THE FIRST PROJECT WORK PILOTS:
A QUANTITATIVE EVALUATION

**A Report to the
Employment Service
and the Department for Education and Employment
by the Policy Studies Institute**

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Disclaimer

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List of Acronyms

DfEE	Department for Education and Employment
ES	Employment Service
JSA	Jobseekers' Allowance
JUVOS	Joint Unemployment and Vacancies Operating System
M/M	Medway/Maidstone
OLS	Ordinary Least Squares
PSI	Policy Studies Institute
PW	Project Work
SCPR	Social and Community Planning Research
TfW	Training for Work
TTWA	Travel-to-Work Area

Executive Summary

Background, objectives and methodology

Project Work was a government programme aiming to help people aged 18-50 who had been claimant unemployed for two or more years to leave the register and get work. The first two pilots were launched in Hull and Medway/Maidstone in April 1996, and involved all the local Employment Service (ES) offices in those areas. During their first 13 weeks on the programme, eligible claimants were offered intensive help with job search through 1-2-1 sessions with client advisers, and there was a Workstart wage subsidy for employers who took them on. If clients had not left the register at the end of 13 weeks, then they were required to spend a further 13 weeks in work experience on special projects or in voluntary agencies. This was intended to be in activities matched to the abilities and interests of the participants but the range of work experience placements was more limited than anticipated and participants were usually placed in low skill work. During work experience, clients were signed off the unemployment register and received instead the Project Work training allowance. Benefit penalties were imposed on those who refused without good reason to do work experience, and those who left work experience early were required to return to complete their 13 weeks before they could sign on again as unemployed.

An evaluation study for the first Project Work pilots was designed by the ES and the Department for Education and Employment. SCPR conducted the fieldwork, and the Policy Studies Institute was contracted to analyse the data. The objective of this analysis was to quantify any effect on the number of people leaving the register, the number taking up full-time and part-time employment, job search activity and wages.

The progress of eligible clients in the pilot offices was compared with that of claimants in other local ES offices who also had been unemployed for two or more years. These comparison offices, which were scattered across England and Wales, were chosen on the basis of the characteristics of their unemployment registers, six being matched with the Medway/Maidstone pilot offices and six with the Hull pilot offices. However the practical difficulty of finding offices in a position to take part in the study meant that, in terms of local labour market characteristics, the match achieved was not very close, particularly for Hull. In addition there was some overlap between the pilot and comparison offices in other new initiatives that were being piloted simultaneously.

In February 1997, further Project Work pilots (separately evaluated) were launched in a number of areas, including four of the six comparison offices for Medway/Maidstone. This meant that comparisons relating to the Medway/Maidstone pilot offices had to be restricted to the period to the end of January of that year.

Data from four sources were collected for the evaluation of the first pilots:
- a sample survey involving a long 'in-depth' interview, known as the *main survey;*
- a short sample survey of clients who left the unemployment register, known as the *leaver survey;*

- administrative data assembled at the local ES offices;
- centrally held data from the JUVOS files for on-flows to and off-flows from the unemployment register, plus, if relevant, the dates of claims for the Project Work training allowance held on the JUVOS training file.

There were technical difficulties with the samples issued for the two surveys, and both suffered from low response rates due to a high proportion of sample members who could not be traced from the address held at the local ES office. In addition, other problems with the leaver survey led to fieldwork being halted prematurely. These factors weakened confidence in the survey results. The administrative data assembled at the local ES offices proved to contain too many gaps and inconsistencies to be useful. Some effort was needed to convert the JUVOS data into internally consistent individual histories, but, once done, these data proved extremely valuable.

The investigations to meet the research objectives included both descriptive analyses and statistical modelling. The former are easy to understand, but cannot take account of all relevant differences in claimant characteristics. The latter requires more explanation, but produces results which are more securely based.

Results

Statistical modelling based on the main survey suggested that, after controlling for other factors that affected employment outcomes, eligible clients in the pilot offices were more likely to get a job than similar clients in the comparison offices. This effect was clearer in Medway/Maidstone than in Hull. However, while eligible clients from the Medway/Maidstone pilot offices were more likely than clients in their comparison offices to get part-time or temporary jobs, they had no statistically significant advantage in getting full-time permanent jobs. We found no clear evidence that Project Work had any association with wages, and evidence about the association between Project Work and job search was confused.

The main survey also indicated that eligible clients in the Hull pilot offices were more likely to leave the unemployment register than similar clients in their comparison offices, but showed no statistically significant difference between the Medway/Maidstone pilot and comparison offices. It was not unusual for older Project Work participants to move from unemployment-related benefits to other social security benefits.

Further evidence for these conclusions came from statistical modelling based on the leaver survey. This suggested that eligible clients in the Hull pilot offices who left the unemployment register were less likely to do so for a job than similar clients in their comparison offices. This did not hold true for Medway/Maidstone.

The JUVOS data provided very large sample numbers but fewer variables to use as controls on the Project Work effect. In analysing these data, time that Project Work participants spent on compulsory work experience was counted in with time spent on the unemployment register to give a single category of 'time unemployed'.

Descriptive analysis of the JUVOS data showed that, over the study period, Project Work participants spent on average less time unemployed than members of their comparison samples. The difference was made up by more time in paid work, more time in full-time education or training, more time in receipt of other social security benefits, and more time in which there was no information on what the client was doing. More time on other social security benefits was particularly important for clients aged 45-50 and for people who had been unemployed for a very long time.

Plots of economic activity status over time from the point of enrolment on Project Work showed that most of the register impact of the programme occurred around the transition between the first 13 weeks of optional intensive help with job search and the compulsory 13 weeks of work experience. Thereafter, the register impact of the programme gradually diminished. The maximum impact of Project Work on employment rates occurred at the same time point. The employment impact was much smaller than the register impact, and it also later decayed. These patterns suggested that some clients in the pilot offices signed off the register in order to avoid going on compulsory work experience.

Statistical models were fitted to the administrative data using the limited number of control variables available. These confirmed the evidence of the plots of a large register impact of Project Work and a smaller but still statistically significant association with an increased probability of employment, both decaying over time. However it was not possible to determine the extent to which these associations were influenced by differences between the local labour markets of the pilot and comparison areas that were independent of the programme. This issue was particularly problematic for employment rates, where the apparent impact of Project Work was small. There was also evidence that some of the register impact of Project Work in Medway and Maidstone was associated with seasonal demand for casual labour over the spring and summer. No evidence of seasonality was found in Hull.

Some features of the administrative data suggested that enrolment of some very long-term unemployed clients to the first Project Work pilots may have been deferred until towards the end of the pilots. There was also evidence that the register impact of Project Work was greatest for people who had been unemployed for a long time. This may have been because people in the comparison offices who reached two years unemployment received Restart courses (which Project Work participants reaching the two-year point did not), or because compulsory work experience had a particularly strong impact on very long term unemployed people, or because very long-term unemployed Project Work participants were particularly likely to be transferred to other social security benefits.

The evaluation of the first Project Work pilots suggests that JUVOS data have great potential in such studies. However it also shows how good evaluation design can be constrained by prior policy decisions about the structure of pilot programmes.

1 Introduction

The Project Work programme

Project Work was a programme run by the Department for Education and Employment (DfEE) and the Employment Service (ES) for people between the ages of 18 and 50 who had been unemployed and claiming benefit for two or more years. It aimed to get them off unemployment related benefits and into work. The programme was launched in two pilot areas, Hull and Medway/Maidstone, in April 1996, and was designed to run until June 1997. In February 1997 it was extended to 27 more areas, and in these extended pilots some aspects of the programme were changed. This report is concerned only with the original two pilot areas, and the form that Project Work took there.

The programme as it was run in Hull and Medway/Maidstone covered everyone who either was already unemployed for two years or more when the programme began (the 'stock'), or who reached two years unemployment while the programme was running (the 'flow'). They were put onto the programme when they went for their regular six-monthly Restart interview, though the entry of the stock to the programme was planned to be staggered over several months to allow a more even workload throughout the life of the pilots. An excellent account of participants' experiences once on Project Work can be found in Ritchie and Legard (1997).

The first element in Project Work, 'Period A', was voluntary. It offered 13 weeks of intensive help with job search through fortnightly '1-2-1' interviews with the same client adviser, who, by getting to know the client better, could give more focussed help. Period A stretched the resources of the Employment Service, and not all clients who agreed to take part had an interview every fortnight or were able always to see the same adviser (Ritchie and Legard 1997). As well as the usual menu of options such as Job Clubs, Work Trials and Training for Work (now called work-based training for adults), which many long-term unemployed clients had already sampled, Project Work participants were offered two additional types of help during Period A. The first of these was Workstart, a wage subsidy totalling at that time £1,500 given to employers who provided employment lasting six months or more for someone who had been claimant unemployed for at least two years. Workstart had previously been piloted in other parts of the country. The second was the discretionary stepped Jobfinder's grant. Flat-rate Jobfinder's grants - lump-sum payments of £200 to long-term unemployed people who found work – were available throughout the country, but in the Hull Project Work pilot offices (though not in Medway/Maidstone) these grants could be increased according to the discretion of the client adviser up to a maximum of £400.

If at the end of Period A Project Work participants had not left claimant unemployment (whether or not they had taken advantage of the help offered), or if they had left claimant unemployment but later signed on again, they moved on to 'Period B'. This lasted 13 weeks and comprised 18 hours per week of work experience plus three hours per week of help with job search. During Period B clients were signed off the unemployment register and put onto a training allowance. Period B was compulsory in that clients were subject to benefit sanctions if they failed without good cause to take part in either the work experience or job search element. Furthermore, if they left before their 13 weeks were completed and tried to sign on again as unemployed, then they were sent back onto Period B, and this cycle could be repeated several times until they had clocked up the

required 13 weeks. Once the 13 weeks had been completed they were allowed to sign on again as unemployed. At this point their claim was registered as a fresh claim and they were no longer classified as long-term unemployed in official statistics, though they kept their eligibility for any government programmes for unemployed people that they had been eligible for previously.

Period B was not intended to provide training for clients; rather its aim was to give them a recent work record and up-to-date reference that might be valued by potential employers, and to re-introduce them to the habits needed to hold down regular employment. Of course it also made it difficult for any clients who might have been illegally working more than the permitted number of hours per week while they were claiming benefit to continue to do so. The provision of both the work experience and job search help elements was contracted out to placement providers. Although where possible providers matched clients with suitable placements, clients did not generally have much choice over what they did. Work experience usually took the form of low skill social, community or environmental work with voluntary agencies or on specially created short-term projects, rather than placements with employers. There were several reasons for this, including the reluctance of employers to offer placements to very long-term unemployed people and trade union opposition to Project Work. This lack of choice combined with the nature of the work offered may have deterred some clients from continuing their claim for benefit, given others a push towards finding a job, and encouraged yet others to try to move from unemployment benefit to other types of benefit.

As we have seen, Project Work incorporated initiatives that had been introduced elsewhere. What was new about the programme was the imposition of benefit sanctions on long-term unemployed people who refused to undertake work experience. For a year or two in the late 1980s the short-lived New Job Training Scheme had imposed benefit sanctions on young people aged under 25 who were long-term unemployed and who refused to take up work experience placements with employers, but Project Work was the first programme to apply such sanctions to older people. Since then, the principal of benefit sanctions has been embodied in the New Deal, first for 18-24 year olds and now for older people. The threat of benefit sanctions created a great deal of local opposition to Project Work: trade unions and organisations representing unemployed people likened the programme to the American 'Workfare', while the local authorities in both Hull and Medway/Maidstone refused to provide placements.

Other factors in the pilot offices

The main criterion for evaluating Project Work was whether it succeeded in increasing the rate at which long-term unemployed people left the register. However exit rates in the pilot offices are likely to have been affected not only by the new programme, but also by other quite separate factors. These include other policy initiatives, as well as the level and nature of the demand for labour in the local economy.

In October 1996, when the first Project Work pilots had been in operation for six months, the Jobseekers' Allowance (JSA) was introduced nationally, replacing Unemployment Benefit and Income Support for the unemployed. The JSA brought more stringent tests of whether claimants were actively seeking work, and it is likely to have had an independent impact on the numbers leaving claimant unemployment. During its first few

months, the JSA increased the workload for ES staff in local employment offices, and some difficulties were experienced with maintaining administrative records. This may have disturbed the smooth running of Project Work.

The launch of the JSA affected the Project Work pilot offices and the rest of the country equally, but one of the pilot areas, Hull, was the target of a special operation in November 1996 that is likely to have affected unemployment exit rates in that area alone. This was a crackdown on benefit fraud that was run as a joint operation between the ES, the police and the Department of Social Security. The crackdown was a high profile campaign involving raids on employers' premises, building sites, outdoor markets and other places where casual work could easily be got. This campaign led directly to the stopping of benefits for those allegedly fraudulent claimants who were caught, and also attracted a lot of local publicity which may have caused other people who were unsure of the legality of their position to cease claiming.

Because the first Project Work pilots were launched in just two areas of the country, an assessment of outcomes is particularly vulnerable to the effects of specific local labour market conditions. The two pilot areas were quite different in character. The Hull pilot area, which included the city of Hull and the neighbouring town of Beverley in the Yorkshire Wolds, was geographically compact, relatively isolated from other large labour markets and surrounded by a large rural hinterland. The pilot offices in Hull had a strong central management and had been used to test a number of ES initiatives. As Ritchie and Legard describe (1997), following the decline of the local fishing and heavy manufacturing industries, Hull lost many semi-skilled and unskilled jobs, and no large employers moved in to take advantage of the pool of unemployed labour. In contrast, the Medway/Maidstone pilot area was widely spread, and included offices in three Medway towns along with Maidstone and Gravesend. A high proportion of residents commuted out of the area to work in Greater London. Ritchie and Legard tell how a decline in the manufacturing and construction industries together with the closure of a local oil refinery and the Chatham dockyard caused job losses that affected mainly semi-skilled and unskilled workers, who were unlikely to find employment in the new technology industries that have since grown up.

In April 1996, at the start of the first Project Work pilots, the claimant unemployment rate in the Medway/Maidstone travel-to-work area (TTWA) stood at 8.2 per cent of the workforce, against a rate for the whole of Great Britain of 7.8 per cent. The rate in Hull was somewhat higher, at 9.8 per cent. In Hull an especially high proportion of employment was of a temporary or casual nature: 26 per cent of vacancies held by local employment offices in April 1996 were for temporary jobs, compared to 14 per cent in Medway/Maidstone.

The evaluation study

The evaluation of the first Project Work pilots comprised both qualitative and quantitative elements. A report on the qualitative evaluation, carried out by SCPR, has already been published (Ritchie and Legard 1997). The quantitative evaluation was jointly designed by the ES and the DfEE. It involved two surveys, the 'main survey' and the 'leaver survey', plus the collection of administrative data. SCPR designed the questionnaires and carried out fieldwork for the surveys, the ES provided the administrative data, and the Policy Studies Institute was contracted to analyse all three data sets. The design of the main survey is described in Chapter 2 and that of the leaver

survey in Chapter 5. The main elements of the administrative data are outlined in Chapter 6, while Appendix 3 records how we cleaned these data and converted them to a form suitable for analysis.

Any attempt to assess how well a programme meets its objectives must not only measure how many participants secure the desired outcomes, but must also tackle the counterfactual question of whether these outcomes would have been achieved if they had not taken part in the programme. In the case of Project Work, this means how many long-term unemployed people would have left the unemployment register or found work if they had not been on the programme.   In evaluations of medical treatments, the counterfactual question is often addressed by the use of random allocation research designs, which randomise out differences between the treatment and control groups. The government has not generally permitted this option for the evaluation of social policy initiatives in Britain, because of arguments about equity. As a result, we have had to rely on statistical controls for confounding factors, on matched comparison group designs, on the analysis of historical trends, or on some combination of these techniques.

The evaluation of Project Work raised particular problems because participation in the work experience element was compulsory for long-term unemployed people who wished to continue claiming unemployment-related benefits. If the programme had been introduced in one local employment office but not in neighbouring offices, then potential participants might have tried to evade participation by switching their unemployment registration to another office. To avoid this, the programme was introduced simultaneously in all the offices within the two chosen pilot areas. This feature of the pilots created problems for the evaluation, as programme participants could not be compared with non-participants in the same area who were otherwise subject to identical local labour market conditions and local policies.

With some evaluations of pilot programmes, the problem of taking account of variation in local conditions can be overcome by introducing the programme into a number of local areas, so that in assessing outcomes we can control statistically for this variation. However the design of the Project Work evaluation was constrained by the fact that the first Project Work pilots were introduced into only two areas, making it hard to distinguish the effects of the programme from the effects of other particular features of those areas. A wider spread of pilot offices across a range of local labour markets and administrative regimes would have given a better chance of detecting any true programme effects.

In the evaluation design for the first Project Work pilots, the counterfactual question of whether any observed outcomes would have been found in the absence of the programme was tackled by nominating a number of 'control' offices in which the progress of people who had been unemployed for two or more years was also monitored. An attempt was made to match these offices on a one-to-one basis with the individual pilot offices that comprised the Hull and Medway/Maidstone pilot areas. The 'control' offices were selected by the ES on the basis of a cluster analysis of all ES local offices using the following variables computed from the office's unemployment register: unemployment stock, off-flow as a percentage of stock, unemployment rate, percentage of postal signers, stock of people unemployed for two or more years, and stock of people unemployed for two or more years as a percentage of the total unemployment stock. Unfortunately, as far as the authors of the present report have been able to ascertain, no information was kept

on the weight attached to each of these variables or on how the matching operation was conducted.

