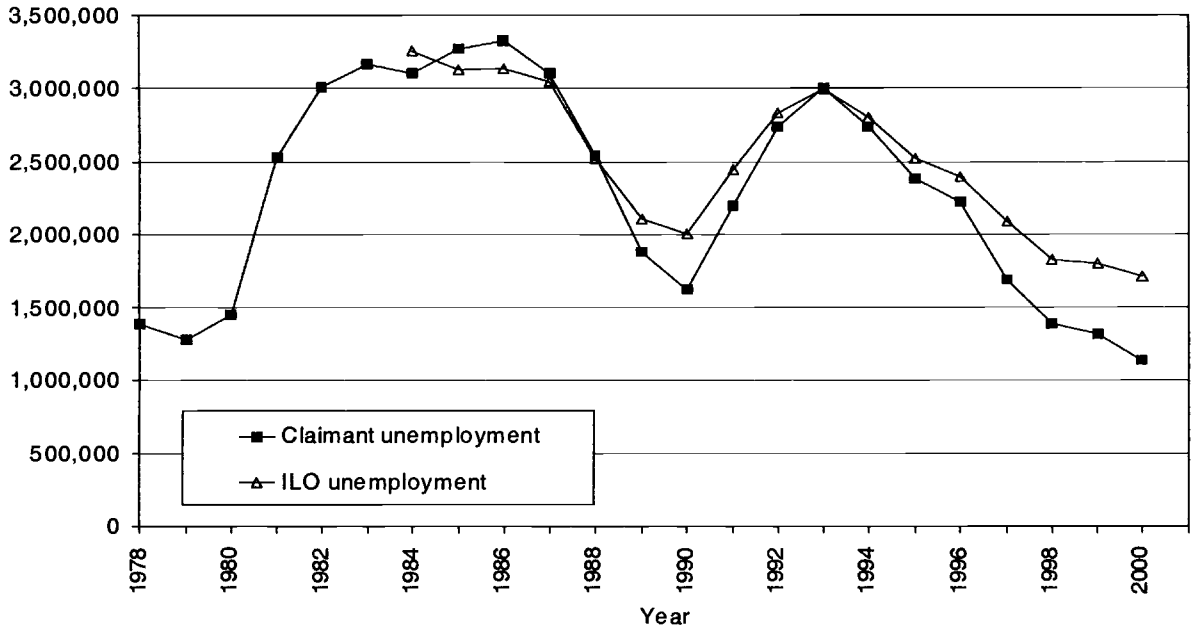

Fig. 11.2 Unemployment—claimant and ILO measures

Sources: Data on ILO defined unemployment were taken from the LFS. Data underlying the figure is available at http://www.statistics.gov.uk and Wells (2000).

Notes: The ILO definition is based on asking out of work individuals whether they would be available and prepared to accept a job within two weeks. The claimant count is the number of people who are receiving unemployment benefits (called *Job Seekers Allowance* since 1994). Although the series track each other relatively well, there will be some people who are ILO unemployed who will not be in the claimant count (e.g., if they left their job voluntarily, this will disqualify them from benefits receipt for a period of time). Similarly, some individuals could be claiming unemployment benefits without genuinely searching for a job.

Table 11.1 **Benefits, Sanctions, and Unemployment: International Comparisons (%)**

Country	ILO Unemployment Rate (c. 1999) (1)	Replacement Rate (c. 1997–1998) (2)	Sanction Rate (1994–1995) (3)
Australia	7.5	71	14.7
Belgium	9.1	61	4.2
Canada	8.1	66	6.1
Denmark	4.8	80	4.3
France	11.2	n.a.	n.a.
Finland	10.7	81	10.2
West Germany	7.4	79	1.1
Italy	10.0	n.a.	n.a.
Japan	4.7	59	0.02
The Netherlands	3.4	82	36
Norway	2.9	73	10.8
Sweden	7.3	85	0.8
Switzerland	1.8	84	40.3
United Kingdom	6.2	67	10.3
United States	4.3	60	25.7

Sources: International Labor Organization (ILO) unemployment rate 1999 from Nickell and van Ours (2000). Replacement rate from Martin (1998, table 4). Sanction rate from Grubb (2000), except for the Netherlands (Boone and van Ours 2000) and Sweden.

Notes: Replacement rate calculated as benefit entitlements before tax as a percentage of previous earnings before tax; first month of unemployment for a person on average earnings, assuming that person is forty years old, has a dependent spouse, has two children, and started work at eighteen. These are all 1994–1995 (except for Japan, 1996). Sanctions rate is defined as total sanctions during benefit periods as a proportion of the average stock of claims 1997–1998. n.a. = not available.

unemployment for prime-age individuals. There is a relatively high proportion of young Britons in jobs and a low proportion of young people in school. There is also a large proportion of British youth that are neither in school nor in the labor force (the "idle"). The United Kingdom has the highest numbers of eighteen-year-old men in this category.[3] Moreover, the United Kingdom has had the largest increase in the proportion of "idle" youth since 1984.

Another feature of the youth labor market is its sensitivity to the business cycle. The unemployment rates of the younger group (see figure 11.3) broadly mirror the overall picture, but are more cyclically sensitive. This is also true for the employment rates (see Bell, Blundell, and Van Reenen 1999).

Turning to wage rates, it is well known that there has been a large increase in earnings inequality in the United Kingdom since 1979 (Schmitt,

3. The proportion idle was 8.4 percent in the United Kingdom in 1997, compared to 2.3 percent in 1984. In 1997 the OECD average was 1.8 percent—5.6 percent in the United States, 4.2 percent in Germany, 3.3 percent in France, and 9.1 percent in Italy (see Blanchflower and Freeman 2000).

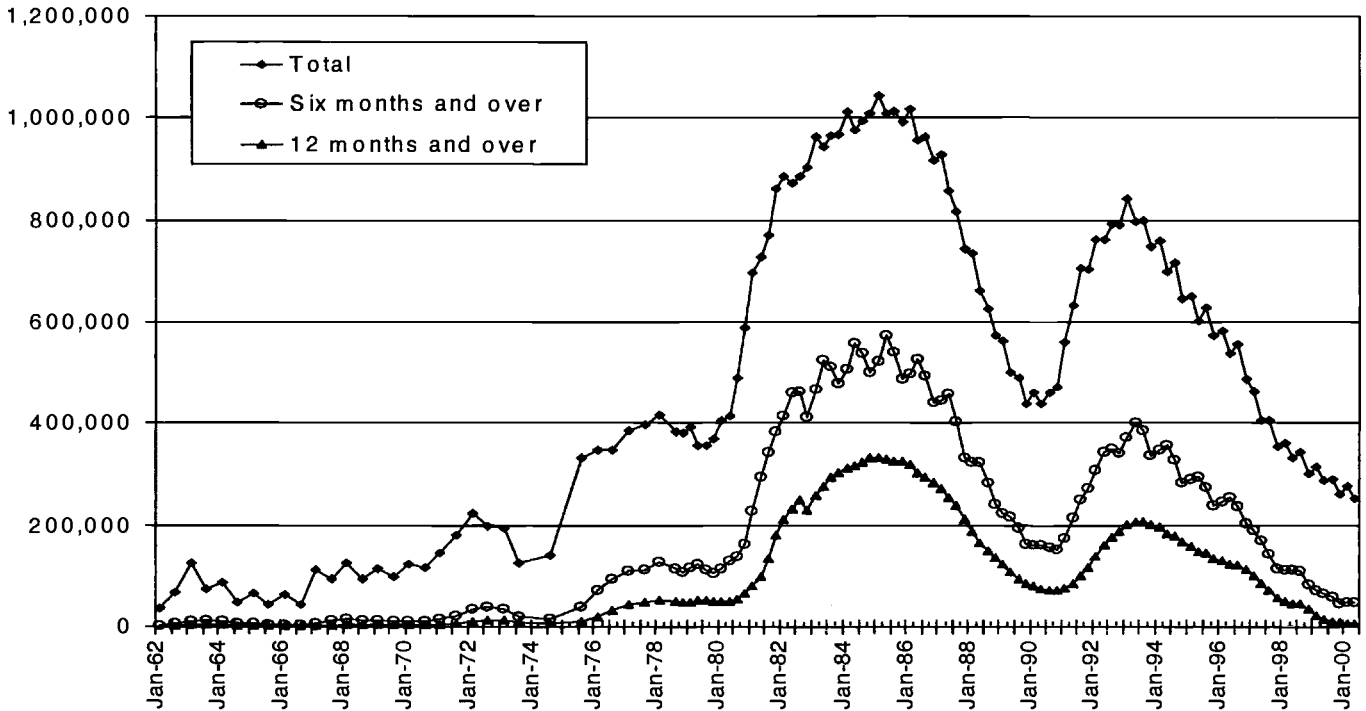

Fig. 11.3 Claimant unemployment amongst eighteen–twenty-four-year-olds

Source: Data underlying the figure is available at http://www.statistics.gov.uk and Wells (2000).

1995; Gosling, Machin, and Meghir 2000). This has occurred between the younger and older age groups, even within gender and skill classes. The uprating of many benefit levels (e.g., the state pension and unemployment benefit) in Britain was pegged to price inflation instead of earnings inflation from 1979–1980. Also, there were real cuts of about 10 percent in most means-tested benefits. This has lead to a fall in the replacement rate relative to other countries (the second column of table 11.1).

To summarize this section somewhat boldly, the United Kingdom has enjoyed lower than average unemployment rates since the mid- to late 1990s than other European countries. Over a longer time frame unemployment has been about average and more volatile than other OECD countries. Youth unemployment is slightly better than average, but there are a surprisingly large number of young people in the United Kingdom who are neither in school nor actively seeking work. Wages are relatively low for young British workers, but so are unemployment benefits.

11.3 Institutions of U.K. Unemployment Benefit Regime

11.3.1 The New Deal in Historical Context

Table 11.2 displays the evolution of the U.K. unemployment benefit regime. The network of the labor exchanges was first founded in 1910 and administered the first unemployment benefits from 1912. Worries about the disincentive effects of unemployment benefit persisted from its foundation. The work test was a fundamental part of the labor exchange but was seen as more humane than the workhouses that preceded it. As Beveridge (1909) put it

> The labour exchange opens the way for "depauperisation" more humane, less costly and more effective than that of the workhouse test—the way of making the finding of work easy instead of making relief hard. (215–216)

The work-test requirement became less pressing during the postwar "Golden Years" of low unemployment. From the late 1960s, however, there appeared to be a shift in attitude toward unemployment benefits away from being a temporary palliative of social insurance and toward being a more permanent redistribution from those with work to the jobless. Additionally, the role of the Employment Service was reoriented toward being a service provider to employers and employees. It attempted to regain a share in the market for filling vacancies because of the fear that employers were losing interest in notifying job centers of vacancies. The Employment Service focused less on finding jobs for the difficult to place long-term unemployed.

The most important consequence of these changes was that the

Table 11.2 **Timeline of U.K. Unemployment Benefit Reforms**

Year	Reform
1910, February	Labor exchange network founded by Winston Churchill
1912	Unemployment benefit introduced and administered by labor exchanges
1919	All claimants had to prove "normally in employment, genuinely seeking employment and unable to obtain it"
1946	National Insurance Act
1961	Visit Job Center once a week (twice a week before)
1974	Benefit Office and Job Center split
1979	13% cut in Employment Service staff
1980	Visit Job Center only once every two weeks
1982	Visiting Job Center voluntary
1982–1985	50% cutback in numbers of staff to enforce work search (fall of 940 to 550 in Unemployment Registration Office)
1986 Restart	Restart mandatory job-related interview; increases in staff (especially for checking fraud); vacancies displayed in benefit office; verification letters sent to unemployed; maximum period of benefit disqualification extended to 13 weeks (was 6 weeks 1913–1986) January—pilots; July—nationwide for those with 1+ year unemployment; October—extended to all with 6 months unemployment
1988	Maximum period of benefit disqualification extended to 26 weeks
1989 Social Security Act	Eligibility requirements increased over "actively seeking work" (must look every week); cannot refuse "unsuitable" jobs paying less than going rate
1990	Employment Service given more independence by being made into an "arm's-length agency"; performance targets (e.g., on referrals)
1991	Mandatory one-week job course for unemployed >2 years
1994/1995	"Stricter benefit regime" doubles number of sanctions/referrals
1996 Job Seekers Allowance	Job Seekers' Allowance (JSA) is the new legal framework based around Job Seekers Agreement: Visit Job Center once every two weeks; more random checking over search; after 3 months unemployment have to search for other occupations
1997	Various compulsory programs (1-2-1, Workwise, Project Work)
1998 New Deal	New Deal for Young People (pilots in January, nation rollout in April)
1998	New Deal for long-term unemployed—all those unemployed for over 2 years (July)
1999, April	National Minimum Wage introduced at £3.60 for adults and at £3 for youths
1999, August	New Deal for over-50s piloted
2000	New Deal for over-50s national rollout
2001	New Deal made a permanent feature of U.K. unemployment benefit regime

Sources: Wells (2000), Price (2000).

Note: Important reforms are italicized.

work-search requirements were less strictly enforced. The function of job centers (job search) and benefit offices (paying benefits) were split in 1974, and they were increasingly located on different premises. An indicator of the relaxation in work search is the number of referrals of unemployed people suspected of not searching for work—this stood at 28,270 in 1968, and, despite a big increase in unemployment, fell to 5,603 by 1976 (Price 2000).

Surprisingly, the advent of Mrs. Thatcher's administration actually reinforced this trend. In 1982 the compulsion to visit a Job Centre if someone claimed unemployment benefit was withdrawn. Cutbacks in public expenditure reduced the numbers of staff to monitor the work-search requirements and help match the unemployed with jobs.[4] These administrative changes, combined with the huge increase in unemployment in the early 1980s (see figure 11.1), swamped the ability of the Employment Service to enforce work search. In terms of gross domestic product (GDP) the U.K. recession troughed in 1981, but despite 5 years of recovery, claimant unemployment only peaked in 1986.

A major period of benefit reform began in 1986. The introduction of the Restart program made interviews with the Employment Service a condition of benefit receipt for all those whose unemployment claims had reached a duration of twelve months or more. These were piloted in January and rolled out nationally in July. Also, in 1986 the government extended the unemployment insurance disqualification period for those deemed to have left their jobs voluntarily from six to thirteen weeks (this was further increased to twenty-six weeks in 1988 and is currently six months).[5]

Since this point, there has been a successive tightening of the work-search requirement. In October 1986 Restart interviews were extended to all those unemployed in excess of six months, with repeated interviews after every subsequent six months. In 1991 mandatory job courses for the very long-term unemployed were introduced. In 1994 the number of sanctions doubled under the "stricter benefit regime."

These changes were consolidated in a new legal framework under JSA, introduced in 1996. A range of measures was introduced to improve job search,[6] and there were more checks over eligibility.

One indicator of the effect of these cumulative changes has been to reduce

4. In the early 1980s large numbers were encouraged to leave the unemployment rolls and draw other forms of benefits (and therefore exit the labor market). This gave the appearance of reducing unemployment. For example, the 1983 budget allowed men over sixty to move on to a higher benefit rate if they signed off of unemployment benefits and on to long-term supplementary benefits. Supplementary benefits *required* that the recipients did *not* look for work (Wells 2000)! The numbers of invalidity benefits rose by 300,000 between 1984 and 1988.

5. Restart also gave menu of options to help get people into work—short courses, training, job clubs, and a Jobstart subsidy (a £20 bonus to the unemployed person if they took a low-paying job).

6. Examples are the Jobseekers' agreements, the enhanced advisory interventions, and that the unemployed could not refuse jobs outside their own occupation after three months, and so on.

the proportion of people seeking work who actually claim benefits. Schmitt and Wadsworth (1999) show that in 1983 90 percent of ILO unemployed men (i.e., those who had actively sought work within the last two weeks) received unemployment benefits, compared to only 80 percent in 1993.

Previous U.K. governments had experimented with wage subsidies. The common feature of these schemes was the payment of a fixed weekly subsidy, typically of around £50 or £60 for the initial months of employment of a long-term unemployed individual. This was sometimes payable to the individual (Jobstart allowance and Jobmatch) and sometimes to the employer (Workstart). In addition, an employer's National Insurance (the main U.K. payroll tax) contributions holiday for the long-term unemployed was introduced in April 1996. Prior to the New Deal none of these schemes were very well funded, and all have suffered from low take-up. For example, in 1996 only 1 percent of all U.K. active labor market funds were spent on wage subsidies, compared to an European Union (EU) average of 10 percent (Martin 1998).

11.3.2 The Elements of the New Deal

The New Deal program has been targeted at specific groups of the unemployed, with an emphasis on the young (eighteen- to twenty-four-year-olds), long-term unemployed (eighteen months or more), lone parents, and disabled people. Pilots for the New Deal for Young People began in January 1998, and the program took effect at the national level beginning in April 1998. The number of young people on the New Deal peaked at just fewer than 150,000 in July 1999 and stood at 86,200 in September 2002.

The windfall tax on the privatized utilities raised £5.2 billion between 1997 and 1999, and all of these funds were hypothecated to financing New Deals of some variety. Table 11.3 shows the government's estimates of the allocation of windfall tax receipts to different elements of the program. The New Deal for Young People received about £1.5 billion by the end of March 2002.