Regardless of the precise formula used in the matching, it would have been more satisfactory to match offices on the basis of the wider labour markets in which they were located rather than their own unemployment registers. The composition of the register of a local office can be affected by a range of idiosyncratic factors (such as the type of housing around it), and is not necessarily a good indicator of the job opportunities available to the people who register there. The latter is more appropriately measured by the characteristics of the larger TTWA in which it is situated. In addition, the matches were based on a single point in time and took no account of trends over time. Thus for example, two offices may have had identical unemployment rates in the chosen month, but one office may have reached this point after falling rapidly from a much higher rate, whereas in the other the unemployment rate may have been falling much more slowly. Job opportunities for unemployed people in two such areas are likely to differ. In the case of the Project Work pilot and 'control' offices there were marked differences in the trends in the TTWA unemployment rate in the year leading up to the launch of the pilots, as we will see below.

There were also problems with the implementation of the evaluation design, that emerged as the process of selecting 'control' offices got underway. Many of those chosen as the best matches to the pilot offices were either deemed to be unsuitable because they were already taking part in the evaluation of a different initiative, or were allowed to withdraw from the study because their workload was already very high. From the point of view of the staff of a local employment office, the role of 'control' office in an evaluation study tends not to be particularly rewarding, and the extra tasks that it entails may be burdensome. Thus the selection process had to be repeated several times before enough 'control' offices were secured, and a progressively lower level of matching was accepted at each attempt.

For all of these the reasons, we prefer not to use the term 'control offices' in this report, as it implies that they differed from the pilot offices only in that they did not implement the Project Work programme. Instead we use the weaker term 'comparison offices', meaning simply that these were the offices with which the pilot offices were compared.

Local labour market characteristics of the pilot and comparison offices

One consequence of the repeated attempts needed to select the comparison offices was that, geographically and economically, the offices that were finally chosen gave the impression of a somewhat *ad hoc* collection. The Hull pilot offices were matched with two offices in the same Yorkshire and Humberside region (Hillsborough in inner-city Sheffield and Chapeltown, a fairly isolated former coal mining district); with three offices in the adjacent region of East Anglia (Barton on Humber at the other end of the Humber Bridge, the small country town of Fakenham in Norfolk and the seaside resort and fishing port of Great Yarmouth); and with one office some hundreds of miles away in Newport in the industrial conglomeration of South Wales. The Medway/Maidstone pilot offices were matched with four offices in the South East: three in the Portsmouth conurbation and one in Newhaven, which adjoins the seaside resort and university town of Brighton. They were also matched with two far distant offices: one in the small country town of

Cardigan in rural West Wales and the other in the heavily industrialised area of Stretford in the Manchester conurbation.

Project Work was intended to reduce the unemployment rate by increasing the flow out of long-term unemployment, so to assess how well the pilot and comparison offices were matched, we need to compare the unemployment rate in their local labour markets over the period leading up to the start of the programme. Table 1.1 shows the relevant TTWA unemployment rates in March 1996 - the month before the start of the Project Work pilots - and in March 1995, one year earlier.

Table 1.1
Unemployment rates in the TTWAs in which the pilot and comparison offices were located, March 1996 and March 1995.

	A	B	C	D
	% of workforce unemploye d March 96	Rank amongst TTWAs in England & Wales	% of workforce unemploye d March 95	Change March 95 to March 96
Medway/Maidstone	**8.3**	**180**	**9.0**	**-0.7**
Comparison offices for M/M:				
Portsmouth area (3 offices)	8.1	171	8.5	-0.4
Stretford (Grtr Manchester)	8.0	166	8.5	-0.5
Cardigan	7.8	161	7.9	-0.1
Newhaven (Brighton)	10.3	226	10.7	-0.4
Hull	**9.6**	**205**	**10.4**	**-0.8**
Comparison offices forHull:				
Barton on Humber	10.3	226	10.9	-0.6
Fakenham (Norfolk)	7.5	151	7.9	-0.4
Great Yarmouth	11.1	239	12.2	-1.1
Hillsborough (Sheffield)	9.8	212	10.3	-0.5
Chapeltown (S. Yorks)	12.7	255	13.7	-1.0
Newport (Glam)	8.1	171	8.9	-0.8

The table confirms the impression of diversity. In March 1996 the unemployment rate for the Medway/Maidstone TTWA was 8.3 per cent. Five of the six comparison offices for Medway/Maidstone were in TTWAs with similar unemployment rates (within 0.5 of a percentage point), though they were not the closest matching TTWAs as we see from the

rank order amongst all TTWAs in England and Wales shown in column B. In the last comparison office for Medway/Maidstone, Newhaven, the TTWA unemployment rate was a full two percentage points above the rate for the pilot offices and 46 places distant in the rank order. Hull was even less well matched with its comparison offices. The March 1996 TTWA unemployment rate for Hull was 9.6 per cent, but only one of its comparison offices (Hillsborough) came within 0.5 of a percentage point of this figure, while the corresponding rate for its other comparison offices ranged from 7.5 per cent in Fakenham to 12.7 per cent in Chapeltown. In rank order of unemployment rates amongst all TTWAs in England and Wales, Hull was number 205, while its comparison offices ranged from rank 151 to rank 255.

Table 1.1 also shows the change in TTWA unemployment rates over the preceding year, from March 1995 to March 1996. Over this period, the unemployment rate had fallen more rapidly in the Medway/Maidstone TTWA than in any of the TTWAs in which its comparison offices were situated - a fall of 0.7 of a percentage point, compared a falls of 0.5 of a percentage point for Stretford ranging down to a mere 0.1 of a percentage point for Cardigan. This suggests that when Project Work was launched in Medway/Maidstone, job opportunities for unemployed people may already have been improving faster than in its comparison offices. For Hull and its comparison offices the picture is more confused. Between March 1995 and March 1996 the unemployment rate for the Hull TTWA fell by 0.8 of a percentage point. One of the comparison offices for Hull had a fall equal to this, while two showed bigger falls and three showed smaller falls. This variation again underlines the diversity of local economic conditions experienced by the offices with which Hull was to be compared.

Although Project Work was intended to reduce the unemployment rate, it is unlikely to have had much impact on the number of vacancies notified to local ES offices (though the Workstart subsidy may possibly have induced a small number of employers to create vacancies that they otherwise would not have offered). Thus we can use the change in the number of vacancies notified as an indicator of the bouyancy of the local labour market during the first few months of the Project Work pilots. Table 1.2 again suggests that in this respect the pilot and comparison offices showed considerable diversity. In the Medway/Maidstone TTWA the number of vacancies notified to local ES offices in September 1996 was 125 per cent of the April figure. While two of its comparison offices showed even greater levels of growth over these five months, there was actually a fall in the number of notified vacancies for the three Portsmouth offices and for Cardigan. In the Hull TTWA, the growth in notified vacancies between April and September 1996 was substantially greater than in the Medway/Maidstone TTWA. One of its comparison offices experienced a similar level of growth to Hull's, three experienced much higher levels of growth, and two experienced much lower levels of growth.

Table 1.2
Vacancies notified in September 1996 as a
percentage of vacancies notified in April 1996 in
the TTWAs in which the pilot and comparison
offices were located.

	%
Medway/Maidstone	**125**
Comparison offices for M/M:	
Portsmouth area (3 offices)	90
Stretford (Grtr Manchester)	147
Cardigan	83
Newhaven (Brighton)	141
Hull	**159**
Comparison offices for Hull:	
Barton on Humber	179
Fakenham (Norfolk)	131
Great Yarmouth	116
Hillsborough (Sheffield)	177
Chapeltown (S. Yorks)	184
Newport (Glam)	160

The diversity of the pilot and comparison offices is also revealed by the relative importance of temporary and casual work in their local economies. Here, the match between Hull and its comparison offices appears to be the least successful. Table 1.3 shows that at the start of Project Work in April 1996, 26 per cent of vacancies in the Hull TTWA were for temporary work. None of the TTWAs in which its comparison offices were located even approached this figure, the next highest being Great Yarmouth with 15 per cent, while rates in the other areas ranged down to two per cent. The proportion of temporary vacancies in the Medway/Maidstone TTWA – 14 per cent – was similar to the proportion in three of the TTWAs in which its comparison offices were situated, but much higher than in the Portsmouth TTWA, the location of its remaining three comparison offices.

This variation in local labour market conditions between the pilot and comparison offices means that we have to be very cautious in assessing the impact of Project Work. Part at least of any observed difference in the rate of exit from long-term unemployment is likely to result from differences in the level and nature of the local demand for labour. The research design makes it virtually impossible to estimate the separate impact of Project Work independently of the impact of local labour market conditions, firstly because the pilot and comparison offices were not well matched in terms of local labour markets, and secondly because the pilots were limited to two areas only, thus giving only very limited variation in local labour market conditions.

Table 1.3
**Temporary vacancies as a percentage of all notified
vacancies in the TTWAs in which the pilot and
comparison offices were located, April 1996**

	%
Medway/Maidstone	**13.7**
Comparison offices for M/M:	
Portsmouth area (3 offices)	4.7
Stretford (Grtr Manchester)	14.8
Cardigan	12.4
Newhaven (Brighton)	12.0
Hull	**26.0**
Comparison offices for Hull:	
Barton on Humber	1.8
Fakenham (Norfolk)	3.6
Great Yarmouth	15.2
Hillsborough (Sheffield)	5.3
Chapeltown (S. Yorks)	7.8
Newport (Glam)	8.5

Contamination of the comparison offices

The usefulness of the comparison offices was also undermined by the government's
decision to extend Project Work to further pilot offices across England and Wales. These
new pilots included the three Portsmouth offices and the Newhaven office that were
already chosen as comparison offices for Medway/Maidstone, and the new pilots began
in February 1997, several months before the first Project Work pilots were due to finish.
This meant that comparisons of the progress of long-term unemployed people in
Medway/Maidstone and four of their six comparison offices had to be limited to the ten
month period April 1996 to January 1997. It was particularly unfortunate that the two
comparison offices for Medway/Maidstone that remained uncontaminated by the
extended pilots were situated in geographically distant and economically very different
areas, namely Cardigan and Stretford.

The extended pilots were not the only new policy initiative that blurred comparisons
between the pilot and comparison offices. Some of the important components of Period
A of Project Work (voluntary 13 weeks of intensive help with job search) were also being
separately and simultaneously piloted in very similar forms in some of the comparison
offices. Table 1.4 shows how these schemes overlapped the pilot/comparison boundary.[1]

In addition, another component of Period A, the Workstart employment subsidy scheme,
had previously been piloted in Stretford (one of the comparison offices for Hull), though
this pilot ended immediately before the first Project Work pilots began.

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[1] We are indebted to Tristan Slinger of the DfEE for compiling this list.

Table 1.4
Overlapping initiatives in the pilot and comparison offices

Initiative	Description	Dates	Project Work pilot offices involved	Project Work comparison offices involved
Discretionary Jobfinder's Grant	Allows client adviser to fix the size of the grant	April 1995-March 1997	Hull	Barton on Humber Hillsborough Chapeltown
1-2-1 for clients unemployed for 30 months or more	Offers intensive job search help through regular interviews with the same adviser	May 1996-March 1997	Medway/Maidstone	Newport Cardigan Newhaven 2 Portsmouth offices
Access to Work Card trial pilot	Issues potential Access to Work beneficiaries with a credit-card style ID card	May 1995-March 1997	Hull	Barton on Humber Hillsborough Chapeltown
National Development Programme	Sets targets aimed at finding new ways to improve performance	1996 -	-	Cosham (Portsmouth)
Benefit Fraud Crackdown	High profile inter-agency campaign to catch people who were working and claiming	November 1996	Hull	-

The number of overlapping initiatives in the Project Work and comparison offices underlines the difficulty of isolating the effects of any single programme in a context where a great deal of related innovation was taking place simultaneously. In addition, certain local offices appeared to be chosen regularly to take part in pilot programmes. This raises the question of the extent to which the apparent impact of any new initiative was attributable solely to the content of the initiative, and the extent to which it was attributable to the special characteristics of the pilot offices and to the side effects of participation in pilots, for example on the motivation of local ES officers and on office record-keeping practices.

Selection into Project Work

As we have seen, Project Work was intended to embrace everyone registered at the pilot offices who had already been claimant unemployed for two or more years at the start of the programme in April 1996, or who reached the two year point during the course of the pilots. Clearly not all eligible people could be brought into the programme immediately, and it was planned to spread out the intake over a period of months. Nevertheless it was

intended that the entire stock of people who had been claimant unemployed for two or more years would have entered Project Work by the end of the pilots, along with all the flow of unemployed people reaching the two year point. This was obviously essential to avoid the unfairness of compelling some people to go on work experience while allowing others who had been claimant unemployed for equally long to escape compulsion.

Unfortunately no central record was kept of whether all eligible unemployed people were actually referred to the programme, and there may have been biases in the pattern of referrals. We are unable to investigate this question directly because the administrative data made available to PSI related only to people who had actually entered Project Work. However an analysis by Nicky Tarry of the ES suggests that, in the additional pilot offices to which Project Work was extended in 1997, there may have been a net under-referral of two groups of eligible claimants: those flowing into two years unemployment during the course of the pilots, and those who had been claimant unemployed for five years or more.[2] However this pattern was not constant over time: referrals of clients who had been unemployed for a very long time were concentrated towards the end of the extended pilots, so these people were under-represented amongst referrals in the early months of the pilots and over-represented amongst referrals in the later months. Conversely, clients who flowed into two years unemployment during the life of the extended pilots were under-represented amongst referrals in these later months.

In the first Project Work pilots, there is anecdotal evidence that referrals may have followed a similar pattern over time, though it was believed that everyone in the stock of people unemployed for two or more years was referred by the time the pilots ended. This complicates the interpretation of the evaluation results.

Some implications

The issues discussed in the preceding sections make the interpretation of the results of the evaluation study in some respects problematic.

The new and distinctive feature of Project Work that was not found in any of the comparison offices, at least until the launch of the extended pilots in February 1997, was the 13 weeks of compulsory work experience. However along with this came other elements in the Project Work package that were shared in various forms by different subsets of the comparison offices. This makes it impossible to quantify precisely an effect for Project Work as a whole, or to explore which amongst its various elements were most effective.

The problem is intensified by the fact that the first Project Work pilots were restricted to only two areas, as this makes it very difficult to take proper account of the impact of local labour market conditions. Pilot and comparison offices were supposed to be matched on labour market characteristics, but as we have seen, this matching was not wholly convincing. There were several problems here: office-level variables were used rather than variables relating to the wider TTWA, the matches were based on data relating to a single point in time and took no account of trends, there were difficulties in securing the co-operation of the offices first selected, and the whole process was not properly documented.

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[2] 'Duration bias in Project Work inflow' (communication from Nicky Tarry of the ES), Oct. 1998.

Finally, we have no information on the ratio of referrals to eligible clients, and there may have been patterns over time in this ratio that affected the evaluation results.

We will see in the remainder of this report that in addition to these issues, there were also serious problems with the quality of the data from the two surveys, and some difficulties with the administrative data used in the analysis. All these factors make it necessary to be very cautious indeed when drawing conclusions about the effectiveness of Project Work.

Plan of the report

The next chapter describes the design of the main face-to-face survey of people from the pilot offices who were eligible for Project Work and people registered at the comparison offices whose length of claim would have qualified them for Project Work had the programme been available there. It presents statistical models for the differences between the pilot and comparison offices in the probability of getting work and leaving the unemployment register. Chapter 3 examines the survey evidence on differences between people in the pilot and comparison offices in the types of jobs they took and in their earnings in paid work, while Chapter 4 looks at the evidence on differences in job search behaviour.

Chapter 5 reports on the leaver survey, which involved a short interview with those people in the pilot offices eligible for Project Work and their counterparts in the comparison offices who signed off the unemployment register. It presents models for the differences between the pilot and comparison offices in the economic activity status and earnings of register leavers.

Chapter 6 presents the administrative data set derived from claims for unemployment benefit and the Project Work training allowance that was assembled for all people registered at the pilot offices who were actually enrolled on Project Work and for people registered at the comparison offices whose length of claim would have qualified them for Project Work. It gives descriptive statistics for patterns in the pilot and comparison offices. Chapter 7 uses this same very large data set to develop statistical models of the impact of Project Work on claims for unemployment related benefits and on the probability of being in a job.

Because the evaluation uses evidence from three different sources, and because not all parts of this evidence are consistent with each other, we have deferred drawing any conclusions about the impact of Project Work until the final chapter. Here results from the main survey, the leaver survey and the administrative data are set alongside each other, and we consider what overall inferences can be drawn and how secure these inferences are. This final chapter also makes recommendations about the design of future evaluation studies.

2 Impact of Project Work on economic activity: main survey evidence

Survey design and response

Two separate interview surveys were conducted of people registered at the Project Work pilot offices in Hull and Medway/Maidstone and at the comparison offices. The present chapter is concerned with the *main survey*, while the *leaver survey* is described in Chapter 5. For details of both we draw on SCPR's techincal report (Stratford 1998).

The main survey involved a computer-assisted face-to-face interview lasting about 30 minutes. The sample consisted of all people in the pilot offices who became eligible for Project Work in the weeks commencing May 20 1996 to July 8 1996 (the pilots having begun in April 1996), plus equivalent people in the comparison offices. Eligibility was defined as being already claimant unemployed for over two years, or reaching the two year point during the sampling period, and entry to the sample was triggered by the issue of an invitation from the local employment office to attend the regular six-monthly Restart interview.

Note that the sample in the pilot offices was drawn from clients who were eligible for Project Work, not from those actually enrolled on the programme. As we saw in Chapter 1, it is possible that some eligible clients, particularly those who had been unemployed for a very long time, were not actually enrolled until a later Restart interview. At the time the survey was designed it was assumed that everyone would be enrolled as soon as they became eligible, and so no information was collected on the date of enrolment. It is possible that some sample members were interviewed before they had been enrolled, though we cannot say how many people this affected.[3]

According to the original survey design, an interview should have been triggered either when sample members left the unemployment register, or when seven months had passed from first becoming eligible for Project Work (or from the equivalent duration of unemployment in the comparison offices), whichever was the earlier. The seven month period was chosen as it gave sample members from the pilot offices time to complete both Part A and Part B of Project Work, with a margin to allow for administrative procedures to be completed. It was of course based on the assumption that eligible clients would be referred to the programme without delay, which, as we have seen, may not always have been the case.

Once a trigger event had occurred, the original intention was to interview sample members within 13-16 weeks, so that the last interviews should have been completed by spring 1997. In fact, the ES issued the names and addresses of sample members to the fieldwork agency SCPR in a sporadic fashion. Stratford (1998) says, 'there were delays in SCPR receiving the sample as well as fundamental problems with the sample itself… At one point fieldwork had to be suspended as the wrong criteria were being used to select the sample sent to

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[3] At interview, 75 per cent of respondents from the pilot offices said that they were offered 'extra help with looking for work for up to 13 weeks', or that they had a 'meeting with an advisor to talk in detail about the work experience part of Project Work'. Thus at worst, 25 per cent of respondents were interviewed before they went on Project Work. The true figure is likely to be smaller than this, as it is not unusual for unemployed people to be confused about the many different programmes they are told about, and to fail to recall details about them.

SCPR'. The effect of these problems was that the interval between leaving the register and the survey interview was often considerably longer than 13 weeks, and indeed a substantial number of interviews did not take place until the autumn of 1997. By this time the extended Project Work pilots described in the previous chapter had already been running for several months in four of the comparison offices for Medway/Maidstone, and it may be assumed that some members of the comparison sample in these areas had themselves been enrolled on Project Work. However they were not asked about this in the survey, as when designing the comparison sample questionnaire, SCPR had (not unreasonably) assumed that the ES would not introduce the programme that was being evaluated into the chosen comparison offices.

The survey was also beset by other problems. The overall response rate was only 49 per cent - 47 per cent for the pilot sample and 50 per cent for the comparison sample. This level of response is not generally considered high enough to generate reliable and representative findings. As Table 2.1 shows, response was considerably lower in Medway/Maidstone pilot and comparison samples than in the Hull pilot and comparison samples. The best response rate - that for the Hull comparison sample - was only 55 per cent.

Table 2.1
Main survey response rates

	Pilot		Comparison	
	N	%	N	%
A: MEDWAY/MAIDSTONE				
Total addresses issued by ES in scope	644	100	544	100
Unproductive addresses	382	59	308	57
Of which:				
No contact	*150*	*23*	*72*	*13*
Refusals	*87*	*13*	*98*	*18*
Moved away	*128*	*20*	*126*	*23*
Other unproductive	*17*	*3*	*12*	*2*
Productive addresses	262	41	236	43
B: HULL				
Total addresses issued by ES in scope	716	100	807	100
Unproductive addresses	337	47	362	45
Of which:				
No contact	*32*	*4*	*49*	*6*
Refusals	*89*	*12*	*96*	*12*
Moved away	*212*	*30*	*207*	*26*
Other unproductive	*4*	*1*	*10*	*1*
Productive addresses	379	53	445	55

Source: Summarised from Stratford 1998, Table 3.1.

Although when the questionnaire was being piloted the interviewers reported meeting with a good deal of hostility from some of the people they approached, in the main fieldwork the biggest source of non-response was not refusals, but the failure to trace sample members. One quarter of the entire sample appeared to have moved from the address that had been issued by the ES, without leaving an address where they could be found. There are probably several reasons for this high level of mobility. In part it reflects the demography of the long-term unemployed, who, compared to the employed population, include more young people, more single and rootless people, and more people living in short-term accommodation such as bedsitters, hostels, squats and temporary berths with friends or relatives. In part also the high proportion of movers in the sample resulted from the delays in issuing the sample and from the temporary suspension of fieldwork (see above), so that in some cases the addresses were already several months old by the time the interviewer knocked at the door. There was also likely to have been a degree of deliberate concealment from anyone with an official air who might be mistaken for a debt collector, a DSS investigator, or an unwelcome visitor of some other kind. Finally, some people would have moved address because they had found work. Because of all these different possible explanations, the low level of response to the survey introduces biases whose effects are difficult to assess.

These specific problems with the main survey, together with the more general problems discussed in Chapter 1, lead us to believe that the main survey data do not offer a secure base on which to build inferences about the impact of Project Work. In the analyses that follow in this and the following two chapters, this is evident in the low level of explanatory power that characterises our statistical models, and in the presence of certain results that are inconsistent with the existing empirical and theoretical literature. As a result, any inferences must be tentative in the extreme.

Modelling strategy

Logistic regression models were constructed to explore the impact of Project Work on three outcome measures for economic activity status:
- having been in employment at any time since becoming eligible for Project Work (or since reaching the equivalent duration of unemployment that triggered entry to the comparison sample);
- being in employment at the time of interview;
- being off the unemployment register at the time of interview.

For the reasons explained above, the models did not include a separate term to indicate whether sample members from the pilot offices had actually been enrolled on Project Work by the time they were interviewed, but treated the effect of participation in Project Work as equivalent to the effect of being registered at a pilot office. The models may therefore have underestimated the impact of the programme by averaging out its effects over participants and non-participants alike.

As we saw in Chapter 1, the evaluation design relied on comparing the Medway/Maidstone pilot offices with one set of comparison offices selected on the basis of the characteristics of their employment registers, and comparing the Hull pilot offices with a different set of comparison offices. Ideally therefore we would fit separate models for the two groups of offices, but small sample numbers (especially in Medway/Maidstone) preclude this as a general strategy. As the previous chapter showed, the two sets of comparison offices were

very different from each other, and it is questionable whether it is valid to pool their data to form a basis against which to compare the effects of the two pilot offices. Thus four dichotomous variables (M/MPILOT, HULLPILOT, M/MCOMP and HULLCOMP) were constructed to indicate to which of these groups the respondent belonged, and several different models were fitted to show the effects of different definitions of the base against which the effects of Project Work were assessed.

As well as these programme variables, the models also included variables for a number of other factors which were associated with economic activity status, and which functioned as control variables for the Project Work effect. They included measures of work history, job search behaviour, and personal and household characteristics. In extensive exploratory modelling, we looked at the effects of many other potential control variables (such as gender), but only those that were statistically significant were included in the final versions of the models. To aid comparability, the same set of control variables was used in all versions of Model 1, and was also used in Models 2 and 3, which examined other aspects of economic activity status. The control variables are listed in Table 2.4 and their effects are discussed later in the chapter.

The names and definitions of all the variables used in the statistical models based on the main survey data can be found in Table A1.1 in Appendix 1 to this report.

Table 2.2 reports the estimates of the effects of Project Work - the 'programme effects' - under different specifications of the models. These models fell into two groups. Versions A and B were based on the full main survey sample, while the last two models split the sample into data for Medway/Maidstone and its comparison offices (version C) and data for Hull and its comparison offices (version D). The advantage of splitting the sample was that it avoided the assumption that the control variables had the same effects in both the Medway/Maidstone and Hull groups of offices; the disadvantage was that it reduced sample numbers.[4] As some respondents had missing values on some control variables, the sample size for the models based on the full sample fell from 1,322 (the total number of productive interviews) to 1,104. For the models based on the split samples, the sample sizes were 403 for Medway/Maidstone and 701 for Hull.

Whether got work at all: programme effects

The first of our three outcome measures for economic activity status was based on a survey question asking whether respondents had done any paid work since the long spell of claimant unemployment that made them eligible for Project Work or for inclusion in the comparison sample. Respondents were scored 1 if they had worked since then and 0 otherwise (regardless of whether they were still in work at the date they were interviewed), and logistic regression models were constructed to explain the variation in this measure. These are shown as Models 1A to 1D in Table 2.2. The interest here was in whether respondents from the pilot offices were more likely to have worked than respondents from the comparison offices. Note that what we have termed Models

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[4] Another way of allowing the effects of the control variables to differ between the two groups of offices is to fit interaction terms, but this can be cumbersome and limited by sample size if the number of possible interactions is large.

Table 2.2
Table 2.2
Main survey logistic regression models for economic activity status: programme effects
under different specifications of the base category (Models 1A-D, 2A-D and 3A-D)

	Model 1 Some work since qualifying spell of unemployment *coefficient*	Model 2 In work at interview *coefficient*	Model 3 Not on unemployment register at interview *coefficient*
I MODELS BASED ON THE FULL DATA SET			
Specification of base category:			
A: Comparison offices for Hull			
Constant	-0.22	-0.09	0.38
M/MPILOT	0.54***	0.79****	0.64***
HULLPILOT	0.35*	0.46**	0.68****
M/MCOMP	0.12	0.39	0.44**
B: Comparison offices for M & M			
Constant	-0.10	0.31	0.82
M/MPILOT	0.42*	0.40	0.20
HULLPILOT	0.23	0.06	0.24
HULLCOMP	-0.12	-0.39	-0.44**
Log-likelihood (Versions A & B)	*-573*	*-483*	*-667*
Degrees of freedom (Versions A & B)	*1086*	*1086*	*1086*
II MODELS BASED ON THE SPLIT SAMPLES			
Specification of base category:			
C: Comparison offices for M & M			
Constant	0.27	1.13	1.82
M/MPILOT	0.45**	0.44*	0.26
Log-likelihood	*-217*	*-191*	*-245*
Degrees of freedom	*387*	*387*	*387*
D: Comparison offices for Hull			
Constant	-0.33	-0.76	-0.13
HULLPILOT	0.38*	0.49**	0.68****
Log-likelihood	*-404*	*-330*	*-411*
Degrees of freedom	*685*	*685*	*685*

Significance levels: * 10%; ** 5%; *** 1%; **** 0.1%.

1A and 1B were in fact the same model: the difference between them was purely presentational, the coefficients for Model 1B being a simple mathematical function of the coefficients for Model 1A.[5]

In Model 1A, the Medway/Maidstone pilot offices, the comparison offices for Medway/Maidstone, and the Hull pilot offices were all compared to the comparison offices for Hull, but it was only the comparison with the Hull pilot offices that is of interest. In Model 1B, all comparisons were made relative to the comparison offices for Medway/Maidstone, but it was only the comparison with the Medway and Maidstone pilot offices that was of interest. Model 1A showed that people in the Hull pilot offices were more likely to have worked than people in the comparison offices for Hull, though the difference was of only marginal statistical significance. Model 1B showed that the Medway/Maidstone pilot offices fared better than their comparison offices, but the difference was again of only marginal significance.

Models 1C and 1D split the sample into two, giving two completely separate models. Model 1C indicated that people in the Medway/Maidstone pilot offices were more likely to have worked than people in the comparison offices for Medway/Maidstone. The programme effect was similar in size to the coefficient for the Medway/Maidstone pilots in Model 1B (which took the comparison offices for Medway/Maidstone as the base), but this time it reached a more acceptable level of significance. Model 1D, for Hull and its comparison offices, also indicated a positive effect for the pilot offices, and again, the coefficient was similar to the coefficient for the Hull pilot offices in Model 1A (which took the comparison offices for Hull as the base). However in this case the effect remained only marginally significant.

This somewhat laborious exercise suggested that people in the pilot offices were more likely to have worked than people in the comparison offices, but that the effect was more secure in Medway/Maidstone than in Hull.

The interpretation of these results is problematic. As we saw in Chapter 1, the comparison offices for both Hull and Medway/Maidstone were both economically and geographically diverse. However Medway/Maidstone had shown a bigger fall in the TTWA unemployment rate over the year before the start of Project Work than any of its comparison offices, while four of the six comparison offices for Hull had higher TTWA unemployment rates than Hull (see Table 1.1). This suggested that differences in initial local labour market conditions could have been at least partly responsible for the differences we found in people's chances of getting a job.

As we argued in Chapter 1, the evaluation design made it impossible to take proper account of local labour market conditions in assessing the impact of Project Work, as there was no satisfactory way of separately estimating the effects of Project Work and of the local labour market. Further exploratory modelling shown in Table 2.3 indicated that local labour market conditions could indeed have contributed to the apparent Project Work effects.

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[5] For example, in 1A the effect of the Hull pilot offices relative to the base category, the comparison offices for Hull, is estimated at 0.35 (the coefficient for the base category being set to zero). In 1B, the difference between the estimates for Hull pilot offices and the comparison offices for Hull is also 0.35 (0.23 minus –0.12).

In order to try to reduce problems of multicollinearity, we combined the comparison offices for Hull and Medway/Maidstone, giving Model 1E. When included in Model 1E alongside the programme variables, none of a number of variables capturing local labour market conditions proved significant. However, dropping the programme variables and substituting three of these local labour market variables (TTWA unemployment rate June 1996, change in TTWA unemployment rate June-December 1996, and change in notified vacancies June –December 1996) produced a model of the same explanatory power as Model 1E. We also found that including one of these variables, change in TTWA unemployment rate June-December 1996 (UNEMDIFF), alongside the programme variables reduced the apparent Project Work effects to statistical non-significance, although UNEMDIFF itself remained non-significant (see

Table 2.3
Main survey logistic regression models for economic activity status: programme effects with predictor variable for local labour market conditions and pooled comparison sample
(Models 1E & F, 2E & F and 3E & F)

	Model 1 Some work since qualifying spell of unemployment	**Model 2** In work at interview	**Model 3** Not on unemployment register at interview
	coefficient	*coefficient*	*coefficient*
E: Without labour market controls			
Constant	-0.16	0.11	0.58
M/MPILOT	0.50***	0.64***	0.48***
HULLPILOT	0.31*	0.31	0.53****
Log-likelihood	*-573*	*-484*	*-670*
Degrees of freedom	*1,087*	*1,087*	*1,087*
F: With labour market controls			
Constant	-0.24	0.03	0.55
M/MPILOT	0.35	0.50**	0.42**
HULLPILOT	0.20	0.21	0.48*
UNEMDIFF	-0.16	-0.16	-0.06
Log-likelihood	*-572*	*-484*	*-670*
Degrees of freedom	*1,086*	*1,086*	*1,086*

Model 1F in Table 2.3).[6] However this model was not satisfactory because of multicollinearity due to the correlation between UNEMDIFF and the programme variables.[7]

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[6] UNEMDIFF was negative if the unemployment rate in December was lower than the unemployment rate in June. Thus the negative sign for the coefficient indicated, as we would expect, that the greater the fall in the unemployment rate, the greater the chances of having worked.

[7] The coefficients for the other control variables in these models were similar to those shown in Table 2.4 below.

Whether got work at all: control variables

We turn now to the control variables in the models for whether respondents had worked at any time since the long spell of claimant unemployment that made them eligible for Project Work or qualified them for inclusion in the comparison sample. As mentioned above, the same set of control variables was used for all versions of Models 1, 2 and 3. Table 2.4 reports the estimates from versions A and B of these models.[8] Most of these control variables had the familiar effects that were expected from standard labour market theory.

The first group of control variables described people's work histories. The first two came from a survey question asking respondents to choose the description that best

Table 2.4
Parameter estimates for the control variables in versions A and B of Models 1 to 3.

	Models 1A & 1B Some work since qualifying spell of unemployment *coefficient*	Models 2A & 2B In work at interview *coefficient*	Models 3A & 3B Not on unemployment register at interview *coefficient*
Work history			
WHSTEADY	0.54***	0.62***	0.07
WHCASUAL	0.33*	0.29	0.09
LASTJMANF	0.42**	0.48**	0.22
LNGTHUNEM	-0.01****	-0.01***	-0.01***
Job search			
NUMMETHS	0.07*	0.09**	0.06*
ACCPTTEMP	0.74****	0.60***	0.51***
MAXHOURS	-0.02***	-0.05****	-0.03
RESNWAGE	-0.29****	-0.39****	-0.27****
RANGEJOBS	-0.54***	-0.56***	-0.34**
Personal			
AGE45UP	-0.72****	-0.63***	-0.27
DRVNGLIC	0.52***	0.46**	0.37***
Household			
MARRIED	0.82****	1.23****	0.83****
OWNEROCC	0.48**	0.60***	0.33*
OTHEREMP	0.56***	0.59**	0.30

*Significance levels: * 10%;   ** 5%;   *** 1%;   **** 0.1%.*

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[8] It will be recalled that versions A and B were in fact the same model with the results presented in a different way.

applied to their own experience of work, thinking about the whole of their adult life.[9]
Spending most of one's working life in steady jobs (WHSTEADY) increased the chances of
having some work after the qualifying spell of unemployment. Having mainly done casual
or short-term work (WHCASUAL) also increased the chances of working, though the effect
was smaller and only marginally significant. People who had worked before their
qualifying spell of unemployment were asked for details of their last job, and this was
classified according to the Standard Industrial Classification (SIC). Those who had worked
in manufacturing industry (LASTJMANF) were more likely to have had a subsequent job,
compared to people whose last job had been in other industries or who had not worked at
all. The last significant variable amongst the various aspects of work history on which data
were collected was the length of the qualifying spell of unemployment (LNGTHUNEM), a
continuous variable measured in months. The longer the qualifying spell of unemployment,
the less likely people were to get a job, even though everyone in the sample had been
unemployed for at least two years.