It is tempting to simply divide the cost of the New Deal by the estimate number of new jobs in section 11.4 (about 17,250) to find a "cost per job created." This would imply that the scheme was expensive (e.g., using the estimates in section 11.4 of about £18,550 per job in 1999–2000). Such a calculation is misleading, however, as participants of the New Deal options would have been claiming JSA, and these costs (and others) must be deducted from the gross costs in table 11.4 to get an estimate of the net exchequer cost. We perform an explicit cost-benefit calculation in section 11.5 to address this issue. This suggests that the actual social cost per additional employee is under £4,000 (£68.1 million/17,250) and, more importantly, that social benefits exceeded social costs.

The program is composed of several parts, with different options offered to different groups of the unemployed. The New Deal for Young People is

Table 11.3 **Allocation of the Windfall Tax, 1997–1998 to 2001–2002 (spending by program in £millions)**

Program	1997–1998	1998–1999	1999–2000	2000–2001	2001–2002	1997–2002
New Deal for 18- to 24-year-olds	50	210	320	440	460	1,480
New Deal for those 25 and over	0	10	110	160	320	600
New Deal for those 50 and over	0	0	0	20	20	40
New Deal for single parents	0	20	50	60	90	220
New Deal for disabled people	0	10	30	90	80	210
New Deal for partners of unemployed people	0	0	10	20	20	50
New Deal for schools	90	270	330	580	310	1,590
Child care	0	20	10	0	0	40
University for Industry	0	5	0	0	0	5
ONE pilots	0	0	0	5	5	10
Action teams				20	20	40
Enterprise development	0	0	0	20	10	30
Total expenditure	140	550	850	1,420	1,340	4,300
Unallocated						900
Windfall tax receipts	2,600	2,600				5,200

Note: ONE indicates "ONE stop gateway."

compulsory for all those aged eighteen–twenty-four who have been receiving the JSA for more than six months. Figure 11.4 summarizes the treatment in a flow diagram. Initially, individuals enter a Gateway period, where they are assigned a personal adviser who gives them extensive assistance with the job search. If the unemployed person is still on JSA at the end of the Gateway period (formally, a *maximum* of four months),[7] they are offered up to four options:

1. Entry into full-time education or training for up to twelve months for those without basic qualifications (without loss of benefits);
2. A job for six months with a voluntary sector employer (paid a wage or allowance at least equal to JSA plus £400 spread over the six months);
3. A job with the Environmental Task Force (paid a wage or allowance at least equal to JSA plus £400 spread over the six months); or
4. A subsidy to a prospective employer for six months, with training for at least one day per week (£60 per week plus an additional £750 training subsidy spread over the six months).

7. In practice the Gateway period can last for longer than the official maximum of four months.

Table 11.4 **Level and Composition of Active Labour Market Policies (ALMPs) in the Group of Seven between 1985 and 1996**

	Spending on ALMP as a % of GDP (1)		Spending on ALMP per Person Unemployed[a] (2)		Spending on Public Employment Service as a % of All ALMP (3)		Spending on Youth Measures as a % of All ALMP (4)		Spending on Direct Job Creation in the Public Sector as a % of All ALMP (5)	
Country	1985	1996	1985	1996	1985	1996	1985	1996	1985	1996
Canada	0.6	0.5	6.2	5.6	37	36	5	5	3	6
France	0.7	1.3	6.6	10.7	20	12	25	19	n.a.	17
Germany	0.8	1.4	10	16.1	26	17	6	5	15	21
Italy	n.a.	1.1	n.a.	9	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.
Japan	0.2[b]	0.1	5.8[b]	3	17[b]	26	0[b]	0	6[b]	2
United Kingdom	0.7	0.4	6.4	5	22	43	35	26	25	2
United States	0.3	0.2	3.8	3.2	25	39	12	15	3	3
European Union[c]	0.9	0.9	13.3	11.3	19	19	14	15	16	15
OECD[d]	0.7	0.7	13	11.4	21	21	11	12	17	14

Source: Martin (1998), tables 1, 2, and 5.

Notes: n.a. = not available. ALMPs include public employment service, youth measures, public-sector job creation, labor market training (for employed and unemployed adults), wage subsidies to private-sector employment, and measures for the disabled (last three items not shown in table).

[a]ALMP per person unemployed is normalized on output per head. Normalizing on productivity is in order to control for the fact that more productive countries will have higher wages, so the figure is comparable to a "replacement rate." This indicator is commonly used by the OECD and in the cross-country analysis of Layard, Nickell, and Jackman (1991) and others.

[b]1987.

[c]Unweighted average.

[d]Unweighted average excluding Czech Republic, Hungary, and Poland.

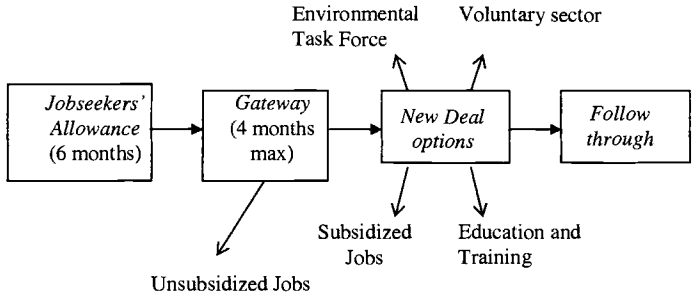

Fig. 11.4 A simplified flow diagram of the New Deal program

Notes: The New Deal for Young People is a mandatory Welfare to Work program. All young people (between the ages of eighteen and twenty-four) who had claimed unemployment insurance (JSA) for six months enter the program. During a Gateway period of at most four months participants are given extensive job-search assistance. Those failing to find an unsubsidized job have four different options: entering employment with a six-month wage subsidy to the employer, twelve-months full-time education or training, working in the Environmental Taskforce (a public-sector job), or working in the voluntary sector. The individual faces the withdrawal of unemployment benefits if they do not cooperate. After the end of the options, participants who rejoin the unemployed enter the "Follow Through," which is essentially the same as the Gateway. For more details, see DFEE (1997).

If an option is refused, the claimant is liable to suffer a benefits sanction. Initially, sanctions take the form of withdrawal of benefits for two weeks, and further refusals may result in repeated benefit sanctions. Individuals returning to unemployment within thirteen weeks after leaving an option go onto the "follow-through" program of job assistance, which is essentially the same as the Gateway.

Individuals can enter options at any time after the sixth month of JSA. The official guidance was that the first month of the Gateway was confined to unsubsidized employment. The second month would then focus on education and training and the third month on the subsidized job option. The public-employment option was only to be used as a last resort in the fourth month. In practice this schema was not rigidly adhered to.

Out of the four options, education and training has been the most popular (about 40 percent of all those who had joined the New Deal options by the end of April 1999 chose education and training). The employer's option had a much lower take-up than anticipated (only 20 percent of all those in options). The reasons for this low take-up is uncertain, but it is worth noting that low take-up has often been a problem for wage-subsidy schemes in other countries. Possible reasons include the following: (1) The U.K. economy was in a prolonged expansion. The crop of unemployed who fail to get unsubsidized jobs even after the Gateway period may have very poor basic skills, making them very unattractive to employers; (2) the requirement to have formal training may impose high costs on employers;

(3) the Employment Service has had little experience in handling job-subsidy schemes; and (4) the failure to secure a job during the Gateway period might generate a stigma effect on the individuals.

11.3.3 The Effectiveness of Previous U.K. Reforms

In general there has been a paucity of high quality evaluations of U.K. labor market reforms, compared with the United States. Random assignment has encountered serious opposition due to a mixture of political and ethical objections. In this respect the United Kingdom is more typical of other European countries.[8] "Evaluations" usually take the form of surveys of participants. Obviously, there is no obvious comparison group, so any counterfactual is purely speculative.

There are several macroeconometric evaluations where the policy is essentially indicated by a set of time dummies. For example, using a time series model the Employment Service (Sweeney and McMahon 1998) claimed that the rule changes in JSA reduced claimant unemployment by about 15,000 to 20,000. Large positive effects of Restart were found by Dicks and Hatch (1989) and Disney et al. (1991). More recently Riley and Young (2001a,b) found moderate effects of the New Deal for Young People when they used a macro approach (about 28,000 extra jobs).[9]

The major problem with these macro approaches is that (aside from conventional aggregation biases) there are many other macroeconomic events occurring simultaneously with the introduction of labor market programs. It is extremely difficult to disentangle the program effect from these macro shocks. For example, the U.K. economy experienced a very sharp upturn in the late 1980s (see figure 11.2) that would have raised employment in the absence of the Restart program.