The next group of control variables measured differences in the way that respondents
looked for jobs. For respondents who were unemployed at the time of interview, these
variables related to the four weeks immediately before interview, while for those who were
in work at interview, they related to the four weeks immediately before they got a job.[10]
People who said they were willing to accept temporary or fixed-term jobs as well as
permanent ones (ACCPTTEMP) were more likely to get work. Respondents were asked
how many of a list of eleven job search methods they had used (NUMMETHS), and the
more different methods they had used, the more likely they were to have got work. They
were also asked about the maximum number of hours per week that they were willing to
work (MAXHOURS). The lower this figure, the more likely they were to get work,
suggesting that part-time jobs were easier to find than full-time jobs. White, Lissenburgh
and Bryson (1997) found a similar effect in their analysis of ES job placing programmes.
Respondents' reservation wage (RESNWAGE), defined as the lowest hourly pay that they
were willing to work for, was also related to job chances: the lower people's wage
expectations, the greater their chances of getting work. The last of the significant job search
control variables referred to the breadth of the search: respondents were asked whether they
were looking for a particular type of job, a range of jobs, or any job they could do. The
model suggests that people who said they were looking for a range of jobs (RANGEJOBS)
were less likely to get work than those who were looking either more narrowly or more
broadly, though the reason for this is unclear.

The third group of control variables in Table 2.4 captured personal characteristics. Only
two proved significant. Respondents who were aged 45 or over (AGE45UP) were less
likely to have worked than younger people (remembering that no sample members were
over 50 at the start of the study). In addition, people who had a driving licence
(DRVNGLIC) were more likely to get a job than those without. Gender had no significant
effect on the chances of having had a job in this data set, though women formed only about

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[9] The list from which respondents were asked to choose was as follows: I have spent most of my
working life in steady jobs; I've mainly done casual or short-term work; I've spent a lot of time out of
work due to sickness/injury; I have spent most of working life self-employed; I've spent more time
unemployed than in work; I have been in work, then out of work, several times over; I have spent a lot
of my adult life looking after the home and family; I have spent a lot of my adult life in full-time
education; None of these apply to me.

[10] There is a degree of endogeneity in the main survey between the reported level of job search
activities and the probability of securing a job which may in part underlie the associations shown in
Table 2.4; see Chapter 4 for further explanation.

15 per cent of the sample. Similarly, neither academic nor vocational qualifications appeared to affect job chances.

The last group of control variables related to the characteristics of the respondent's household. Respondents who were living with a spouse or partner (MARRIED) were more likely to have found work than single or previously partnered people. Owner-occupiers (OWNEROCC) were more likely to get work than tenants or people with other forms of housing tenure. In addition, people living in a household where there was an adult in employment other than their spouse or partner (OTHEREMP) were more likely to get work than people in households without another employed adult.

As we saw in Table 2.4, job search variables were significant predictors of the probability of getting a job. However it could be argued that any impact of Project Work on job chances was partly mediated through its impact on job search.[11] Not only did the first 13 weeks on Project Work give optional intensive help with job search, but in addition, Ritchie and Legard (1997) provide evidence that the prospect of 13 weeks of compulsory work experience acted as a spur to some clients to step up their job search. By including measures of job search behaviour as control variables, we may therefore have underestimated the impact of the programme. [12]

To test this hypothesis we refitted version E of our model excluding the job search control variables, and Table 2.5 reports the estimates for the programme effects under this specification. Comparing these estimates with the estimates reported for Model 1E in Table 2.3, we see that while the coefficient for the Hull pilot offices was increased slightly (from 0.31 to 0.40), the coefficient for the Medway/Maidstone pilot offices, contrary to expectation, was reduced by a similar amount (from 0.50 to 0.41). As there are technical reasons for expecting the programme effects to increase when any block of significant control variables is removed,[13] this exercise offers no consistent evidence that Project Work had an effect on the probability of getting a job via an effect on job search.

Table 2.5
Main survey logistic regression models for economic activity status: programme effects with control variables for job search omitted from version E (Models 1G, 2G and 3G)

	Model 1G	Model 2G	Model 3G
	Some work since qualifying spell of unemployment	In work at interview	Not on unemployment register at interview
	coefficient	*coefficient*	*coefficient*

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[11] We are grateful to Nicky Tarry of the ES for this suggestion.

[12] There was indeed a positive correlation between the programme variables and NUMMETHS, though not big enough to produce multicollinearity in the models.

[13] In any regression model there is low-level multicollinearity, so that when some predictor variables are excluded, the remaining predictor variables can be expected to explain more of the variation in the dependent variable. In the present case, the explanatory power of the model was not very great, and so any increase in the apparent programme effects consequent on removing the job search variables can be seen as a spurious result of under-specifying the model.

Constant	-1.18	0.03	-0.83
Programme effects:			
M/MPILOT	0.41**	0.45**	0.29*
HULLPILOT	0.40**	0.43**	0.56****
Log-likelihood	*-691*	*-600*	*-809*
Degrees of freedom[+]	*1,265*	*1,265*	*1,265*

Note: The sample size was bigger than in version E of the models as people who had missing values on the job search variables were included. We tested for whether this introduced any biases through dummy variables indicating missing information on the job search variables. These were non-significant and their inclusion did not affect the substantive results.

The contrasting impact of removing job search control variables on the programme effects for the Medway/Maidstone and Hull pilot offices indicates once again that the two areas had very different labour markets. The differences between them are underlined in Table 2.6, which shows the coefficients for the control variables in versions C and D of Model 1, the versions based on the split samples for the two areas.

Table 2.6
Parameter estimates for the control variables in versions C and D of Models 1 to 3

	Model 1 Some work since qualifying spell of unemployment		Model 2 In work at interview		Model 3 Not on unemployment register at interview	
	1C M/M *coefficient*	1D Hull *coefficient*	2C M/M *coefficient*	2D Hull *coefficient*	3C M/M *coefficient*	3D Hull *coefficient*
Work history						
WHSTEADY	0.96***	0.22	0.96***	0.28	0.21	-0.08
WHCASUAL	-0.03	0.48**	-0.20	0.50*	-0.59*	0.42*
LASTJMANF	0.41	0.50**	0.33	0.69***	0.17	0.27
LNGTHUNEM	-0.01***	-0.01***	-0.01**	-0.01**	-0.01**	-0.01**
Job search						
NUMMETHS	0.00	0.12**	0.00	0.17***	-0.02	0.10**
ACCPTTEMP	0.51*	0.91***	0.37	0.82***	0.30	0.64***
MAXHOURS	-0.02	-0.04***	-0.05***	-0.05***	-0.03**	-0.04***
RESNWAGE	-0.40***	-0.24**	-0.43***	-0.37***	-0.43***	-0.16*
RANGEJOBS	-0.18	-0.78***	-0.20	-0.80***	-0.21	-0.43**
Personal						
AGE45UP	-0.50*	-0.94***	-0.37	-0.88***	-0.21	-0.29
DRVNGLIC	0.35	0.67***	0.23	0.66***	0.43*	0.38**
Household						
MARRIED	1.08***	0.66***	1.58***	1.01***	0.86***	0.81***
OWNEROCC	0.15	0.75***	0.25	0.91***	0.20	0.45*

OTHEREMP	0.61	0.49*	0.83**	0.41	0.58*	0.10

Significance levels: * 10%; ** 5%; *** 1%; **** 0.1%.

Sample numbers for the Medway/Maidstone pilot offices and their comparison offices (403) were smaller than for the Hull sample (701), and this could explain why there were fewer significant coefficients in Model 1C than Model 1D. However there were also differences in the pattern of significant results between the two samples. The first point to note is that in Medway/Maidstone and its comparison offices, having a history of steady jobs significantly increased the probability of getting a job, while having done mostly casual or temporary work had no significant effect. In contrast, in Hull and its comparison offices, the probability of getting a job was increased by a history of casual or temporary work, while a history of steady jobs had no significant effect. The explanation for this contrast is evident in Table 1.3 in Chapter 1, which shows that at the start of the Project Work pilots, temporary vacancies formed more than one in four of all notified vacancies in the Hull TTWA, compared to one in seven in the Medway/Maidstone TTWA.

The second point of contrast in the models for the split samples lies in the coefficients for the job search variables. Four of this group of five variables (RANGEJOBS, NUMMETHS, ACCPTTEMP and HRSWANTS) were much more powerful predictors in the Hull sample than the Medway/Maidstone sample (which explains why omitting these variables in Model 1G increased the apparent programme effect in Hull but not Medway/Maidstone). For the fifth variable, the reservation wage (RESNWAGE), the pattern was reversed: this had a more powerful effect in the Medway/Maidstone sample than in the Hull sample.[14]

Whether in work at the time of interview

The second set of logistic regression models estimated the impact of the same set of predictor variables upon another binary outcome variable: whether the respondent was actually in work at the time of interview.

Models 2A - 2D in Table 2.2 above indicated that, on this criterion, the Hull pilot offices did significantly better than their comparison offices. The Medway/Maidstone pilot offices also appeared to do better than their comparison offices, but in this case the difference was of only marginal statistical significance. The coefficients for the control variables in these models were similar to the coefficients for the control variables in Models 1A - 1D (see Tables 2.4 and 2.6).

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[14] These differences in the coefficients for the control variables in the split samples sheds light on why the apparent programme effects were slightly bigger, and, in the case of Medway/Maidstone, more significant, in the split samples than in the combined sample. Though in theory these effects should be more significant in the combined sample because of larger sample numbers, if there are a number of interactions between the control and the programme variables, then split samples will provide better fitting models.

Our earlier discussions about the role of local labour market conditions and job search also apply to this second series of models. We do not go through these arguments again here, but interested readers will find the relevant results in Tables 2.3 to 2.6.

Whether off the unemployment register at the time of interview

Our third outcome measure was whether the respondent was off the unemployment register at the time of interview (Model 3 in Tables 2.2 - 2.6). People off the register at this point included not only those who were in work, but also those who were drawing other types of social security benefits and those who were neither in work nor drawing any benefits. The same set of predictor variables was used as in Models 1 and 2.

The models showed that people in the Hull pilot offices were significantly more likely to have left the unemployment register than people in the comparison offices for Hull, but that there was no significant difference between the Medway/Maidstone pilot and comparison offices.

The explanatory power of these models was limited, and several control variables that were significant in Models 1 and 2 were non-significant in Model 3 (see Tables 2.4 and 2.6). Most notably, only one of the four work history variables, UNEMPLEN, was significant. This was probably because of the heterogeneous nature of sample members who were not on the unemployment register. Another notable finding was that while being aged over 45 reduced the probability of getting a job, it did not reduce the probability of leaving the unemployment register. This suggested that a proportion of older sample members moved from unemployment-related benefits to other types of social security benefits (again, remembering that no-one was over 50 at the start of the study).

3   Impact of Project Work on the type of jobs taken: main survey evidence

Introduction

In this chapter we look at the type of jobs obtained by people from the Project Work pilot offices who found work. This issue is relevant to the cost effectiveness of any programme aimed at getting unemployed people off the register, for if people leave to take short-term temporary jobs, then the register impact of the programme may be only short-lived. In Chapters 6 and 7 we use administrative data on unemployment and training benefit claims to examine the permanence of Project Work's impact directly. In addition, if Project Work participants left the unemployment register to take part-time or very low paid jobs, then there may still have been a substantial cost to the Exchequer in terms of in-work benefits. Thus in this chapter we use data from the main survey to look first at whether there was any difference between the pilot and comparison offices in rates of entry to part-time or temporary jobs rather than permanent full-time jobs, and, second, at wage levels in the jobs taken.

Permanent full-time jobs versus part-time or temporary jobs

The first question was addressed through a multinomial logistic regression model for the type of job, if any, that respondents first entered after the spell of unemployment that made them eligible for Project Work or triggered their entry to the comparison sample. This is shown as Model 4 in Table 3.1. The definitions of the variables used in this model (and all the other models in this chapter) can be found in Appendix 1. Instead of a two-way dependent variable as in the simple logistic models presented in Chapter 2, the dependent variable in Model 4 had three mutually exclusive categories: did not enter a job, entered a part-time or temporary job, and entered a permanent full-time job. Part-time was defined as working 8-29 hours per week and full-time as working 30 hours or more per week.

There were three possible comparisons that could be made between these three possibilities: the odds of a part-time or temporary job rather than no job, the odds of a permanent full-time job rather than no job, and the odds of a part-time or temporary job rather than a permanent full-time job. Only two of these three comparisons were modelled, because each one was a simple mathematical function of the other two, and it made no difference to the model which two of the three comparisons were chosen. The coefficients in column (a) of Table 3.1 relate to the (log) odds of entering a part-time or temporary job rather than no job, while column (b) relates to the (log) odds of entering a permanent full-time job rather than no job.

The coefficients for the predictor variables in Model 4 show how they affected these relative odds. For example, the coefficient for being female (FEMALE) in column (a) of Table 3.1 was positive and statistically significant. This means that the relative

odds of entering a part-time or temporary job rather than no job were increased by being a woman. In contrast, the coefficient for FEMALE in column (b) was negative

Table 3.1

Multinomial logistic regression model for the type of job first entered after the qualifying spell of unemployment (Model 4)

	(a) Part-time or temporary job (rather than no job) *coefficient*	(b) Permanent full-time job (rather than no job) *coefficient*
Programme effects		
A: Base category comparison offices for Hull:		
Constant	0.30	-1.57
M/MPILOT	0.93***	0.49
HULLPILOT	0.35	0.57*
M/MCOMP	0.17	0.28
B: Base category comparison offices for M/M:		
Constant	0.47	-1.30
M/MPILOT	0.76**	0.21
HULLPILOT	0.18	0.29
HULLCOMP	-0.17	-0.28
Control variables		
Work history		
WHSTEADY	0.52**	0.45*
LNGTHUNEM	-0.01***	-0.01***
LASTJSOC5	-0.49	0.65**
LASTJSOC8	-0.10	0.80***
Job search		
ACCPTTEMP	0.62**	0.32
MAXHOURS	-0.05****	-0.01
LOOKFT	-0.40	0.92**
RESNWAGE	-0.29**	-0.59****
RANGEJOBS	0.00	-0.92***
HRSSPENT	-0.02	0.04**
NUMMETHS	0.05	0.14**
Personal		
FEMALE	0.88***	-0.82*
HEALTHPRB	0.05	-0.53*
Household		
SPOUSEEMP	0.70*	1.24***
KIDAGE0-2	1.03**	0.59*
KIDAGE3-4	2.14****	1.35****
KIDAGE5-10	1.64****	-0.17
OWNEROCC	0.27	0.61*

Log-likelihood	*-535*
Degrees of freedom	*952*

Significance levels: * 10%; ** 5%; *** 1%; **** 0.1%.

and marginally significant. This means that the relative odds of entering a full-time job rather than no job were reduced by being a woman.[15]

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[15] It is important to note that these effects are on *relative* rather than absolute odds, so it does not necessarily follow from these coefficients that there were more women than men in part-time or temporary employment or that there were more men than women in permanent full-time employment. To take a hypothetical case, imagine that there are two bags each containing 10 balls. In bag A, one ball is yellow, three are pink and six are green. In bag B, two balls are yellow, four are pink and four are green. The odds of drawing a pink rather than a yellow ball out of bag A are 3/1=3, while the odds of drawing a pink rather than a yellow ball out of bag B are smaller: 4/2=2. Thus the odds of drawing a pink rather than a yellow ball are reduced by taking bag B rather than bag A. Nevertheless there are still greater odds of drawing a pink ball (rather than a ball of another colour) out of bag B than out of bag A.

As with versions A and B of Models 1 to 3 in Table 2.2, we present the programme effects from the model in two different ways in order to bring out the comparison between each group of pilot offices and their own specific comparison offices. The estimates in group A show the programme effects relative to the comparison offices for Hull, and those in group B show the programme effects relative to the comparison offices for Medway/Maidstone. These two sets of figures are just two different ways of presenting the same estimates, and the one set is a simple mathematical function of the other.[16]

The programme effects in Model 4 show that people from the Hull pilot offices did not have a significantly better chance of getting a part-time or temporary job (rather than no job at all) than people from the comparison offices for Hull, though they did have a marginally significant advantage in securing permanent full-time jobs. In contrast, people from the Medway/Maidstone pilot offices had a clearly significant advantage in getting part-time or temporary jobs over people from the comparison offices for Medway/Maidstone, but had no significant advantage when it came to full-time permanent jobs.

These results should be seen in the context of all the difficulties of interpretation that were discussed at some length in Chapters 1 and 2. If they are considered meaningful, then they suggest that Project Work increased the proportion of long-term unemployed people getting jobs largely by increasing the numbers taking part-time or temporary jobs.

On the whole, the control variables in Model 4 functioned as expected. As our main focus is on the programme effects, we merely report the model results for these without discussing their interpretation.

Among the control variables that captured work history, a history of steady jobs (WHSTEADY) significantly increased the chances of both part-time or temporary jobs and permanent full-time jobs, while the longer the qualifying spell of unemployment (LENGTHUNEM), the smaller the chances of either. People whose last job was in a craft or related occupation (LASTJSOC5) or who had been plant or machine operatives (LASTJSOC8) had greater relative chances than others of a full-time permanent job rather than no job. Looking next at job search behaviour, the chances of a part-time or temporary job (rather than no job) were significantly increased by a willingness to accept temporary work (ACCPTTEMP), though they decreased as the maximum number of hours that respondents were willing to work (MAXHOURS) increased and as the reservation wage (RESNWAGE) got larger. In contrast, the chances of a permanent full-time job (rather than no job) were significantly bigger for those who were looking for a full-time job (LOOKFT), and the chances also increased with extra hours spent looking for work each week (HRSSPENT) and with the number of job search methods used (NUMMETHS). We found again that people who were looking for a range of jobs rather than one specific

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[16] See footnote 3 on page 20.

job or any job at all (RANGEJOBS) had poorer chances of a permanent full-time job, echoing the result in Table 2.4 in Chapter 2.[17]

Turning next to the control variables capturing personal and household characteristics, women (FEMALE) had greater chances than men of entering a part-time or temporary job and lower chances of a permanent full-time job, while the chances of a permanent full-time job were also reduced by long-term health problems or disability (HEALTHPRB).   Having a spouse or partner in employment (SPOUSEEMP) increased the chances of a part-time or temporary job (rather than no job), but increased the chances of a permanent full-time job even more. The presence of young children (KIDAGE0-2, KIDAGE3-4 and KIDAGE5-10) tended to increase the chances of both part-time or temporary work and permanent full-time work, but the impact on the former was the biggest. Finally, owner-occupiers (OWNEROCC) were more likely to enter permanent-full time jobs than people with other forms of housing tenure.