Fortunately, however, there have been useful microeconometric evaluations of the Restart initiative using microdata. In 1989 a sample of just under 9,000 individuals were identified who were approaching their sixth month of unemployment. A random control group of 582 were selected who were not obliged to take part in the Restart interview. These individuals were followed up in surveys three months and nine months after their first Restart interview (or nine months and thirteen months after the start

8. Martin (1998, 14) recounts one leading European policy maker's frank explanation for this absence of good evaluations: "Most of our programs are lousy! They were dreamed up quickly to give the Minister some good news to announce at a time when unemployment is rising. We do not want evaluations revealing to the general public how bad our programs are; we know this already."

9. Disney et al. (1991) used the ratio of Restart interviews to eligible participants; Anderton, Riley, and Young (1999) and Riley and Young (2001b) use the number of New Deal participants as a share of the claimant count. Riley and Young (2001a) multiply the latter variable by the average number of days that individuals receive personal advisor interviews as their indicator of "New Deal intensity."

of their benefit claim, in the case of the control group). This information was matched to administrative records using their unique National Insurance (U.K. Social Security) numbers.

Dolton and O'Neill (1995, 1996)[10] analyze this data and find that the group who were randomized out of Restart had median unemployment duration one month longer than those who did receive Restart. They also examined the destinations of those leaving the claimant count using a competing risks model. They found that the strongest effects of Restart came from exits into jobs rather than exits to training or nonparticipation. This Restart effect appeared to work through both increasing the arrival rate of job offers and by making the treated group more likely to accept a job if they received an offer.

Some of the job-subsidy schemes have been evaluated, but usually through surveys without a good comparison group. One analysis of the Workstart pilots (where firms received a wage subsidy for employing the long-term unemployed) concluded that only 17 percent of the Workstart vacancies represented new employment that would not have existed without the subsidy and that much of the employment of the long-term unemployed occurred at the expense of the shorter-term unemployed (Atkinson and Meager 1994).

11.3.4 The United Kingdom in International Context

The OECD estimates of spending on active labor market policies (ALMP) across countries between 1985 and 1996 are given in table 11.4. These include administration of the public employment service (a rough proxy for resources in job assistance and job search monitoring), youth measures, training, public-sector job creation, subsidized private-sector jobs, and job help for the disabled. The first column simply gives ALMP as a proportion of GDP. This might be seen as misleading as some countries will have different unemployment and wage levels than others. To partially deal with this, column (2) presents the standard OECD measure of ALMP per person unemployed normalized on output per head. The figure can be regarded as analogous to a replacement rate with the generosity of spending on ALMP per client compared to the outside wage (as proxied by output per head).[11]

Despite the rhetoric, there has not been a rise in this measure of ALMP and GDP in the OECD as a whole over this period on either measure. If

10. White and Leakey (1992), using the same data, also found that Restart significantly reduced unemployment duration, increased the probability of finding a job, and increased the probability of moving into employment training. They could find no evidence that Restart reduced the average quality of a job match either through lower wages or deteriorating job length. Surprisingly, however, there was no evidence that this effect was driven by increased job search (as measured by number of job applications, etc.).

11. This variable is used in the cross-country comparisons of Layard, Nickell, and Jackman (1991) among others.

anything, there has been a slight decline. There is a clear difference between the main continental European countries and the United Kingdom. France and Germany both have higher spending per unemployed person, and both have increased their ALMP intensity. The United Kingdom not only spent less but also saw a decline over this period. Britain appears closer to the United States, Japan, and Canada in this respect.

The next three columns of table 11.4 decompose the spending of ALMP into its three main components—public employment service, youth programs, and government jobs. The toughening of work search requirements in the United Kingdom is indicated by column [3]—there has been a large increase in the proportion of resources devoted to the public employment service, from 22 percent in 1985 to 43 percent in 1996. In contrast there have been large falls in the proportionate spending on job creation in the public sector (from 25 percent to 2 percent) and to a lesser extent in youth programs (from 35 percent to 26 percent).

The picture is different in continental Europe. Both France and Germany spend a larger proportion of resources on state job creation, and their proportionate spending on the public employment service has fallen between 1985 and 1996. The trends are also in the opposite direction of the United Kingdom. Again, the United Kingdom is closer to the United States, who has also increased the resources going to public employment services (25 percent to 39 percent). Unfortunately, comparative data for those leaving the New Deal options are not yet available. Overall spending on ALMP in the United Kingdom has almost certainly increased after 1997 and is weighted more toward the youth component today than in 1996.

The level of U.K. unemployment benefits is low compared to most other European countries (table 11.1). Offsetting this is the fact that the sanctions regime in the United Kingdom is only about average by international standards. The Netherlands, for example, has also managed to lower unemployment in the 1990s but, unlike the United Kingdom, has maintained a high replacement rate. The Dutch introduced a very tough sanctions regime, however, to offset the disincentive effects of high replacement rates (see Nickell and van Ours, 2000, for a discussion).

11.3.5 What Elements of the New Deal Might Work? Evidence from U.S. Research

The New Deal Gateway provides both job-search assistance and job-search monitoring (with accompanying sanctions for noncompliance). The study reported in section 11.4 identifies a treatment effect over and above the impact of employment subsidy, but is this effect due to the "carrot" of mentoring from the personal adviser or the "stick" of a harsher benefit regime?

There is a large literature on the evaluation of the U.S. unemployment insurance (UI) system. The Social Security Act of 1935 created the UI system.

Each state administers and implements its own system subject to federal guidelines. In particular, there is a requirement for work-search monitoring, although the precise way this is implemented varies by state. There have been several demonstration projects (randomized trials) investigating the impact of variation in the way job-search monitoring and assistance impacts on the duration of claims and recipients' employment and earnings.

Meyer (1995) offers an excellent survey of five experiments.[12] He finds evidence that job-search monitoring and assistance together significantly reduce the duration of claims. There were less clear-cut effects on overall earnings. Unfortunately "[t]his combination of additional services and tightened eligibility checks makes it difficult to determine what aspects of the experiments induced the changes in outcomes . . ." (Meyer 1995, 114). Ashenfelter, Ashmore, and Deschenes (1999) attempt to tackle this problem by analyzing four experiments where the treatment of stricter enforcement and verification of work-search behavior can be separated from job assistance.[13] They find no significant effects on claim duration from tighter monitoring.

Anderson (2000) reports three further recent experiments.[14] Like Ashenfelter, Ashmore, and Deschenes (1999), the Maryland study allows a distinction between job assistance and monitoring. The most stringent monitoring method, wherein each of the normal two contacts per week were verified, resulted in a fall in duration of 10 percent (about 1.5 weeks), compared to dropping of all job-search monitoring. The reason for the differences between the two studies appears to be because the treatments in Ashenfelter, Ashmore, and Deschenes (1999) were relatively weak. The tougher monitoring of the job search was only in the first week after submitting a claim for UI but *before* the claim was accepted.

There have been several recent studies of the impact of reforms to the benefit regime in the Dutch labor market. Abbring, van den Berg, and van Ours (1997) and Van den Berg, van der Klaauw, and van Ours (1998) estimate that job-finding rates double after the imposition of a sanction. Van den Berg and van der Klaauw (2001) could find no effect of counseling and monitoring, but they argue that these interventions provided little significant job-search assistance to the unemployed and were targeted mainly at groups who had relatively good labor market prospects. Gorter and Kalb (1996) found that more intensive counseling to an eligible group with worse prospects had a significant impact on exits to employment.

12. The experiments include the Nevada Claimant Placement Program, the Charleston Claimant Placement and Work Test Demonstration, the Wisconsin Eligibility Review Pilot Project, the New Jersey UI Reemployment Demonstration, and the Washington Alternative Work Search Experimentation.

13. These sites were in Connecticut, Massachusetts, Virginia, and Tennessee.

14. The experiments include the Utah Quality Control Program Improvement Study, the Maryland UI Work Search Demonstration, and the Job Search Assistance Demonstration in Florida and Washington, D.C.

In our U.K. results (section 11.4), we do not find evidence that individuals were dropping off the rolls as they approached the start of the New Deal Gateway (between the fifth and sixth months of an unemployment spell—as we will see in table 11.7). If monitoring were extremely unpleasant, we would have expected more claimants to stop claiming prior to entering the New Deal (as appeared to happen with Restart). There is no significant change in outflows as the New Deal approaches; thus, we are inclined to believe that it is the carrot rather than the stick that has been most effective in delivering employment increases, a view shared by the qualitative evaluations of New Deal participants (e.g., Hasluck, 2000).[15]

There is also a more extensive U.S. literature on the use of wage subsidies. Both Katz (1998) and Dickert-Conlin and Holtz-Eakin (2000) conclude that employer-based subsidies have not proven successful. Katz argues that part of this is due to stigma effects as only the most disadvantaged are typically able to get such subsidies, and this acts as a bad signal to potential employers. This may also explain why take-up rates are usually very low. Katz does find some evidence of an effect of the targeted job tax credit (TJTC) for disadvantaged youth in his own work. In section 11.4 I report results of using a similar methodology to Katz, exploiting the age-eligibility criterion to estimate the effect of the New Deal.

Perhaps the closest experimental evidence for the New Deal is the U.S. Welfare to Work programs. Bloom and Michalopoulos (2001) survey twenty-nine different initiatives that had demonstration projects (random assignments). Eight of these schemes were job-focused (rather than education or training focused) and mandatory for welfare recipients. Although the precise impact effect differed from program to program, a statistically significant effect of the program on employment probabilities was found in all eight cases.

In this paper I do not examine the impact of the training and public-sector job element of the New Deal program due to lack of postoption data.[16] There is a large U.S. literature on the impact of training programs for the unemployed and a rather smaller literature on the impact of public work programs.[17] Generally, the outcomes of evaluations of training programs for young men have been disappointing.[18] It is worth remembering

15. Only 2 percent of participants in the New Deal have suffered sanctions.

16. Bonjour (2001) look at the relative success of different New Deal options using a special survey. They find that eighteen months after entering the New Deal, the employer option had the best outcomes in terms of getting people into work.

17. For a survey of public service employment and mandatory work, see Ellwood and Welty (2000).

18. See the survey in Heckman, Lalonde, and Smith (1999). The main argument is that most of these schemes fail to significantly raise the human capital of participants. A similar conclusion is reached about U.K. public training schemes by Dolton (1992). Previous U.K. training schemes have included Training Opportunities Programs (TOPS), the Youth Training Scheme, Employment Training, and Training for Work.

that the pool of young unemployed men in the United Kingdom is proportionately much larger than in the United States, so there may be greater scope for positive program effects. The U.S. target group of disadvantaged are more likely to be "hardcore" jobless whose human capital is very difficult to raise.

11.4 An Empirical Evaluation of the New Deal for Young People

It is possible to examine the performance of the Gateway period of job assistance using publicly available microdata (for a full analysis, see Blundell, Costa Dias, Meghir, and Van Reenen 2001).[19]

We consider the treatment group to be young people unemployed for six months (continuous claims of JSA). The outcome of greatest interest is the flow of this group into employment over the four months of the Gateway period (months six through ten of JSA). We also examine all outflows from unemployment over the same period (e.g., to training).

The New Deal treatment effect considered (the job outflow by month ten of unemployment) comprises the effects of both the job-assistance and monitoring element of the New Deal *and* the wage-subsidy element.[20] It is possible to estimate a lower bound to the job assistance and subsidy element (unsubsidized jobs) as we know from administrative sources the actual proportion of the unemployed who obtained subsidized jobs (these numbers are presented in the empirical results). By deducting the proportion that flow into the subsidized jobs from the overall treatment effect, one can obtain a lower bound of the pure Gateway effect. The "true" effect of job assistance is likely to be higher as some of those obtaining subsidized jobs would have obtained them even in the absence of a subsidy, despite the best efforts of the Employment Service to minimize this "deadweight."

In the absence of random assignment there are two possible ways to construct the comparison group. The first method is to exploit the fact that the New Deal was piloted in some areas ahead of the National rollout. We compare nineteen–twenty-four-year-olds[21] in the pilot areas (Pathfinders) to similar nineteen–twenty-four-year-olds in nonpilot areas over the same period of time before and after the introduction of the program. The second method is to examine an older age group who are ineligible for the New Deal. We choose to focus on twenty-five–thirty-year-olds who have been

19. This section is based on an analysis of the Joint Unemployment and Vacancies Operating System (JUVOS) data, which contain information over time for a sample of 5 percent of those claiming unemployment-related benefits in the United Kingdom.

20. Originally, I had hoped to deal with this problem by focusing only on the first two months of the New Deal Gateway when no one was supposed to go on the wage-subsidy option. Unfortunately, the New Deal Evaluation Database showed that some people went on the employer option even in the first month of the Gateway.

21. We drop eighteen-year-olds because there has been a large increase in the participation rate in full-time education for this group in recent years.

unemployed for six months as the comparison group. After the national rollout, only the comparison across age groups is possible.

There are many potential biases in using either of these comparison groups. Most pressing is the issue of substitution, that the older unemployed will be less likely to gain employment because firms will prefer New Deal participants (for example, firms receive a subsidy for employing a twenty-four-year-old but not a twenty-five-year-old). Consequently, using the older age group may lead us to *overestimate* the positive effects of the New Deal. The Pathfinder pilots versus the non-Pathfinder pilot comparison should be informative in this regard. Substitution effects imply that we should estimate *smaller* effects when comparing young people in pilot versus nonpilot areas than when we estimate using younger versus older individuals *within* the pilot areas. Unlike the older group, young people within nonpilot areas are unlikely to be adversely affected by the New Deal. Substitution also implies that we should, ceteris paribus, expect to see the outflow rates of the older group decline in the pilot areas (where they are losing out to the younger group) compared to the nonpilot areas.

This discussion illustrates that there is no one obviously "correct" age comparison to consider. Using the regression discontinuity design approach of comparing eligible twenty-four-year-olds with noneligible twenty-five-year-olds has some appeal as we would expect these groups to be very similar in productivity characteristics. Unfortunately, substitution would be most severe for this age comparison because the twenty-five-year-olds would be the closest substitutes for the twenty-four-year-olds. Furthermore, the precision of the estimates falls as we focus on smaller and smaller slices of the data. As a consequence, we believe the five-year age groupings for treatment and control are the best balance, but we were also careful to investigate alternative age cutoffs in the empirical work.

A second issue is that of equilibrium wage effects. If the New Deal reduces equilibrium wage pressure (for example, through increased search) then we will be *underestimating* the effects of the New Deal in increasing employment. The use of different comparison groups may again be informative in this context. Consider the scenario where there are no substitution effects and only equilibrium wage effects in the local labor market. In this case, comparing young people in the pilot versus nonpilot areas will reflect some of the positive job effects associated with reduced wage pressure. Comparing younger versus older people within the Pathfinder areas will not capture the equilibrium wage effects as the job chances of both groups are improved. Thus, equilibrium wage effects imply that we should estimate *larger* effects when comparing young people in pilot versus nonpilot areas than when we estimate using younger versus older individuals *within* the pilot areas. The bias is in the opposite direction of that of the substitution effect.

Whichever comparison group is chosen, the method is to compare the

difference in the outflow rates between these two groups after the New Deal began, compared to the difference in the outflow rates before the New Deal started.

Table 11.5 contains the raw data on the outflow rates to jobs for the different groups. The data is taken from the Joint Unemployment and Vacancies Operating System (JUVOS), an administrative longitudinal database. This follows a random 5 percent of all individuals who have ever claimed unemployment benefits. The upper panel contains data from the pilot period and the lower panel from the national rollout. The pilot period considers those who reached six months on unemployment benefits (JSA) between January 1, 1998 and the end of March 1998 ("after the program"). I follow them four months later (i.e., ten months after they become unem-

Table 11.5 **Flows from the Claimant Count into Employment: Men (conditional on being on Job Seekers Allowance (JSA) for six months)**

	Flows by the End of the 8th Month on JSA			Flows by the End of the 10th Month on JSA		
	Before the Program (1)	After the Program (2)	Difference (3)	Before the Program (4)	After the Program (5)	Difference (6)
Pilot period						
1. Treatment group: 19–24-year olds in Pathfinder areas	0.141	0.180	+0.039	0.241	0.330	+0.089
2. Comparison group: 19–24-year-olds in all other areas	0.165	0.146	–0.019	0.271	0.250	–0.021
3. Difference in differences			+0.058			+0.110
4. Comparison group: 19–24-year-olds in matched non-Pathfinder areas	0.149	0.133	–0.016	0.228	0.233	+0.005
5. Comparison group: 25–30-year-olds in Pathfinder areas	0.150	0.153	+0.003	0.276	0.260	–0.016
National rollout						
6. Treatment group: 19–24-year-olds	0.158	0.170	+0.012	0.258	0.281	+0.023
7. Comparison group: 25–30-year-olds	0.138	0.124	–0.014	0.230	0.199	–0.031
8. Difference in differences			+.026			+0.054