Wages

The analysis of wages is a very important part of the evaluation of a labour market programme because it is the best way of measuring the programme's wider economic effects. If participants get better paid jobs than they would otherwise have obtained, this can be taken as evidence that the programme leads to higher productivity.

We looked at the variation in net hourly pay amongst those respondents who had been employed after their qualifying spell of unemployment and who provided valid wage data for their first job since that spell of unemployment. The sample size for this model was fairly small, but we were able to use an Ordinary Least Squares (OLS) model because the hourly wage variable was normally distributed.[18] The results of the analysis are reported as Model 5 in Table 3.2. The explanatory power of this model was limited, with an adjusted R^2 of 0.15, and only a small number of variables had a statistically significant impact on pay.

Just as with Model 4, we present the programme effects in two ways, first taking the comparison offices for Hull as the base category and then taking the comparison offices for Medway/Maidstone as the base category. The results show that, once other factors

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[17] It is possible that Project Work affected the type of job taken partly through its impact on job search, so that the programme effects in Model 4 underestimate the true impact of the programme. However the analyses presented in the previous chapter suggest that any such effect was probably fairly small.

[18] We followed the standard practice of using the natural logarithmic transformation of net hourly wages as our dependent variable. The use of a natural logarithm as the dependent variable increases the efficiency of estimation by increasing the extent to which the variable follows a Gaussian (normal) distribution, while it also enables the coefficients to be interpreted as approximate elasticities. We included people who had worked since the qualifying spell of unemployment but were no longer in work at the time of interview. However the wages for those no longer in work were not uprated for inflation as the time since they left their job was at most a few months.

had been controlled for, respondents from the Medway/Maidstone pilot offices were estimated to earn 21 per cent more than respondents from the Medway/Maidstone

Table 3.2
Main survey OLS regression model for net hourly pay in first job after the qualifying spell of unemployment (Model 5)

	coefficient
Programme effects	
A: Base category comparison offices for Hull:	
Constant	1.14
M/MPILOT	0.23***
HULLPILOT	-0.02
M/MCOMP	0.04
B: Base category comparison offices for M/M::	
Constant	1.17
M/MPILOT	0.19**
HULLPILOT	-0.05
HULLCOMP	-0.04
Control variables	
Job characteristics	
UNION	0.21***
SELFEMP	-0.30****
CONSTRCTN	0.17**
Personal	
VOCQUALS	0.16***
ACADQUALS	-0.12**
Sample selection variable	
LAMBDA	-0.08
Adjusted R^2	*0.15*
Degrees of freedom	*252*

*Significance levels: * 10%;   ** 5%;   *** 1%;   **** 0.1%.*

comparison offices.[19] However the earnings of respondents in Hull were not significantly different from those of people from the comparison offices for Hull. Because of issues discussed in Chapter 1, it is not possible to say whether this was because Project Work as implemented in Medway/Maidstone but not as implemented in Hull had a positive effect of on wages, or whether the result simply reflected local differences in wage levels, and more specifically the fact that the Medway/Maidstone pilot offices were closer to London (with its higher wage rates) than their comparison offices.

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[19] $e^{0.19} = 1.21$.

Following our usual practice, we merely report the effects of the control variables without discussing their interpretation in detail. Though only a few job characteristics influenced pay in this model, their effects were plausible. Where there was a recognised union at the workplace (UNION), pay was estimated to be increased by 23 per cent. Pay was estimated to be reduced by 26 per cent for self-employed workers (SELFEMP), but increased by 19 per cent for those employed in the construction industry (CONSTRCTN). Only two personal characteristics were found to have significant effects in this model: as expected, the possession of vocational qualifications (VOCQUALS) increased pay by an estimated 17 per cent, but contrary to expectation, pay was significantly depressed (by 11 per cent) by the possession of academic qualifications. Note that in this sample, academic qualifications were on average fairly low, and anything from CSE upwards was counted as such. No job search variables were significant in this model.

A LAMBDA variable, computed from a probit model (not shown) estimating the influences on job entry and reflecting the importance of unmeasured characteristics in explaining job entry, was introduced as part of the standard procedure for the elimination of sample selection bias (Heckman, 1979). This variable was non-significant.

A number of other variables which are typically important in wage models, such as gender and age, were non-significant in this model. Along with the low explanatory power of the model and the counter-intuitive finding on academic qualifications, this suggests that we should be cautious when interpreting the results of the wages model.

4 Impact of Project Work on job search: main survey evidence

Introduction

As Chapter 1 explained, the first 13 weeks of Project Work, Period A, consisted of optional intensive help with job search primarily through a series of optional 1-2-1 interviews with client advisers. Period B, the further 13 weeks of compulsory work experience, also included three hours per week of assisted job search. According to standard labour market theory, more intensive and efficient job search on the part of long-term unemployed people should both increase the chances of securing a job and improve the suitability of the match between worker and job. In turn this should both increase productivity and lengthen the time that the worker stays in the job. The longer-term impact of Project Work on job search is thus of some interest.

In the main survey, information on job search was collected retrospectively in relation to one of two periods:
- for those who were unemployed at the time of interview, the last four weeks before interview;
- for those who were in work at the time of interview, the last four weeks before signing off.
We refer to these two periods together as 'the job search period'. Note that for people enrolled on Project Work who got a job at any time between one and six months after their enrolment, the whole of this job search period was spent on Project Work, a time when they had particular incentives to show evidence of an active job search. For people who either did not secure a job or who secured a job more than four weeks after the end of Project Work, the whole of the job search period fell after the end of Project Work, when they had less incentive to provide such evidence. Thus there is a degree of endogeneity in our data between the level of job search and the probability of getting a job, and this may form some part of the explanation for the strong associations between job search and job chances shown in Models 1 and 2 in Chapter 2.

Three models were developed to explore the association between Project Work and job search. These examined the time spent looking for work, the number of job applications and the number of job search methods used.

Time spent looking for work

The number of hours that respondents spent each week in looking for work approximated to a normal distribution (mean value nine) and so an OLS regression model was used for this analysis. This is reported as Model 6 in Table 4.1.

The programme variables in Model 6 show us that long-term unemployed people from the Project Work pilot offices in Medway/Maidstone spent

significantly more time searching for work than long-term unemployed people in the pooled comparison sample. However for people from Project Work pilot offices in Hull this effect was reversed: they spent significantly *less* time than members of the comparison sample. There are

Table 4.1
Main survey OLS regression model for hours per week
spent looking for work (Model 6)

	coefficient
Constant	7.30
Programme variables	
(relative to pooled comparison sample)	
M/MPILOT	1.46***
HULLPILOT	-1.80****
Control variables	
Work history	
WHCASUAL	-1.03**
Job search	
PARTICJOB	-1.55****
Personal	
AGE	0.08****
FEMALE	-1.36**
Household	
NCHILD1	0.22
NCHILD2	0.49
NCHILD3	1.20*
Adjusted R^2	*0.49*
Degrees of freedom	*1161*

*Significance levels: * 10%;   ** 5%;   *** 1%;   **** 0.1%.*

several possible explanations for this contradictory finding. Firstly, it is possible that the coefficient for the Medway/Maidstone pilot offices reflects a true effect of Project Work upon job search, and that the way in which job search assistance was delivered was much more effective in Medway/Maidstone than in Hull. This would be a curious interpretation as the two groups of offices were following similar guidelines. A second, more plausible explanation relates to the differences in the structure of the local labour markets in the two areas. Hull is relatively isolated geographically and the possibilities of spending a lot of time on looking for jobs were therefore limited, whereas people in Medway/Maidstone could explore the much larger labour markets of the Greater London area. A third possible explanation is in terms of the endogeneity between the measures of job search intensity and the probability of getting a job that was explained in the introductory section of this chapter. As sample members in Medway/Maidstone moved into jobs more quickly than their counterparts in Hull, the defined job search period was more likely to fall during their time on Project Work and so the level of their job search activity was more likely to be boosted by the requirements of the programme. However this explanation fails, as a further model (not shown) that was restricted to

people not in employment at the time of interview produced similar results to those reported above.

Turning now to the control variables in Model 6, we see that respondents who had done mainly casual or short-term work in the past (WHCASUAL) spent significantly less time searching than others. This may have been because casual work is often obtained through informal contacts rather than through systematic search. Those who were looking for a particular job (PARTICJOB), rather than for a range of jobs or for any job, also spent less time on job search, presumably because their search was more focused. Women also spent less time looking for work than men, possibly because they were more likely to restrict their search to within easy travelling distance of home. These results are consistent with expectations and easy to explain, but others are more puzzling. Age for example (AGE) seemed to have a positive effect on job search, as did the number of children (NCHILD1, NCHILD2 and NCHILD3). There is no obvious reason why this should be the case and, if anything, one might expect these to have negative effects as these factors may limit individual geographical mobility. The presence of these results should perhaps encourage us to be cautious about attaching too much weight to the overall model findings.

Number of job applications

Perhaps a better measure of job search effort than the number of hours spent each week in looking for work is provided by the number of job applications that respondents made during the job search period. This variable was not normally distributed, so an OLS regression model was not appropriate. Its distribution showed that the sharpest distinction was between those who had made no applications (19 per cent) and those who had made at least one (81 per cent). Thus a logistic regression model was constructed with the binary outcome variable set to 1 if the respondent had made one or more applications and 0 if they had made none. This is reported as Model 7 in Table 4.2.

The coefficients for the programme variables in this model show that, compared to members of the pooled comparison sample, people from both the Medway/Maidstone and Hull pilot offices were more likely to have made at least one job application during the job search period. As explained in the introduction to this chapter, for some members of the pilot samples the job search period fell during their time on Project Work. Thus we cannot say whether this increased rate of job applications represented a long-term change in behaviour that continued after the end of the programme.

Similar programme effects were obtained from a multinomial logistic regression model (not shown) with a three-way outcome variable distinguishing between respondents who had made no job applications during the job search period, those who had made only one, and those who had made more than one. Likewise, other models (also not shown) that were restricted to those who were not working at the time of interview gave very similar results.

The coefficients for the control variables in Model 7 were all in accordance with expectations. The work history group of variables showed that a relatively disadvantaged employment background decreased the probability of making a job application. Thus people who said they had spent more time unemployed than in work (WHUNEMP) were less likely than others to have made an application. The same was true of people who had last worked as plant or machine operatives (LASTJSOC8), while the probability of making an application fell as the length of the qualifying spell of unemployment increased (LNGTHUNEM). These findings are consistent with the view that people with poor employment records tend to become discouraged and to lack confidence in their search for jobs. Not surprisingly, the more

Table 4.2
Main survey logistic regression model for whether made
at least one job application (Model 7)

	coefficient
Constant	1.70
Programme variables	
(relative to pooled comparison sample)	
M/MPILOT	0.45**
HULLPILOT	0.69****
Control variables	
Work history	
WHUNEMP	-0.60****
LASTJSOC8	-0.51**
LNGTHUNEM	-0.005**
Job search	
HRSSPENT	0.09****
LOOKFT	-0.39*
PARTICJOB	-0.42**
Personal	
FEMALE	-0.45**
ACADQUALS	0.36**
HEALTHPRB	-0.53***
MINORITY	0.96*
Attitudes	
NOCHANCE	-0.47***
Log-Likelihood	*-503*
Degrees of freedom	*1137*

*Significance levels: * 10%;   ** 5%;   *** 1%;   **** 0.1%.*

hours per week that people spent looking for a job (HRSSPENT), the more likely they were to have made an application, while those who were looking for a full-time job (LOOKFT) or for a particular job (PARTICJOB) were less likely to have made an application, presumably because fewer vacancies had arisen that were acceptable to them. As in Model 6, women (FEMALE) displayed a below average level of job search activity, as also did people with a long-term health problem or disability. In contrast, people who had academic qualifications (ACADQUALS - meaning here CSE or GCSE grades D-F and above) were more likely to have made an application than those who had none. There was also a marginally significant association between being a member of a minority ethnic group (MINORITY) and an increased likelihood of making a job application. Finally amongst the control variables, people who thought that unemployed people's chances of getting a job were 'very bad' (NOCHANCE) were less likely to make an application than others.

Number of job search methods used

Our last job search model examined the impact of Project Work on the number of job search methods used. Respondents were shown a list of methods and were asked to say which ones they had used every week during the job search period; we then calculated

Table 4.3
**Main survey OLS regression model for number of job
search methods used**
(Model 8)

	coefficient
Constant	3.64
Programme variables	
(relative to pooled comparison sample)	
M/MPILOT	0.49****
HULLPILOT	-0.02
Control variables	
Work history	
WHSTEADY	0.42****
LASTJMANF	-0.25*
LASTJCONS	0.28*
Job search	
RANGEJOBS	0.27**
RESNWAGE	0.09**
MAXHOURS	0.01**
LIMITED	0.31***
Personal	
CSE	0.41**
OLEVEL	0.50****
POSTOLEV	0.94****
DRVNGLIC	0.22*
Adjusted R^2	0.95
Degrees of freedom	1119

Significance levels: * 10%; ** 5%; *** 1%; **** 0.1%.

the total number of methods used.[20] This variable had a mean of five and was distributed quite normally, so an OLS regression model was appropriate. This is shown as Model 8 in Table 4.3.

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[20] This list was: looking at adverts in the local paper; looking at adverts in national newspaper/magazine; looking at adverts in trade/professional journal; looking at adverts in shop window/noticeboard on street; going to a private recruitment agency; going to Jobcentre to look at vacancies on display; going to

The results of this model reflected the results for Model 6 (Table 4.1): compared to the pooled comparison sample, significantly more job search methods were used by people from the Medway/Maidstone pilot offices but not by people from the Hull pilot offices. As we speculated earlier, this may be because people in Medway/Maidstone, the closest to London of all the areas included in the study, had access to a much wider and more diverse labour market than other sample members.

Once again, there was a strong association between having a better employment record and the intensity of job search effort, in that respondents who had in the past mainly worked in steady jobs (WHSTEADY) tended to use more job search methods. There were also marginally significant associations with the industry of the last job. Manufacturing industry (LASTJMANF) was associated with the use of fewer methods and the construction industry (LASTJCONS) associated with the use of more methods, a pattern which perhaps reflected the very different structures of these industries. Most other results appeared plausible. People who were looking for a range of jobs (RANGEJOBS) used more methods than others, while the higher the reservation wage (RESNWAGE) and the more hours per week that people were willing to work (MAXHOURS), the greater the number of methods used. People who said that their job search had been limited because of the costs involved (LIMITED) also tended to use more job search methods, which is understandable if people chose to use cheaper search methods first, so that only those who were already using many methods considered using methods that were expensive.[21] People tended to use more job search methods if they had academic qualifications, and the higher their qualifications, the more methods they tended to use (CSE, OLEVEL and POSTOLEVEL). Finally, people tended to use more job search methods if they possessed a driving licence.

Jobcentre to ask about vacancies from staff; going to Jobclub; trying to find self-employed work; asking friend or relative; directly contacting employer(s) (telephone, letter, visit).

[21] We are grateful to Nicky Tarry for this suggestion.

5 Analysis of the leaver survey

Survey design and response

As with the main survey, the sample for the leaver survey was drawn from people in the Project Work pilot and comparison offices who had been claimant unemployed for two years or more at April 1996, or who reached this point during the course of the study. It differed from the main survey in that the sample consisted solely of people who had since left the unemployment register. The leaver survey sample excluded people who formed part of the sample for the main survey, that is, people who became eligible for Project Work in the weeks commencing May 20 1996 to July 8 1996 and their equivalents in the comparison offices.

The survey was mainly intended to give information on the destinations of people who left the unemployment register. At the time the survey was designed, this information was not available from administrative records. Later it became possible to extract partial information on destinations from JUVOS for everybody who was registered unemployed, and so the leaver survey became in some respects redundant.

The original intention was to conduct the leaver survey interviews between five and seven weeks after respondents left the register. In fact, a variety of problems with the administrative systems the ES used to issue the sample to the fieldwork agency, detailed in Stratford (1998), meant that the interviews often took place much later than this. These problems were also responsible for the waste of many interviews where the respondent had been incorrectly sampled.

Full details of the survey design and response rate are given in Stratford (1998). The planned design involved a short telephone interview, and it was hoped that the cheapness of this method would allow coverage of all eligible people in the pilot and comparison offices who left the unemployment register during the study period. In the event, the nature of the survey population made telephone interviews impossible in many cases, and the majority of interviews were carried out face-to-face, thereby greatly increasing costs.

As a result of all these difficulties, the DfEE and ES decided to halt the survey prematurely, before the names and addresses of all sample members had been issued to interviewers. Stratford (1998) notes that pilot offices seemed to provide the names and addresses of sample members more quickly and more regularly than the comparison offices, and so it is possible that the curtailed sample under-represented the comparison offices. Of the 1303 interviews achieved, only a quarter were from comparison offices. In total, 1262 addresses remained unissued when fieldwork was halted, of which 916 were in from the pilot offices and 346 were from the comparison offices.