Notes: Estimates used the Joint Unemployment and Vacancies Operating System (JUVOS) 5 percent longitudinal sample of JSA claimants. Selected observations are all unemployed individuals completing a six-month spell on JSA over a predefined time interval. The present table considers those obtaining six months of JSA between the second and fourth quarters of 1997 and 1998 for the national rollout estimates, and the first quarters of 1997 and 1998 for the pilot period estimates. Individuals verifying this criterion are then followed up to the end of the eighth and tenth months on JSA to check whether they have found a job. The eligible group (defined by the age or pilot area criterion) is compared with the selected control group.

ployed). This group is compared with the same age group who reached six months on unemployment benefits between January and March 1997 ("before the program"). The national rollout considers individuals who reached six months on unemployment benefits between April 1, 1998 and December 31, 1998 ("after the program"). They are compared with the same age group between April and December 1997 ("before the program").[22]

Focusing on the flows between six and ten months in the pilot period (row 1 column [6] in table 11.5), we can see that nineteen–twenty-four-year-olds were 8.9 percentage points more likely to obtain jobs in the post-New Deal period. In the nonpilot areas (row 2) nineteen–twenty-four-year-olds were actually less likely to get jobs (a fall of 2.1 percentage points). So the difference-in-differences effect is a full 11 percentage points (row 3)—an extremely large increase on a pretreatment base of 24.1 percent. The next two rows compare different possible comparison groups: "matched Pathfinder areas" (where we select areas with similar characteristics to the Pathfinder areas) and twenty-five–thirty-year-olds in the Pathfinder areas. The implied difference-in-differences effects are similar to the first comparison group. The lower panel of table 11.5 examines data from the National rollout (post-April 1998). The magnitude of the New Deal effect is still positive but about half the size of that estimated for Pathfinder areas. There is an increase of 5.4 percentage points compared to the preprogram base of 25.8 percentage points (a 5.4/25.8 = 20 percent increase in the outflow rate). We show in the following that this is due to the return to a big "impact" effect in the first quarter that the New Deal is introduced.

The raw difference-in-differences estimates in table 11.5 do not correct for compositional changes. These may be important if the composition of the groups changes systematically over time. In table 11.6 we include a set of extra controls—marital status, sought occupation, region, the number of past unemployment spells, and the proportion of time spent unemployed in the previous two years. The final column contains our main results. In row one we compare young people in pilot and nonpilot areas. In row two we compare younger people to older people within the pilot areas. The results are almost identical to the raw difference-in-differences estimates in table 11.5. The fact that the point estimates are both about 10–11 percentage points regardless of whether we use area or age as the comparison group is interesting. It implies that we cannot reject a simple model where there are no substitution or equilibrium wage effects of the program.[23] It is reassuring that row three shows that the trends for the older

22. An advantage of ceasing to examine any outflows after April 1999 is that the National Minimum Wage was first introduced in April 1999. Minimum-wage effects in analyses that cover this later period may confound the New Deal effects.

23. It is also consistent with a more complex model where both of these effects cancel each other out. It may be, of course, that these effects take longer to play out due to lags of adjustment.

Table 11.6 **Regression Results for Gateway Employment Effects by the End of the 10th Month: Men (conditional on being on Job Seekers Allowance [JSA] for six months; percentage point increase in the probability of leaving unemployment)**

Treatment Group	Comparison Group	No. of Observations	Estimates Based on Difference-in-Difference Method
		Pilot Period	
1. 19–24-year-olds living in Pathfinder areas	19–24-year-olds living in all non-Pathfinder areas	3,716	0.110** (0.039)
2. 19–24-year-olds living in Pathfinder areas	25–30-year-olds living in Pathfinder areas	1,096	0.104* (0.055)
3. 25–30-year-olds living in Pathfinder areas	25–30-year-olds living in all other areas	3,180	0.016 (0.042)
4. 19–24-year-olds living in Pathfinder areas	31–40-year-olds living in Pathfinder areas	1,169	0.159** (0.050)
5. 19–24-year-olds living in Pathfinder areas	19–24-year-olds living in matched non-Pathfinder areas	1,193	0.134** (0.053)
6. Outflow into the employment option (affecting 19–24-year-olds living in Pathfinder areas)[a]		4,486	0.057
	Overall Effect for the Sample including the Pilot Period and the National Rollout[b]		
7. 19–24-year-olds	25–30-year-olds	17,433	0.053** (0.013)
8. Outflows to subsidized jobs[a]		55,051	0.039
	Decomposition of New Deal into First Quarter and Second/Third Quarter Effects		
9. Effect for the pilot period—1st quarter the program operates in Pathfinder areas		1,096	0.104* (0.055)
10. Effect for the 1st quarter the program operates in non-Pathfinder areas		5,169	0.088** (0.025)
11. Effect for the 2nd and 3rd quarters the program operates in all areas		11,161	0.031* (0.016)

Source: Blundell, Costa Dias, Meghir, and Van Reenen (2001).

Notes: Standard errors are in parentheses. Estimates use the Joint Unemployment and Vacancies Operating System (JUVOS) 5 percent longitudinal sample of JSA claimants. Selected observations are all unemployed individuals completing a 6-month spell on JSA over a predefined time interval. This table considers those obtaining six months of JSA between the second and fourth quarters of 1997 and 1998 for the national rollout estimates. The first quarters of 1997 and 1998 are used for the pilot period estimates. Individuals verifying this criterion are then followed up to the end of the tenth month on JSA to check whether they have found a job. The eligible group (defined by the age or pilot area criterion) is compared with the selected control group.

Table 11.6 (continued)

All estimates from regressions include a set of other controls, namely marital status, sought occupation, region, and some information on the labor market history (comprising the number of JSA spells since 1982 and the proportion of time on JSA over the two years that precede the start of the present unemployment spell).

[a]Estimates of the outflows to options are obtained from the New Deal Evaluation Database (NDED).

[b]For the first three quarters, the New Deal is operating in each region.

[c]The decompositions are based on allowing the New Deal effect to differ in the first quarter it was introduced (January through March 1998 for the pilot period and March through May for the national rollout) from subsequent quarters.

**Significant at the 0.05 level.

*Significant at the 0.10 level.

groups were statistically identical in Pathfinder areas to non-Pathfinder areas. If young people were being substituted for older age groups in the pilot areas, one would have expected worse outcomes for the twenty-five–thirty-year-olds in the pilot areas. This does not appear to be the case. We compare the young unemployed to a slightly older age group (thirty-one–forty) in row four and to matched non-Pathfinder areas in row five. These results show (if anything) a slightly larger New Deal effect.[24]

Note that 5.7 percent of the sample joined the subsidized job option during the program (row 6). This enables us to put a lower bound on the effect of the job-assistance element of the program of about 5.3 percentage points (i.e., 11 percent–5.7 percent). Even if none of those who were given subsidized jobs would have obtained them in the absence of the program, there remains a 5.3 percentage point outflow into unsubsidized jobs attributable to the New Deal.[25] If half of all subsidized jobs are deadweight, then the effect of job assistance and monitoring rises to 8.15 percentage points (11 percent–2.85 percent).

The final three rows of table 11.5 examine the "program introduction" effect. It is noticeable that the employment impact of the New Deal was greater in the first quarter that it was introduced (both in the pilot areas and the nonpilot areas after the national rollout) than in the subsequent two quarters. Comparing rows eight and nine (first quarter) with row ten (second and third quarter) illustrates that the program introduction effect appears twice as large as the subsequent impact. Other U.K. labor market programs have also experienced "cleaning out the register" impact effects. But these are usually thought to stem from improved administrative pro-

24. Other studies have also failed to uncover significant substitution effects in the New Deal program (e.g., Anderton, Riley, and Young 1999; Riley and Young 2001a).

25. The design of the program emphasized finding unsubsidized employment when participants first entered the New Deal. So the true effect of job assistance may be close to this lower bound. The greater impact of some U.S. and Dutch assistance schemes may have been because the target group had been employed for shorter periods of time.

cedures and reductions in fraud. It is more likely that the impact effect of the New Deal came from the energizing of personal advisers in the Employment Service who greeted the New Deal with a lot of enthusiasm. This naturally diminishes over time. It would be unwise, however, to consider the post–first quarter lower figure as the "steady state" effect as it is still based on only six months of data.

There are many criticisms of these results that are partially taken up in table 11.7. First, we examined whether the quality of job matches had deteriorated by using the outflow to jobs that lasted at least thirteen weeks as the outcome variable.[26] The treatment effect is very close to that for all jobs, so there is no evidence that New Deal jobs are of significantly lower quality, on this measure at least. Second, it may be that individuals are delaying their exits from unemployment prior to the New Deal in order to take advantage of the generosity of the program. If this was the case, one would expect to see a decline in outflows in the month before the program starts. The third row of table 11.7 shows that there are no selectivity effects between month five and six of JSA (we could also find no New Deal effects on earlier months of JSA).

The third experiment we consider uses outflows to all destinations as the outcome variable (row 4). The New Deal effect is much larger—double the effect on employment. But this is to be expected as a much larger proportion of individuals flow onto some kind of option (13.7 percent of the sample in row 5). Also, the baseline proportion exiting to all destinations is much higher than to jobs alone.

The analysis focuses on men because three-quarters of all New Deal participants are male. We also found that the pre-New Deal outflow behavior of twenty-five–thirty-year-old women was trending in a very different way from that of nineteen–twenty-four-year-old women, whereas it was similar for men. These differential trends relate to changing patterns of participation due to children. Nevertheless we can still examine the pilot versus nonpilot experiments, as the outflow trends for young women were similar in pilot and nonpilot areas. The results are shown in row six of table 11.7. The point estimates are smaller than those of men (6 percent compared to 10 percent), although the smaller sample size means that the coefficients are very imprecisely estimated.

Finally, one could consider using other age cutoffs than the ones that we chose to focus on. For example, in the spirit of regression discontinuity design (see Hahn and Van der Klaauw 1999), one could simply compare twenty-four-year-olds with twenty-five-year-olds (rather than nineteen–twenty-four-year-olds with twenty-five–twenty-nine-year-olds). As discussed previously, this has the advantage that the two groups will be sub-

26. There is, unfortunately, no information on earnings in JUVOS. The survey information in Hales et al. (2000) suggests that the New Deal participants are earning only slightly above the minimum wage.

Table 11.7 **Regression Results of Further Investigations of the New Deal Effect (percentage point increase in the probability of leaving unemployment)**

Experiment	No. of Observations	Estimates Based on Difference-in-Difference Method
Pilot Period		
Men		
1. Outflows to sustained jobs (13 weeks or more in job)	17,433	0.045** (0.011)
2. Outflows to sustained subsidized jobs (affecting 19–24-year-olds)[a]	55,051	0.031
3. Outflows to employment between 5th and 6th months of JSA	20,957	0.004 (0.008)
4. Outflows to all destinations (19–24-year-olds vs. 25–30-year-olds from the national rollout areas and Pathfinder areas	17,433	0.108** (0.015)
5. Outflows to all New Deal options (affecting 19–24-year-olds)	55,051	0.137
Women		
6. Outflows to employment (using 19–24-year-olds in Pathfinder vs. 19–24-year-olds in non-Pathfinder areas)	1,169	0.061 (0.058)
Pilot Period with National Rollout		
7. Women: Outflow into the employment option (affecting 19–24-year-olds in the Pathfinder areas)	1,693	0.048
8. Men: Using 24- vs. 25-year-olds (instead of 19–24- vs. 25–29-year-olds)	2,767	0.068** (0.033)

Source: Blundell, Costa Dias, Meghir, and Van Reenen (2001).

Notes: Standard errors are in parentheses. Estimates of the effects of the New Deal used the Joint Unemployment and Vacancies Operating System (JUVOS) 5 percent longitudinal sample of Job Seekers Allowance (JSA) claimants. The table considers those obtaining six months of JSA between the second to fourth quarters of 1997 and 1998 for the national rollout estimates. The first quarters of 1997 and 1998 are used for the pilot period estimates. Individuals verifying this criterion are then followed up to the end of the tenth month on JSA to check whether they have found a job. The eligible group (defined by the age or pilot area criterion) is compared with the selected control group.

All estimates are from regressions include a set of other controls, namely marital status, sought occupation, region, and some information on the labor market history (comprising the number of JSA spells since 1982 and the proportion of time on JSA over the 2 years that precede the start of the present unemployment spell).

[a]Estimates of the outflows to options are obtained from the New Deal Evaluation Database (NDED).

**Significant at the 0.05 level.

*Significant at the 0.10 level.

ject to the same trends, but the disadvantage that substitution effects (if they exist) will be strongest for these age groups. The results of this experiment are contained in row eight. The treatment effect is larger in magnitude (0.068), although not significantly different from the baseline effect (0.053), which is unsurprising given the much smaller sample size.

In conclusion, the range of experiments contained in table 11.7 suggests that the results are quite robust.

11.5 Cost-Benefit Calculation

Any cost-benefit analysis must proceed with a large degree of caution due to the uncertainty surrounding key parameters. Nevertheless, it is important to try and put the numbers into perspective, no matter how crudely (see table 11.8 for a summary and appendix for more details). The analysis is forward looking—I seek to investigate whether the New Deal would be a program that would be worth making a permanent feature of the U.K. labor market.

One of the main benefits of the New Deal is the number of jobs (and therefore extra output) created. To estimate the number of jobs, a number of assumptions have to be made regarding the counterfactual. I simulate the change in steady state for an economy that broadly matched the U.K. economy in 1998 when the New Deal was introduced.

Table 11.8 **Preliminary Cost Benefit Analysis of New Deal (in £millions)**

Item	Description	Baseline (1)	Optimistic (2)	Pessimistic (3)
Key assumptions		Employment up by 17,250 (average wage £7,272)	Employment up by 17,250 (average wage £8,500)	Employment up by 15,000 (average wage £7,272)
1. Increased output from jobs	No. of new jobs × average earnings	125.4	146.6	98.8
2. Gross Exchequer cost	Transfers to participants in New Deal, direct cost of Gateway, etc.	250.3	250.3	254.2
3. Benefit and tax savings	JSA, Housing Benefit, income tax and NI, etc.	148.8	156.1	133.6
4. Direct cost of Gateway	Personal advisers, etc.	52.9	52.9	55.0
5. Net Exchequer cost	(Item 2) – (Item 3)	101.5	94.2	120.6
6. Excess burden of taxation	(Item 5) × excess burden (15%)	15.2	14.1	18.1
7. Total social costs	(Item 4) + (Item 6)	68.1	67.0	73.1
8. Net Social Benefit	(Item 1) – (Item 7)	57.3	79.6	25.7

Notes: See appendix and section 11.5 for details of the calculations.

The estimates from the previous section showed that the effect of the New Deal was to raise the employment outflows (see table 11.5) of young men by 5.3 percentage points—an elasticity of about 0.2 (i.e., 5 percentage points over a presample base of 25.8 percent). I consider three main labor market states only (employment, short-term or under six months of unemployment, and long-term or over six months of unemployment). I then simulate a permanent increase in monthly outflow rates from long-term unemployment to employment using this elasticity of 0.2 (keeping all the other outflow rates constant) and solve for the new steady states stocks. The stock of long-term unemployment (including those in the Gateway and on nonjob options) falls by about 20,000 per year, and the employment level rises by 17,250.[27] The number of short-term unemployed rises by 2,850 because employment is higher and the outflow rate from employment to short-term unemployment is unchanged (by assumption).

I use the estimates of the mean starting wages of workers on the New Deal subsidized job option from the survey in Hales et al. (2000) of £3.78 per hour,[28] average annual earnings are just under £7300. Using this as our measure of output leads to a social benefit of £125 million (row one).

On the costs side, we have a gross exchequer cost in row two of £250 million[29] (about £100 million in allowances for the various New Deal options, £50 million for the Gateway and £100 million for the resource inputs into the options). We have to deduct off (1) the fact that unemployment and other benefits were already being paid to these individuals, and (2) those individuals that gain jobs and enjoy higher allowances will be paying some more tax. These items total £149 million (row three). In addition, some of these costs are transfers, so these will contribute only to social costs due to the excess burden of taxation (the deadweight loss involved in a higher level of taxation). Using an excess burden rate of 15 percent, this is about £15 million (row six). On the other hand, the cost of maintaining the Gateway is a real productive cost due to the diversion of resources from other parts of the economy. There are also real resource costs involved in supplying the options, but (following Layard 2000) I assume the benefits of taking an option (e.g., the increased human capital associated with training) perfectly offset these costs. Summing the excess burden and Gateway gives a total social cost of just over £68 million.

27. This is consistent with the more macro-based approaches. Anderton, Riley, and Young (1999) estimate an employment impact of the New Deal for Young People of 18,000 between January 1998 and October 1999. Riley and Young (2001b) estimate that the New Deal for Young people has increased youth employment by 15,000 per year between March 1998 and March 2000.

28. This may be an underestimate as it does not take into account wage growth over the year and the fact that those on the New Deal subsidy may be less productive than those who left the Gateway for an unsubsidized job.

29. This is lower than the numbers in table 11.3 because our analysis is in long run. In steady state the New Deal has reduced the equilibrium numbers of the long-term unemployed, so total costs are lower.

The social cost is much less than social benefits of the extra output generated, so there ends up being an annual net social benefit of the program of just over £57 million. The figure is lower than that of the £100 million net benefit in Layard (2000), mainly because I empirically estimate slightly smaller effects of the New Deal on unemployment and employment than those on which Layard bases his calculations. Furthermore, I use actual rather than assumed wage gains. Nevertheless, I concur with his conclusion that the social benefits of the New Deal are likely to outweigh its social costs.