Table 5.1 summarises response to the leaver survey. As with the main survey, response rates were very poor, but higher in Hull and its comparison areas than in Medway/Maidstone and its comparison areas. Once again, the main problem was not

Table 5.1
Leaver survey response rates

	Pilot		Comparison	
	N	%	N	%
A: MEDWAY/MAIDSTONE				
Total addresses issued by ES in scope	814	100	340	100.0
Unproductive addresses	411	50	166	49
Of which:				
No contact	*155*	*19*	*37*	*11*
Refusals	*60*	*7*	*21*	*6*
Moved away	*126*	*15*	*93*	*27*
Other unproductive	*70*	*9*	*15*	*4*
Productive addresses	403	50	174	51
B: HULL				
Total addresses issued by ES in scope	961	100	252	100.0
Unproductive addresses	387	40	100	40
Of which:				
No contact	*95*	*10*	*32*	*13*
Refusals	*56*	*6*	*11*	*4*
Moved away	*227*	*24*	*47*	*19*
Other unproductive	*9*	*1*	*10*	*4*
Productive addresses	574	60	152	60

Source: Summarised from Stratford 1998, Table 2.2.

the number of refusals, but rather the fact that large numbers of people (around 500) had apparently moved house without leaving an address where they could be traced. The causes and implications of such a high rate of mobility were discussed in Chapter 2. These problems were exacerbated by the fact that the leaver survey questionnaire was very short, and so we had very little data to use as control variables. Thus it is difficult to establish firm conclusions from the leaver survey, and any results based on the analyses in this chapter must be treated with great caution.

Being in work at interview

In order to be included in the sample for the leaver survey, respondents had to have left the unemployment register. However by no means all had left to take a job: some had moved onto other social security benefits and some had gone off benefit without starting

work. Of those who had taken a job, some were unemployed again by the time they were interviewed. In all, just under half of the leaver survey sample were in work at the time of interview. A logistic regression model was developed to test whether those from the Project Work pilot offices had better chances of being in work at this point, and the results are shown as Model 9 in Table 5.2. As with Models 1-3

Table 5.2
Leaver survey logistic regression models for economic activity status (Models 9 and 10)

	Model 9 In work at interview *coefficient*	Model 10 Off unemployment register at interview *coefficient*
SPECIFICATION OF BASE CATEGORY:		
A: Hull comparison offices		
Constant	-0.42	0.09
Programme variables		
M/MPILOT	-0.38*	-0.45*
HULLPILOT	-0.73****	-0.36
M/MCOMP	-0.13	-0.26
B: Medway/Maidstone comparison offices		
Constant	-0.56	-0.17
Programme variables		
M/MPILOT	-0.24	-0.19
HULLPILOT	-0.60***	-0.10
HULLCOMP	0.13	0.26
C: Control variables Versions A & B:		
MARRIED	1.24****	1.64****
LIVESPARS	1.18****	1.26****
OWNEROCC	0.59****	1.09****
FEMALE	-	0.42**
Log-Likelihood	*-824*	*-723*
Degrees of freedom	*1296*	*1295*

Significance levels: * 10%; ** 5%; *** 1%; **** 0.1%.

based on the main survey, we present the results in two different ways: Part A of the table shows the programme effects relative to the Hull comparison offices, while Part B shows the programme effects relative to the Medway/Maidstone comparison offices. The definitions of all the variables used in the this and the other models reported in this chapter are listed in Appendix 1.

At first sight, the results of Model 9 contradict the results of the parallel model based on the main survey (see Model 2 in Table 2.2). Register leavers from the Hull pilot

offices were significantly less likely to be in a job at interview than register leavers from the Hull comparison offices. The coefficient comparing the Medway/Maidstone pilot offices with their comparison offices was also negative, though non-significant. In contrast, the main survey Model 2 indicated that respondents from the Hull pilot offices were significantly more likely to be in a job at interview than respondents from the Hull comparison offices, while the effect for the Medway/Maidstone pilot offices relative to their comparison offices was also positive, though non-significant. However the main survey and leaver survey results are not directly comparable, for the latter were restricted to people who left the unemployment register during the study period, and so can say nothing about the overall chances of getting a job. Model 3 in Table 2.2 already suggests that people from the Hull pilot offices were more likely to leave the unemployment register than people from the Hull comparison offices. What Model 9 appears to show is that *if* people from the Hull pilot offices left the unemployment register, then they were less likely to do so for a job than people from the Hull comparison offices. This is compatible with both Model 2 and Model 3.

Of course there is a very real possibility that the Model 9 results were generated simply by differential response bias in the leaver survey. There are however two other fairly plausible explanations. The first is that people from the pilot offices who left the register were less likely to do so for a job than people from the comparison offices who left the register. This could arise if client advisers encouraged people enrolled on Project Work to switch to other types of social security benefits, or if the prospect of compulsory work experience caused people to sign off regardless of whether they could claim any alternative benefits. The second possible explanation is that people from the pilot offices who left the register to take up jobs held those jobs for a shorter period on average than people from the comparison offices, so that they were out of work again by the time of their interview for the leaver survey. This could arise if they were encouraged to take up temporary or casual work, or if the terms and conditions of the jobs they took were less satisfactory.

As mentioned earlier, the number of control variables available in the leaver survey is very limited. Those that were found to be significant in Model 9 had effects in line with expectations. Register leavers who were married or living with a partner (MARRIED) were more likely to be in work at the time of interview than unmarried respondents, and those who were owner-occupiers (OWNEROCC) were more likely to be in work than those with other types of housing tenure. In addition, register leavers who lived with their parents (LIVEPARS) were also more likely to be in work than other register leavers. This was possibly because they were less likely to be eligible for social security benefits other than unemployment related benefits, and possibly also because their low housing costs made it possible for them to accept jobs with low wages. Gender was non-significant in Model 9.

Being off the unemployment register at interview

While all members of the leaver sample had to have left the unemployment register in order to be included in the sample, about a third were signing on again by the time they were interviewed. Model 10 in Table 5.2 shows our logistic regression model for being signed off at interview.

In this model, neither the comparison between the Hull pilot offices and their comparison offices nor that between the Medway/Maidstone pilot offices and their comparison offices was statistically significant. This suggests that, having left the unemployment register, people from the pilot offices were not more at risk of becoming unemployed again than people from the comparison offices. This is despite the fact that Model 9 indicated that register leavers from the Hull pilot offices were less likely to be in jobs than register leavers from the Hull comparison offices. One possible explanation for this pattern of findings is that register leavers from the Hull pilot offices were more likely than people from their comparison offices to have moved onto some other form of benefit or to have gone off benefit without getting work.

The control variables in Model 10 had effects similar to their effects in Model 9. Married register leavers were much more likely to be still signed off at the time of interview than single respondents, and leavers who lived with their parents or who were owner-occupiers were more likely to be still signed off than leavers in other types of housing tenure. In addition, in this model, female leavers (FEMALE) were more likely than male leavers to be still signed off. The fact that they were no more likely than men to be in work (Model 9) suggests that this was because women leavers were more likely than men either to have moved onto other benefits or to have signed off benefit without getting a job.

Wages analysis

Respondents to the leaver survey who had worked since their qualifying spell of unemployment and who gave valid wage data were included in an OLS regression model for net hourly pay (Model 11 in Table 5.3). The very narrow range of control variables available meant this model had limited explanatory power, with an adjusted R^2 of 0.11.

Table 5.3
Leaver survey OLS regression model for net hourly pay in first job after the qualifying spell of unemployment (Model 11)

	coefficient
SPECIFICATION OF BASE CATEGORY:	
A: Hull comparison offices	
Constant	1.15
Programme variables	
M/MPILOT	0.10**
HULLPILOT	-0.05
M/MCOMP	-0.004
B: Medway/Maidstone comparison offices	
Constant	1.14
Programme variables	
M/MPILOT	0.11**
HULLPILOT	-0.05
HULLCOMP	0.004
C: Control variables Versions A & B:	
SELFEMP	0.19****
SOC9	-0.06*
NONPERM	0.11****
FEMALE	-0.08*
LAMBDA	-0.04
Adjusted R^2	*0.11*
Degrees of freedom	*512*

Significance levels: * 10%; ** 5%; *** 1%; **** 0.1%.

There was some evidence of a positive association with pay for people from the Medway/Maidstone pilot offices, who earned on average 12 per cent more than people from the Medway/Maidstone comparison offices, after controlling for other factors. This is similar to the finding from the main survey wages model (Model 5 in Table 3.2), but, just as with the main survey, it is impossible to say from the data we have whether the difference was an effect of Project Work or a regional effect. As in the main survey model, the pay of people from the Hull pilot offices was not significantly different from the pay of people from the Hull comparison offices.

THE EFFECTS OF SOME OF THE CONTROL VARIABLES IN MODEL 11 CONTRADICTED THE RESULTS OF THE MAIN SURVEY WAGES MODEL. CONTRARY TO EXPECTATION, THE SELF-EMPLOYED (SELFEMP) RECEIVED A PAY PREMIUM, WHEREAS THE FINDINGS FROM THE MAIN SURVEY SHOWED A PAY PENALTY. IN ADDITION, WOMEN (AS EXPECTED) EARNED LESS THAN MEN, WHEREAS THE MAIN SURVEY MODEL SHOWED NO SIGNIFICANT DIFFERENCE BETWEEN THE SEXES. THE REMAINING TWO SIGNIFICANT CONTROL VARIABLES INDICATED THAT PEOPLE IN FIXED-TERM AND TEMPORARY JOBS (NONPERM) RECEIVED A PAY PREMIUM, WHILE UNSKILLED MANUAL WORKERS (SOC9) SUFFERED A PAY PENALTY. A LAMBDA VARIABLE, REPRESENTING THE EFFECT OF UNMEASURED VARIABLES AND COMPUTED FROM A PROBIT MODEL FOR ENTRY TO WORK, WAS INSIGNIFICANT.

6 Descriptive analysis of administrative data

Sources of administrative data

So far we have concentrated on the survey data that were collected for people eligible for Project Work in the pilot offices in Hull and Medway/Maidstone. We turn now to data assembled from administrative records. The present chapter reports a simple descriptive analysis of these administrative data, comparing labour market outcomes for people from the pilot offices who were enrolled on Project Work and claimants from the comparison offices who had also been unemployed for two or more years.[22] In Chapter 7 we use multivariate techniques to take the analysis further.

The administrative data came from two sources. Both for people from the Project Work pilot offices and for people from the comparison offices, the ES supplied data from the Joint Unemployment and Vacancies Operating System (JUVOS) on-flow and off-flow files. These record the start and end dates of all spells of claimant unemployment, plus first destination on leaving the unemployment register. For people from the pilot offices only, the ES matched these with data from the JUVOS training file, giving the dates that participants received the Project Work training allowance and so identifying the period or periods, if any, that they spent on the work experience element of Project Work.

It is important to note that administrative data for people from the pilot offices were only supplied for unemployed people who were actually enrolled on Project Work. As we saw in Chapter 1, it is possible that the pattern of entry to the programme varied between groups of eligible clients with different durations of unemployment, with clients who had been unemployed for a very long time over-represented amongst enrolments towards the end of the pilot period, and under-represented amongst early entrants. We return to this point later.

Setting up these two sources of administrative data in a form suitable for analysis was no trivial task.[23] Appendix 3 sets out the main steps involved and describes the assumptions that we made in order to obtain an internally consistent series of dates. Overall, three per cent of the original 11,829 people for whom we were given data were excluded from the analysis because their work histories were impossible to disentangle or contained serious missing information. These problems were more common in the data for the pilot offices, as here we needed to reconcile spells of claimant unemployment with spells claiming the Project Work training allowance; for people from the comparison offices only the former were relevant. In total,

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[22] For more information on the structure of these two samples, see Chapter 1.

[23] We are indebted to Martin Range of the Social Studies Faculty, Oxford University for writing the very complex computer programs needed to fulfil this task.

4.3 per cent of the pilot offices sample and 2.2 per cent of the comparison offices sample were excluded.

Having cleaned the data, we constructed individual work histories which identified people's labour market status in each week from the start of the qualifying spell of unemployment to the end of the study period. Here again an important proviso must be noted. For those weeks in which sample members were claiming neither unemployment benefit nor training benefit, the only information we had on labour market status came from the JUVOS record of the client's first destination on leaving the unemployment register. We had no further information on their activities until they started a fresh claim for unemployment benefit or began another spell of work experience on Project Work. Thus estimates of the time spent in each labour market status were based on the assumption that people who left claimant unemployment stayed in their initial destination until this point (or until the end of the study period, if they did not make a fresh claim or start a new spell of work experience).

Throughout the analysis, we class together time spent claiming unemployment benefit and time spent on the work experience element of Project Work ('Part B'). The reason for this is that people enrolled on Project Work who were still claimant unemployed at the end of the 13 weeks of optional extra help with job search known as 'Part A' had no option but to start work experience if they wished to continue to receive a benefit income. This would then be provided in the form of the Project Work training allowance, to which they were automatically transferred at this point.[24] If they left their placement before they had completed 13 weeks of work experience and tried to sign on again as unemployed, they were sent back onto work experience, and indeed some people went through this process several times before finally clocking up the required 13 weeks. Thus any fair comparison between the pilot and comparison samples must treat time spent on work experience as equivalent to time unemployed.

Table 6.1
Classification of labour market status used to construct the week-by-week histories

Labour market status	*Definition*
Unemployed or on Project Work work experience	Claimant unemployed as identified by JUVOS claim start and end dates, or on Part B of Project Work as identified in training benefit file
Paid work	Signed off for a job (JUVOS exit codes B, N, P)
Education/training	Signed off for education or training (JUVOS exit codes E, I, M, U)

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[24] See Chapter 1 for further details.

Other benefits	Signed off to claim another benefit (JUVOS codes D, L, Q, R)
No information	Signed off but no information on why (JUVOS codes A, F, H, S, W, Y), plus any period between leaving PW work experience and signing on again.
Censored (excluded from the analysis)	Signed off and has left the labour market through retirement, death, imprisonment etc. (JUVOS codes G, J, C, T, V, K, O)

Our classification of labour market status is shown in Table 6.1. The JUVOS unemployment exit codes on which it is based are explained in Appendix 3. The categories are mutually exclusive as only one exit code is recorded in JUVOS for each unemployment spell. If sample members had more than one status in a single week, they were given the code for the status that they held for the greatest number of days. Note that the administrative records for receipt of the Project Work training allowance contained no data on first destination after ceasing to claim the allowance, so it was only if they straight away signed on again as unemployed that we knew people's labour market status in the period immediately after their training allowance had ceased. A small number of sample members had been given JUVOS exit codes that indicated that they were no longer competing in the labour market (for example, they had retired, died or been sent to prison). We used data for these people only up until the time that a censoring event of this type occurred, even though in one or two cases there were subsequent entries in the JUVOS record .

Period covered by the analysis

The period covered by our analysis of the administrative data started with the invitation to the Restart interview that triggered entry to Project Work (in the pilot offices) or to the comparison sample (in the comparison offices). This was the interview that became due on reaching a claim duration of two years or, for claimants who had already been unemployed for more than two years, the next Restart interview in the regular six-monthly cycle. As we saw in Chapter 1, in some cases and especially if clients had been unemployed for a very long time, the triggering Restart interview for the pilot sample was not the first one after the start of the pilots, but the one after that. The earliest date that the invitation to the triggering Restart interview was issued was 1st March 1996. Some clients did not receive their invitation until 1997, but we confined the analysis to people who had received their invitation by the end of December 1996. This ensured that we had a reasonably lengthy follow-up period in which to examine their subsequent progress.

The administrative data allowed us to track individuals through to the middle of June 1997. Thus for individuals invited to attend interviews at the beginning of March 1996, we had information on labour market status for a

maximum of 68 weeks. However, as Chapter 1 described, in February 1997 the Project Work pilots were extended into four of the comparison offices for Medway/Maidstone, making these offices useless for comparison purposes after January 1997. This meant that when analysing outcomes through to June 1997 we had to exclude both the comparison offices in which the extended Project Work pilots had been introduced and the pilot offices with which they had been matched.[25] We refer to the remaining pilot and comparison offices as the 'non-contaminated' sample. Thus the full set of pilot and comparison offices was analysed only up to the end of January 1997.

The non-contaminated sample was dominated by Hull and its comparison offices. While Project Work participants from the Hull pilot offices formed 58 per cent of the full pilot sample, they formed 83 per cent of the non-contaminated pilot sample. Similarly, while people from the Hull comparison offices formed 51 per cent of the full comparison sample, they formed 82 per cent of the non-contaminated comparison sample.

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[25] See Chapter 1 for more details.

Proportion of time in various labour market states

Table 6.2 shows the mean percentage of time that Project Work and comparison samples spent in each of the labour market states defined in Table 6.1, taking the basic unit of time as one week. The figures were obtained by calculating the relevant percentages separately for each individual, and then taking the mean percentage for the group. This approach gave equal weight to all sample members, regardless of the date of their triggering Restart invitation and thus regardless of the number of weeks of data that they contributed to the analysis. It means that the figures in Table 6.2 are not biased in favour of people who entered Project Work early.