The other two columns of table 11.8 show the sensitivity of these calculations to changes in key assumptions. Column (2) assumes optimistically that average annual earnings of those getting jobs from the New Deal are £8500. This increases the net benefits to about £80 million. The final column makes one change from the first column by pessimistically assuming that the employment effect is only 15,800, (1 standard error below the estimated effect in table 11.6). The net benefit falls to £26 million, half of that in column (1). This illustrates the importance of the magnitude of the employment effect in determining overall benefits. If the employment effect fell to 14,000, then the social costs and benefits are broadly equal.

This analysis probably underestimates the value of the program for three reasons. First, it does not take into account the social benefits of reduced crime, teenage pregnancy, and so on. Second, it does not factor in the redistributive effects from relatively wealthy older taxpayers to the less wealthy young unemployed. Finally, we do not estimate the extent to which the New Deal program enhances the employability and productivity of individuals who participate in the options. We merely assume that the resource input per New Dealer is not more than the present value of the benefit received. On the other hand, the welfare benefits may be overstated as we have not given any weight to the value of leisure for the unemployed.

The job assistance element of the New Deal is more cost effective than the New Deal options as there is no subsidy involved. The lower bound of the job-assistance and monitoring effect works out to increase steady state employment by about 8,000.

Existing U.S. evaluations are rather pessimistic about the ability of temporary government jobs and training schemes to raise the long-term prospects of the young unemployed, especially young men.[30] It is worth remembering, however, that the U.S. schemes focus on extremely disadvantaged youth who may be from a comparatively lower part of the ability distribution than the New Deal participants considered here (especially for men). The success of the employment subsidy option will also hinge on the extent to which the experience of work and training will raise productivity, thereby enabling workers to keep their jobs when the subsidy runs out (Bell, Blundell, and Van Reenen 1999).

30. For a recent survey, see White, Auspos, and Richhio (1999).

11.6 Conclusions

In this chapter I have examined the British New Deal for the Young Unemployed. This is a major program to enhance the employment rates of eighteen–twenty-four-year-olds. The youth labor market is an important issue in most countries, especially in Europe where the unemployment rates of the young are well above the OECD average.

The main finding is from the analysis of outflow rates to jobs before and after the introduction of the New Deal. The program appears to have had a significant effect in moving more young people into jobs. According to our estimates, young unemployed men are about 20 percent more likely to find jobs each month because of the New Deal. I estimate that the New Deal has lead to an increase in "steady state" youth employment of over 17,000.

The New Deal should be seen as the latest step in the progressive moves in Britain to tighten the obligation to search for work while claiming unemployment benefits. This process began with the Restart reform of 1986 that made work-focused interviews compulsory for those on longer unemployment durations. One important difference, however, is that the New Deal is much more generous in providing advice and hard cash (e.g., for wage subsidies and training) than previous reforms.

A "reengineered" New Deal has continued since the Labour Party's reelection in 2001. The government has put greater emphasis on intensifying the job search and extending mandatory options for an ever-larger proportion of benefit recipients. The employers' wage subsidy is the element that is most vulnerable to being cut, due to its low take-up. Such a cut could be premature. As this paper has shown, the wage subsidy appears to have had a significant impact on increasing jobs (at least in the first few months on the program). The long-term success of the New Deal hinges critically upon improving employment prospects through the acquisition of better job skills, either in the Gateway period or, more likely, during one of the options. It will take some time to monitor the extent to which these dynamic gains in worker productivity really have been boosted by the New Deal.

Taken as a whole, though, the program is judged to be a modest success at a modest cost. Its social benefits appear to outweigh its social costs.

Appendix

Cost-Benefit Analysis

I perform a forward-looking analysis of the effects of the New Deal for Young People. Assuming the stock of people unemployed for more than six months has been eliminated, the New Deal for Young People will have

its effect on the flow of eighteen–twenty-four-year-olds unemployed for six months.

I begin with our estimates of the effects of the New Deal on increasing outflows. The baseline estimate (table 11.6, row seven) is that New Deal has increased the probability of leaving unemployment for a job by 0.205 (=0.053/0.258). We assume that this elasticity is true for all groups (men and women, whatever duration of unemployment over six months, etc.).

I perform a simulation exercise for a counterfactual economy matched to the features of the U.K. economy in 1998. There are three states: employment, short-term unemployment (under six months), and long-term unemployment (over six months). Individuals on the nonemployer options are treated as long-term unemployed for the purpose of calculating the stocks. I assume that the labor force for young people is fixed at 1.875 million. Initially there are 125,000 long-term unemployed; 250,000 short-term unemployed; and 1.5 million employed. With an outflow rate of 10 percent, this implies an impact effect of an additional monthly outflow of 2,562 (= 0.1*0.205*125), or 31,000 per year. In steady state, however, the stocks will adjust to the new outflow *rates* so the equilibrium flows and stocks will be different. Under the assumption that the New Deal only impacts on the flow rate between long-term unemployment and employment, we can solve for the new steady-state levels of the three labor market states. Long-term unemployment falls by 20,088. Of these individuals, 17,250 enter the stock of employment, and 2,840 become short-term unemployed.

To compute the benefits we assume that young people who get jobs as a result of the New Deal produce an amount equal to the corresponding wage. Hales et al. (2000) report on a survey of people on the employer option that suggests an average hourly wage of £3.78. Assuming a thirty-seven-hour week, this implies annual earnings of £7,272. So one clear benefit is the increase in employment, multiplied by annual earnings (17,250 × £7,272 = £125.4 million).

Other benefits include the gross output of the voluntary and environmental options and the value of training.

On the cost side we have to include

- The resource cost of the Gateway period (although there was already something like this under the previous JSA regime). These are mainly administrative costs, such as the salaries of personal advisers.
- The transfers to individuals and firms involved with New Deal options. These only matter from a social point of view because of the excess burden of taxation. This has to be calculated from the increased additional taxation necessary to finance the New Deal. There are transfer payments to employers through subsidized jobs and to participants through the other options. I assume that in steady state there are 12,000 participants on the subsidized job option. Of the long-term

unemployed 72 percent are in the Gateway or Follow-Through, 14 percent are in the full-time education and training option, and 14 percent are in the Environmental Task Force or voluntary-sector option. These proportions approximate those in 1999. The subsidies given to each of these groups is defined by the program (see section 11.3.2). The sum of these is the gross exchequer cost. We must deduct from this the benefit payments that would have been received by young unemployed people if the New Deal did not exist. Also, we include the additional taxes received by the Revenue. This net exchequer cost is the additional tax that needs to be raised, and this will have a deadweight cost associated with it. Note that the transfers themselves are not included in the social costs.

- The annual cost of JSA (£2,080) for all those on New Deal options (to calculate the benefit savings). For those who are employed as a result of the New Deal, there are also potential savings in housing benefits (£2,080 on a rent of £40 per week) and council tax benefits (about £468), but not all participants on the New Deal can claim these (e.g., if they live with their parents). I extracted data on eighteen–twenty-four-year-olds on JSA using the FES. About 40 percent of the relevant group claimed these benefits, so I weighted the value by this proportion. For the employed group I used the wage (£7,200) to calculate income tax and national insurance (about £472 and £361, respectively, for those on £7,300 per year). Finally, as net disposable income has risen, consumption will rise and there will be a further tax take through value added taxes (VAT) and excise duties. Taking all these elements into account implies an average tax and benefits saving of around £3,600 for each person who moves off unemployment as a result of the New Deal.
- The resource costs of the New Deal options. We make the simplifying assumption that the output of the voluntary and environmental options is equal to the resource costs. One would expect that the output is rather higher. We also assume that the value of training is equal to the resource input. This is controversial as most U.S. studies find little effect of training on unemployed youth. The New Deal courses are, however, typically much longer than those in U.S. programs (up to twelve months in full-time education), so the assumption is not unreasonable.

I have not put any value on the lost leisure time of those who were unemployed but are now productively engaged in different activities.

These calculations ignore many of the potential benefits of the New Deal. First, it does not take into account the social benefits of reduced crime, teenage pregnancy, and so on. Second, it does not factor in the redistributive effects from relatively wealthy older taxpayers to the less

wealthy young unemployed. Finally, we do not estimate the extent to which the New Deal program enhances the employability and productivity of individuals who participate in the options. We merely assume that the resource input per New Dealer is not more than the present value of the benefit received.

The calculations also assume that there are no substitution effects or general equilibrium effects. The former would increase the costs, and the latter would increase the benefits. Strong evidence of large substitution or major general equilibrium effects was not uncovered in section 11.4.

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