Table 6.2
Mean proportion of time from the triggering Restart invitation that was spent in various labour market states

	Hull pilot offices	Compar-ison offices for Hull	M/M pilot offices	Compar-ison offices for M/M	All pilot offices	All compar-ison offices
	%	%	%	%	%	%
A: to end January 1997 *(full sample)*						
Unemployed or on PW work experience	77	87	72	84	75	86
Paid work	9	6	11	6	10	6
Education/training	5	1	5	1	5	1
Claiming other benefits	5	2	5	3	5	3
No information	4	3	7	6	5	4
Total	**100**	**100**	**100**	**100**	**100**	**100**
Base N	*3,689*	*2,616*	*2,612*	*2,519*	*6,301*	*5,135*
B: to mid June 1997 *(non-contaminated sample)*						
Unemployed or on PW work experience	67	82	68	74	67	80
Paid work	12	9	11	9	12	9
Education/training	7	2	6	2	7	2
Claiming other benefits	7	3	6	6	7	4
No information	6	4	10	9	7	5
Total	**100**	**100**	**100**	**100**	**100**	**100**

	Base N	3,690	2,616	742	564	4,432	3,180

Part A of the table is based on the full set of pilot and comparison offices and takes the analysis up to the end of January 1997. For those invited to Restart at the beginning of March 1996, percentages were calculated on the basis of activities over the full 49 weeks, but the mean number of weeks on which the percentages were based was 29 for the pilot sample and 28 for the comparison sample. The last two columns of the table show that Project Work participants from the pilot offices spent 11 percentage points less time in claimant unemployment (including work experience on Project Work) than the comparison sample. Over the average 28 week time-frame this translated into just over three fewer weeks unemployed. As the table shows, Project Work participants in Medway/Maidstone were at a slightly greater advantage relative to people from their comparison offices than participants in Hull.

However, the difference between the pilot and comparison offices in the time spent in paid work was much smaller than the difference in time spent unemployed. Between all the pilot offices and all the comparison offices there was a gap in this respect of 4 percentage points, which equated to one extra week in employment on average in the pilot offices out of the mean of 28. Once again, Project Work participants in Medway/Maidstone were at a slightly greater advantage relative to people from their comparison offices than participants in Hull. In both groups of offices, Project Work participants also spent proportionately more time in education or training and more time claiming other benefits than their counterparts in the comparison offices, and they also had a greater proportion of weeks in which we had no information on activity status.

Part B of Table 6.2 exploits the full work history available by taking the analysis up to the middle of June 1997. For those invited to their triggering Restart interview at the beginning of March 1996, percentages were calculated on the basis of activities over the full 68 weeks between these two dates, but the mean number of weeks on which the percentages were based was 47. Sample sizes were smaller than in Part A of the table because the analysis was based only on the non-contaminated offices.

Over this extended time-frame the gap between all the pilot offices and all the comparison offices in the mean percentage of time spent in unemployment (including work experience on Project Work) widened to 13 percentage points, or just over six weeks out of the mean of 47. However three points should be noted. First, the unemployment gap between the Medway/Maidstone offices and their comparison offices (6 percentage points) was far narrower than that between the Hull pilot offices and their comparison offices (15 percentage points). We come back to this issue later. Second, the overall gap between all the pilot offices and all the comparison offices widened only because the non-contaminated pilot and comparison samples were dominated by Hull. If Hull and Medway/Maidstone were represented in the non-contaminated sample in the same proportions as in the full sample,

the gap between the pilot and comparison offices over the period to June 1997 would have remained the same as the gap over the period to January 1997, namely 11 percentage points.[26] Third, the gap between the Hull pilot offices and their comparison offices in the proportion of time spent unemployed was as great as it was, not because Project Work participants in Hull were more successful in finding work than participants in Medway/Maidstone, but because people in the comparison offices for Hull spent a particularly high proportion of time unemployed. It is very likely that this reflected conditions in the local labour markets in which the Hull comparison offices were situated. As Chapter 1 showed, at the start of the Project Work programme three of Hull's six comparison offices had unemployment rates in double figures.

The gap between the pilot and comparison offices in the mean proportion of time through to June 1997 that was spent in paid work was similar in the Hull and Medway/Maidstone groups of offices, and not unlike the corresponding figures for the period through to January. The same was true of the mean proportion of time spent in education and training. However the mean proportion of time spent claiming other benefits gave a different picture: this proportion was 4 percentage points greater in the Hull pilot offices than in their comparison offices, but was identical in the Medway/Maidstone pilot and comparison offices. This suggests that there may have been differences between Hull and Medway/Maidstone in the way that Project Work was implemented.

Gender, age and length of unemployment

Gender made very little difference to the size of the gap between all the pilot and comparison offices in the mean proportions of time spent in various labour market states. Comparing Part A of Table 6.3 with Part B, we see that this was true both for
the period through to January 1997 and for the extended period through to mid-June. The only difference between the sexes was that women tended to spend a little less time than men unemployed and a little more time claiming other benefits.

Table 6.3
Mean proportion of time from the triggering Restart invitation that was spent in various labour market states, by gender

	Men		*Women*	
	All pilot offices %	All comparison offices %	All pilot offices %	All comparison offices %

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[26] This calculation was based on the assumption that the percentage of time spent in unemployment in the Medway/Maidstone pilot offices dropped from the non-contaminated sample was the same as in the offices that were retained, and similarly for the comparison offices for Medway/Maidstone.

A: to end January 1997				
(full sample)				
Unemployed or on PW work experience	75	86	73	83
Paid work	10	6	10	7
Education/training	5	1	5	1
Claiming other benefits	5	2	7	4
No information	6	4	5	5
	100	**100**	**100**	**100**
Base N	*5299*	*4281*	*887*	*718*
B: to mid June 1997				
(non-contaminated sample)				
Unemployed or on PW work experience	68	81	65	77
Paid work	12	9	12	9
Education/training	7	2	7	1
Claiming other benefits	7	4	10	6
No information	7	5	6	7
Total	**100**	**100**	**100**	**100**
Base N	*3730*	*2690*	*613*	*422*

Table 6.4 breaks down the proportion of time in each labour market state by age group. As expected, the mean proportion of time in work fell with increasing age and the mean proportion of time unemployed rose a little. In each age group, Project

Table 6.4
Mean proportion of time from the triggering Restart invitation that was spent in various labour market states, by age

	Under 25		*25-34*		*35-44*		*45+*	
	All pilot offices	All comparison offices	All pilot offices	All comparison offices	All pilot offices	All comparison offices	All pilot offices	All comparison offices
	%	%	%	%	%	%	%	%
A: to end January 1997								
(full sample)								
Unemployed/ PW	72	83	74	85	75	87	78	87

work experience							
Paid work	12	8	11	7	9	5	8	6
Education/training	6	2	5	1	5	1	4	1
Other benefits	3	2	5	2	6	3	7	3
No information	7	5	6	4	6	4	4	4
Total	**100**	**100**	**100**	**100**	**100**	**100**	**100**	**100**
Base N	*972*	*744*	*2415*	*2027*	*1816*	*1401*	*983*	*827*
B: to mid June 1997 *(non-contaminated sample)*							
Unemployed/ PW work experience	65	79	68	81	66	80	70	81
Paid work	14	10	12	9	12	8	9	8
Education/training	8	3	7	2	6	1	6	2
Other benefits	5	3	6	4	9	5	10	4
No information	8	5	7	5	7	5	5	4
Total	**100**	**100**	**100**	**100**	**100**	**100**	**100**	**100**
Base N	*723*	*502*	*1751*	*1287*	*1229*	*850*	*640*	*473*

Work participants spent on average less time unemployed and more time in work than people from the comparison offices, though in both respects the gap between the pilot and comparison offices was narrower for the over-45s than for younger people. These results held true whether the analysis was curtailed at January 1997 (Part A of the table) or taken through to June (Part B).

The pilot offices were also associated with extra time spent claiming other benefits, particularly among older age groups. Among those aged 45 and over, roughly half of the gap between the pilot and comparison offices in time spent in unemployment was accounted for by time spent on other benefits. Once again, these findings held true for both the full and non-contaminated samples.

Table 6.5 looks at the length of the 'qualifying spell' of unemployment, namely the spell that in the pilot offices gave eligibility for Project Work and in the comparison offices triggered entry to the comparison sample. This had a

complex association with the proportion of time spent in different labour market states. It will be recalled from Chapter 1 that the qualifying spell had to have lasted at least two years. In the comparison sample the mean proportion of time spent unemployed increased, as we would expect, with the length of the qualifying spell, while the mean proportion of

Table 6.5
Mean proportion of time from the triggering Restart invitation that was spent in various labour market states, by length of qualifying spell of unemployment

	Under 2.5 years		2.5-2.9 years		3.0-4.9 years		5 years or more	
	All pilot offices	All comparison offices	All pilot offices	All comparison offices	All pilot offices	All comparison offices	All pilot offices	All comparison offices
	%	%	%	%	%	%	%	%
A: to end January 1997 *(full sample)*								
Unemployed/ PW work experience	76	82	71	86	72	87	77	90
Paid work	11	8	13	7	11	6	7	4
Education/training	4	2	6	1	6	1	4	1
Other benefits	3	3	4	2	5	2	7	2
No information	5	6	7	4	6	4	4	3
Total	**100**	**100**	**100**	**100**	**100**	**100**	**100**	**100**
Base N	*1732*	*1474*	*745*	*735*	*1954*	*1790*	*1871*	*1136*
B: to mid June 1997 *(non-contaminated sample)*								
Unemployed/ PW work experience	68	74	63	79	65	83	70	86
Paid work	14	12	16	10	12	8	9	5
Education/training	6	3	7	2	8	2	6	1
Other benefits	5	4	5	4	7	3	10	4

	No information	7	7	8	6	8	4	5	4
Total		**100**	**100**	**100**	**100**	**100**	**100**	**100**	**100**
Base N		*1155*	*917*	*490*	*439*	*1335*	*1081*	*1452*	*743*

time in paid work fell. In the pilot offices, however, these relationships were U-shaped: people with a short qualifying spell (under 2.5 years) spent on average more time unemployed and less time in work than people with slightly longer qualifying spells. This pattern can be seen in both Part A and Part B of Table 6.5.

Because of this difference between the pilot and comparison offices, the gap between the pilot and comparison offices in the mean proportion of time spent unemployed was much narrower for people whose qualifying spell was less than 2.5 years than for people whose qualifying spell was longer than this. While in the former case the gap was six percentage points, with longer qualifying spells the gap ranged from 13 to 18 percentage points.

For people with qualifying spells of under 2.5 years, the pilot/comparison office gap in the mean proportion of time unemployed was explained mainly by people in the pilot offices spending more time in work or in education and training than people in the comparison offices. For people with longer qualifying spells, extra time spent claiming other benefits became an important factor. This was especially so for people with very long qualifying spells of unemployment: in the period up to January 1997, Project Work participants with qualifying spells of five years or more spent on average 5 percentage points more time claiming other benefits than people from the comparison sample with unemployment spells of similar length.

Employment and unemployment rates over time

We have established that, overall, Project Work participants spent on average less time in unemployment and more time in work and claiming other benefits than their counterparts in the comparison offices, though the size of the gap varied with age and the length of the qualifying spell of unemployment. We now examine whether the gap between members of the pilot and comparison samples changed with increasing time from the triggering Restart invitation. To do this, we identified each sample member's labour market status at five-weekly intervals after his or her triggering Restart invitation (so the calendar dates to which these time points refer vary with the date of the Restart invitation). For this analysis we used only the non-contaminated sample that excluded the four comparison offices where the extended Project Work pilots were launched in February 1997 and their four matched pilot offices.

Graph 6.1, which shows data for Hull and its comparison offices, plots the unemployment and employment rates over a period of 50 weeks at five-

weekly intervals from the triggering Restart invitation.[27] These rates were calculated simply as the number in the relevant category expressed as a percentage of the number in the sample for whom we had information at that time point. As throughout this chapter, we combined time spent on the work experience element of Project Work with time spent claimant unemployed to calculate the unemployment rate. Note that in Graph 6.1, the estimate for each time point is based on all people who had data for that time point. As this number diminished as the number of weeks since the triggering Restart invitation grew, estimates for later time points are more subject to sampling error than estimates for earlier points.

By definition, all members of both the pilot and comparison samples were unemployed at week 0, the date of the Restart invitation. Thereafter, the graph shows that the unemployment rate fell more rapidly for Project Work participants in Hull than for its comparison sample. The gap between the two widened particularly quickly after fifteen weeks, which was when most participants who had not already left the register had completed Part A of the programme (voluntary help with job search) and were facing compulsory work experience under Part B. This widening of the gap continued up to week 30, after which the gap gradually started to narrow again, for while the unemployment rate for the pilot sample remained roughly constant after week 30, the unemployment rate for the comparison sample continued to fall. By week 35, when most Project Work participants had completed the programme, one in two were unemployed, compared to three in four comparison sample members, leaving a gap of 25 percentage points. By week 50, this gap had closed to 18 percentage points.

The bottom two lines in Graph 6.1 show that lower unemployment rates over time for participants in Hull relative to the comparison sample were partly accounted for by their higher employment rates. Employment rates diverged markedly after fifteen

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[27] The actual percentages plotted in Graph 6.1 and in Graphs 6.2-6.4 that follow are reported in Table A4.1 in Appendix 4.

GRAPHS 6.1 ON THIS PAGE

GRAPH 6.2 ON THIS PAGE

weeks, producing a gap of 7 percentage points at week 25. Subsequently this gap narrowed to a fairly constant 3 or 4 percentage points from week 40 onwards. Even at week 25, the gap in employment rates was small relative to the gap in unemployment rates, indicating that increased enrolments on training courses and transfers to other benefits played a more important role in reducing the unemployment rate in the Hull pilot offices than did job starts.

Graph 6.2 is similar to Graph 6.1 except that it is based only on people for whom we had data for a full 50 weeks after the triggering Restart invitation. Thus sample numbers are constant at each time point, and later time points are no more vulnerable to sampling error than early time points. It was only possible to have 50 weeks of data for a members of the pilot sample if their invitation to attend the triggering Restart interview had been issued before the end of June 1996, so Graph 6.2 is based on early entrants to Project Work.

We showed in Chapter 1 that people who had been unemployed for a very long time may have been under-represented amongst early entrants, and it is well established that unemployed people's chances of leaving unemployment fall with increasing duration of unemployment (see, for example, Payne, Casey, Payne and Connolly 1996). It follows that early entrants to Project Work should have been on average more likely to leave unemployment than later entrants, so Graph 6.2 should show a greater gap between the unemployment rates in the pilot and comparison samples than Graph 6.1. Moreover, as the later the date of the triggering Restart invitation, the fewer the number of weeks data that we had, this difference should only be observable in the early weeks after the triggering Restart invitation.

This is exactly what we see when we compare Graph 6.2 with Graph 6.1. At week 20, Graph 6.2 showed a gap of 26 percentage points between the pilot and comparison sample unemployment rates, compared to a gap of 18 percentage points in Graph 6.1. By week 35, the two graphs were very similar, with gaps of 26 and 25 percentage points respectively.

Thus the picture obtained from Graph 6.1 is partly obscured by compositional differences between the samples at different time points. In a constant sample, the apparent short term impact of Project Work on the unemployment rate was greater, but the decay in this impact over time was also more rapid.

The corresponding pictures for Medway/Maidstone and its comparison offices are given by Graphs 6.3 and 6.4. Here sample numbers were smaller than for Hull because we used just the uncontaminated sample, and the plots were correspondingly more subject to sampling fluctuation. Graph 6.3, based on diminishing sample numbers over time, shows that over the first fifteen weeks after the triggering Restart invitation the unemployment rate for Medway/Maidstone Project Work participants was very close to the unemployment rate for the comparison sample. Thereafter a gap began to open up, with the unemployment rate for the pilot sample falling much more quickly until week 25. At week 35 it stood at 13 percentage points, less than half the size of the corresponding gap between the Hull pilot and comparison

offices. From then onwards, unlike as in Hull, the gap remained fairly constant. Graph 6.4, based only on people for whom we had data at week 50 and thus with constant sample numbers over time, shows more similarity with the picture for Hull. The gap

GRAPH 6.3 ON THIS PAGE

GRAPH 6.4 ON THIS PAGE

between the pilot and comparison samples emerged earlier than in Graph 6.3, was bigger at its widest point, and then narrowed.

In both graphs, trends in employment rates in Medway/Maidstone and their comparison offices mirrored trends in the Hull samples, with the early advantage of the pilot sample decaying over time. Again, much of the gap in unemployment rates was accounted for by ways of moving off unemployment benefit that did not involve getting jobs.

Perhaps the most notable feature of all four graphs is the sharp downturn in the unemployment rate for both the Hull and Medway/Maidstone pilot samples at the 15 week point, when people who had not left the register had to start compulsory work experience. This drop was even sharper in the graphs based on constant samples over time, where it was not obscured by compositional differences.

The 'benefit crackdown' in Hull

It will be recalled from Chapter 1 that between November 4th and November 29th 1996 a 'benefit crackdown' campaign was mounted in Hull as part of a concerted effort to reduce benefit fraud. To test whether this had any impact on outflows from claimant unemployment in Hull, we examined unemployment rates over the 11 week period from October 14th to December 29th.[28] This analysis was based on the constant samples of people for whom we had data for all 11 weeks.

Over this period, the gap in the unemployment rates between the Hull pilot offices and their comparison offices widened from 14 to 16 percentage points. However this change was gradual and began before the benefit crackdown,. During November itself the gap widened by only 0.4 percentage points. In the Medway/Maidstone pilot and comparison offices over the same dates the situation was similar: the gap in claimant unemployment grew gradually from 16 to 20 percentage points, widening by 0.8 of a percentage point during November. These observations suggest that the benefit crackdown had no sudden impact on unemployment amongst Project Work participants in the Hull area.

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[28] As before, the category 'unemployment' included both claimant unemployment and time spent on the work experience element of Project Work.

8 Discussion and conclusions

How good is the evidence?

We have avoided drawing any conclusions about the effectiveness of Project Work until this final chapter. This is because there are three separate sources of evidence - the main survey, the leaver survey, and the administrative data - and there are deficiencies in each. Conclusions drawn from any single source may be misleading: we need to look at the composite picture derived from all three sources, and to assess the extent to which this is obscured by the quality of the evidence.

The conclusions we draw relate to two separate issues: the effectiveness of Project Work, and the kind of evaluation research that is needed to establish clear conclusions about the impact of government programmes for unemployed people. First, however, we review the quality of the data collected.

The basic problem that impaired the value of all three sources of evidence was that the structure of the pilots made it difficult to reach clear conclusions about their effectiveness. These problems were discussed in detail in Chapter 1. One of the most serious difficulties arose from the fact that Project Work was launched in only two areas and applied to everybody in those areas who had been claimant unemployed for two or more years. This means that we cannot take proper account of the impact on outcomes of differences between the local labour markets in which the pilot and comparison offices were situated. Another serious problem is that there were a lot of other new policy initiatives being mounted in both pilot and comparison offices at the same time as Project Work was being set up, making it hard to be sure of the specific impact of Project Work as distinct from the impact of other changes. Finally, in the middle of the evaluation period, new Project Work pilots were launched in four of the comparison offices for the Medway/Maidstone pilot, making data from those comparison offices after January 1997 useless for evaluation purposes.

These difficulties were made worse by the way in which the evaluation study was implemented. No proper record was kept of the procedure for matching pilot and comparison offices, and in terms of local labour market characteristics the match that was achieved was approximate only. One reason for this was that several offices selected as good matches had to be rejected, either because they were already piloting other initiatives, or because their workload was already so high that they could not be asked to undertake the extra record-keeping involved in taking part in the study. In addition, no central record was kept of the pattern of enrolments on Project Work in relation to the number of eligible clients. Evidence from the later extended Project Work pilots suggests that the pattern of entry over the course of the first pilots may have differed between eligible clients with different durations of unemployment, and this complicates the interpretation of the analysis results.

In addition to these problems, there were specific problems with each of the three separate sources of evidence on which the evaluation was intended to rest. The 'main survey' of people from the pilot offices who were eligible for Project Work and people from the comparison offices who had also been unemployed for two or more years suffered from a very low response rate. In Medway/Maidstone and its comparison offices, interviews were achieved with scarcely more than two in five of the claimants sampled. Details can be found in Chapter 2. This poor level of response was due not to an excess of direct refusals, but largely because of a failure to make contact with a substantial proportion of the sample (despite repeated attempts), and because of a particularly high proportion of sample members who interviewers were told had moved away from the address on record at the local ES office. These difficulties were almost as marked in the comparison samples as in the pilot samples, and so cannot be attributed to the adverse publicity which had met the launch of Project Work in both pilot areas. They suggest that geographical mobility together with a degree of hostility and suspiciousness towards authority and possibly a range of social, health and personal problems associated with long-term unemployment made the interview survey method not very well suited to gathering data on this population.

Response to the 'leaver survey' of people from the pilot and comparison offices who left the unemployment register during the study period was slightly higher than response to the main survey, but was still well below the level needed for us to have confidence in the survey estimates. Chapter 5 gives details. However this survey suffered from serious problems in the way that the sample design was implemented in the local ES offices. In the end, the survey was cut short prematurely, leading to a big imbalance between the pilot and comparison offices in the number of interviews achieved, and making it virtually impossible to say what other sampling biases were present.

In addition to the two surveys, the evaluation design had originally envisaged using administrative data gathered directly in the pilot and comparison offices to chart claimants' progress. However there proved to be many problems with these data, including missing entries, internal inconsistencies, and the fact that much of the data was recorded in a way unsuitable for computer analysis.[29] Some of these problems were probably due to the fact that the data had to be transferred via manual transcription from the computer system that client advisers used when dealing with clients to be input a second time into the computer system on which the data base was built up. This manual procedure was bound to introduce many data errors. It was also very time-consuming and prone to be neglected when advisers were under pressure. This first administrative data base was eventually abandoned for the purposes of the analyses presented in this report.

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[29] Although the local offices held much information that could have been very useful in the analysis, such as the qualifications that clients possessed, the information was recorded in an unsystematic way that made it difficult to code, even if resources had been available to carry out this task.

The administrative data that we finally used were extracted from national administrative systems that drove payments to claimants, namely the JUVOS on-flow and off-flow files and the JUVOS training file. Chapter 6 gives details. These systems are audited regularly, increasing confidence in their accuracy. However there were problems even with these data. First, it was not possible to establish everybody's labour market status in every week: over the full follow-up period, there was missing information on status on average for seven per cent of the time in the pilot samples and five per cent of the time in the comparison samples. This was not a serious problem if the weeks for which we had no data were spread randomly across every labour market status, but if missing information was biased towards particular labour market states, then the validity of inferences based on the administrative data may have been undermined. Second, the JUVOS off-flow file holds information only on people's first destination after leaving unemployment related benefits. We had to assume that people stayed in this destination until they began a new claim or started to claim the Project Work training allowance. This assumption was unlikely to hold true for everyone, but we had no way of estimating the extent of any biases that it introduced. Third, for the reasons explained in Appendix 3, there was probably a bias in the data we were able to use against people with more complex histories. Fourth, there was only a limited range information available to use to control for individual variation in the pilot and comparison samples, though this would have been less of a problem if there had been a nationally representative spread of pilot and comparison offices.

Was Project Work effective?

Despite the limitations of the data, we can probably be confident that people enrolled on Project Work left the unemployment register at a faster rate than people in the comparison offices who had also been unemployed for two or more years. The most persuasive evidence for this conclusion comes from the analysis of the administrative data in Chapters 6 and 7. The finding is subject to the reservations outlined above, but the size of the register effects associated with Project Work meant that the gap between the pilot and comparison samples was unlikely to be wholly explained away by unobserved differences between them due to any biases in the data.

Perhaps the most convincing evidence is found in Graphs 6.1 - 6.4 in Chapter 6, which show a sharp drop in registered unemployment in both the Hull and the Medway/Maidstone pilot offices around the time that participants were due to move from Part A of Project Work, the optional 13 weeks of intensive help with job search, to Part B, the 13 weeks of compulsory work experience. Participants were offered very little choice over what they did during Part B, and the work tended to be low skilled and unstimulating. According to a separate evaluation of Project Work based on qualitative data, 'the transition from Period A to Period B was shown to be one of the more problematic stages in the Project Work programme. The participants who were resistant to the placements were at their most despondent or hostile, and certainly in the early stage of Project Work felt they were walking into an unknown…The

lack of choice of work placements was strongly criticised by the Project Work participants. It is bad enough, they say, to be "made" to work but to have to work at something which they neither liked nor had any interest in made it all the more unpalatable' (Ritchie and Legard 1997, page iv). In this context it is highly plausible that some people would search for any means possible to avoid Part B, and this could only be achieved if they signed off from unemployment related benefits.

The evidence of the graphs was backed up by statistical modelling of the administrative data. This showed that by the twentieth week after their entry to the programme, Project Work participants in both Hull and Medway/Maidstone showed a substantial and highly significant reduction in unemployment probabilities relative to people in the comparison offices of the same age and sex who had been claiming unemployment-related benefits for a similar length of time.

The evidence from the weaker survey sources was not as persuasive, but lent some modest support to the conclusion that Project Work reduced the claimant unemployed count. The main survey showed that by the time they were interviewed, which was at a very variable interval after they entered Project Work, Project Work participants in Hull were significantly more likely to have left the unemployment register than their counterparts from the comparison offices. However the main survey offered no evidence of a register effect of Project Work in Medway/Maidstone. The leaver survey was not relevant to this particular issue, as it was confined to people who had already left the register.

Though we can be reasonably confident that Project Work increased the number of long-term unemployed people leaving the register, it was much harder to establish from the study data whether the programme also increased the number entering paid work. The descriptive analysis of the administrative data showed that the difference between the pilot and comparison samples in the mean proportion of time spent in paid work was much smaller than the difference in the mean proportion of time spent unemployed, a finding confirmed by the time trends in Graphs 6.1 - 6.4. Fifty weeks after entering the study, 17 per cent of the Hull pilot sample were in work compared to 14 per cent of the comparison sample for Hull; for Medway/Maidstone the corresponding figures were 17 per cent and 13 per cent. Moreover, although these graphs showed that the rate of job entry in the pilot samples tended to be slightly faster over the 26 weeks that participants were enrolled in the programme than later on, the change in slope was small compared to the very marked time trends in register exits. In the statistical models based on the administrative data, the effects of participation in Project Work on the probability of being in paid work, though significant, were much smaller than the effects on the probability of being unemployed. The smallness of the apparent impact of Project Work on job entry means that there is a risk that it could be explained away by hidden biases in the data arising from the problems we have described in the quality of the evidence.

The discrepancy between the pilot and comparison offices in the proportion leaving claimant unemployment and the proportion moving into paid work was largely filled by an increased rate of transfer in the pilot offices to other social security benefits and to government supported training. Younger claimants were particularly likely to start on training programmes, and older claimants were particularly likely to transfer to other benefits. The rate of transfer to other benefits also increased markedly with the length of time that claimants had been out of work, a feature that revealed itself in the finding from the statistical modelling of the administrative data that Project Work had a bigger register effect on the long-term unemployed stock than the long-term unemployed flow.

Though these transfers succeeded in reducing the claimant count in the pilot offices and may have improved the quality of life for some people, they did not lead to savings for the public purse: the kinds of benefits that people were likely to be moved onto tend to be more generous than unemployment related benefits, and the total costs of government supported training for unemployed people are greater than the costs of unemployment related benefits alone. Of course, this training may have increased claimants' long term job chances, but people did not necessarily need the prospect of compulsory work experience to encourage them to train.

Turning back now to Project Work participants who got paid work, we have some indication of the kinds of jobs they took from the main survey. Statistical modelling reported in Chapter 3 suggested that in Medway/Maidstone, Project Work increased the odds of getting a part-time or temporary job, but had no impact on the odds of taking a permanent full-time job. This finding was supported by the statistical modelling of the administrative data, which gave indications that Project Work participants in Medway/Maidstone were more likely than members of their comparison sample to have taken short-term seasonal employment. In Hull there was no evidence of this in either the main survey or the administrative data.

The data provided only ambiguous evidence on the impact of Project Work on the wages that people obtained in subsequent jobs. This was because the restriction of the first pilots to just two localities made it impossible to distinguish the effects of the programme from the substantial regional variations that exist in wage levels. Other things being equal, people in the Medway/Maidstone pilot sample who found work earned significantly more than members of their comparison sample, but this may simply have been because Medway/Maidstone was closer to London than any of the areas with which it was compared. There was no significant difference between the wage levels of workers in the Hull pilot and comparison samples. These results were obtained in both the main survey (Chapter 3) and the leaver survey (Chapter 5).

Evidence on job search was also confused, and it was difficult to separate the impact of Project Work from the impact of the very different labour markets served by the Medway/Maidstone and Hull offices. It appeared that, after controlling for relevant factors, people from the Medway/Maidstone pilot

offices spent *more* time looking for work than people from the comparison offices, while people from the Hull pilot offices spent *less* time. While the Medway/Maidstone pilot sample used more job search methods than members the comparison sample, there was no difference for Hull. However members of both the Medway/Maidstone and Hull pilot samples were more likely to have made at least one job application than the comparison samples.

Activities after leaving the unemployment register are highly relevant to the question of how long the register impact of Project Work lasted. People who signed off to try to avoid compulsory work experience, without providing themselves with an alternative source of income, may well have signed on again later. Those who took temporary jobs also may have rejoined the claimant count after a few weeks or months. In contrast, people who transferred to other types of social security benefits probably remained on those benefits for some time. People who took permanent jobs or who perhaps legalised jobs they had been doing while claiming stood a good chance of staying off the register for some time, though they may have received in-work benefits. People who left the unemployment register to embark on government-supported training for unemployed adults under the Training for Work (TfW) programme as it was then called were likely to have returned to the register after a while, unless the training programme was itself successful in leading to work. A separate evaluation study of TfW suggests that this depended a good deal on the type of placement that people were given, with employer placements being by far the most likely to lead to a job (Payne, Payne, Lissenburgh, and Range, 1999). However this study also shows a high degree of selectivity in the allocation of employer placements under TfW, which were most likely to be given to people who had been unemployed for a comparatively short time. The Project Work client group, who had been unemployed for a minimum of two years, were not likely to be high in the queue for these desirable placements.

Let us consider, then, the direct evidence we have as to whether the register impact of Project Work decayed over time. The conclusion to be drawn about this from the administrative data depends on whether we take a sample that diminished in size over time as people for whom we had data for shorter periods dropped out, or whether we take the subsample of people which was constant in size over time because we excluded people for whom we did not have data for the whole of the follow-up period. Though there was evidence of decay in the former, decay was much more apparent in the latter, in both the graphs in Chapter 6 and in the statistical models in Chapter 7. This seemed to be because decay was masked to some extent by compositional changes in the non-constant sample. Decay was measured over a period of less than a year from the start of the Project Work process, and we cannot say whether there was any further decay in the longer term.

The leaver survey (Chapter 5) also provided some weak evidence on longer-term outcomes for people who left the unemployment register during the study period. It indicated that, when interviewed after a (very variable) interval, members of the Hull pilot sample who had left the register were less likely to be in work than leavers belonging to the comparison sample for Hull. No parallel difference was found between the Medway/Maidstone pilot and

comparison samples. However the many problems with the leaver survey suggest that evidence from this source is perhaps best disregarded.

We saw earlier that the register impact of Project Work was strongest at the point of transition between Parts A and B of the programme, and could be interpreted as the effect of avoiding action on the part of potential entrants to Part B. If people did not leave the register before Part B, there was no evidence in our data that they gained any significant benefits from their time on work experience. This accorded with the findings of the qualitative evaluation, which reported that 'the most widespread criticism [of Part B] was that participants did not "learn" anything during their work experience which would benefit their job prospects or CVs' (Ritchie and Legard 1997, page iv). It also accords with evidence from evaluations of the former Employment Training and Employment Action programmes (Payne, Lissenburgh, White and Payne 1996) and the Training for Work programme (Payne, Payne, Lissenburgh and Range 1999) that project based work experience is of relatively little value to unemployed people in terms of the impact it has on their job chances.

Implications for the planning of evaluation studies

The story of the evaluation of Project Work is not a particularly happy one. Before summarising the findings from our analysis, we pause for a moment to spell out some of the lessons it holds for the planning of future evaluation studies.

The most obvious of these lessons relate to technical aspects of research design. On the more positive side, the administrative data extracted from national sources were very valuable, particularly as the large-scale interview survey method proved to be ill suited to the study of very long-term unemployed people. Administrative data provide very large sample numbers at low cost, avoid problems of biases in recall and response, and minimise the additional burdens imposed by evaluation on local ES offices. They are particularly useful for assessing programme outcomes when their time structure is exploited. In the present study, records from two separate administrative sources were linked, and it is not hard to envisage that future studies might be able to track people's movements between unemployment, paid work and other social security benefits.

Another lesson concerns the importance of allowing for the impact of variations in local labour market conditions. The ideal evaluation design has programme participants and non-participants side-by-side in the same local labour markets, though the fact that Part B of Project Work was compulsory was thought to rule this out in this case. If we cannot have participants and non-participants in the same labour markets, then both the pilot and comparison areas should be scattered across a (preferably nationally representative) range of labour markets, so that the effects of labour market variation can be modelled.

The third lesson, put broadly, is not to assume that the way the rules say that the programme should be run is the way that it will work out at the coal face. Over-confidence on this point in the case of Project Work meant the information was not collected that would have enabled us to identify any biases in the pattern of enrolments over time.

The story of the evaluation of Project Work also holds lessons of a wholly non-technical nature, about the importance attached to evaluation when new programmes for unemployed people are conceived.

In the case of the first Project Work pilots, the needs of evaluation appeared to be secondary to other requirements. Research design possibilities were very constrained by the prior policy decision to limit the pilots to just two areas. The requirements of good design were further overridden by the decision to include four of the original comparison offices among the new wave of extended pilots. Evaluation deserves to be given more importance than this - government programmes for unemployed people are expensive, and clear evidence of their effectiveness is needed if this expense is to be justified. Where the programme involves compulsion, it is even more important to be sure that it will benefit the people who are compelled to take part.

It may be objected that waiting for evaluation results lengthens the time needed to set a new programme in motion. This, however, is a problem only if the first priority is to launch a new programme regardless of whether it works or not. The last two decades have taught us that new programmes are two a penny; hard information on their effectiveness is rare. High unemployment is not going to go away for some years yet. In this context it is surely better to take a long-term view, and to spend the time needed to ensure that government programmes for unemployed people are of value both to their clients and to the economy as a whole.

Conclusions

The problems with the data that we have described at some length make it difficult to summarise our findings about Project Work. The following conclusions are offered as the most reasonable judgements that can be made on evidence that is far from perfect.

- Project Work probably increased the rate at which long-term unemployed people left claimant unemployment.
- This appeared to be largely because people signed off to avoid going on compulsory work experience, rather than because they benefited from work experience.
- The register impact of Project Work decayed over time.
- Project Work may have slightly increased the rate at which long-term unemployed people got jobs, but the evidence for this is less certain.
- If the programme did increase the rate at which long-term unemployed people got jobs, this was largely by increasing rates of entry to part-time and temporary jobs, rather than to full-time permanent jobs.
- Part at least of Project Work's apparent register impact was attributable to an increased rate of transfer of long-term unemployed people to other types of social security benefits. This was particularly true for older people and for people who had been unemployed for a very long time.
- Another part of Project Work's apparent register impact was attributable to an increased rate of transfer of long-term unemployed people to government training programmes.
- The register impact of Project Work was greater for people who had been unemployed for a long time than for people who had just reached two years of unemployment.
- Project Work had no clear impact on wages.
- Evidence about the impact of Project Work on job search was confused.

Appendix 1: Definitions of variables in the main and leaver surveys

Programme variables

M/MPILOT	Medway/Maidstone pilot offices
HULLPILOT	Hull pilot offices
M/MCOMP	Comparison offices for Medway/Maidstone
HULLCOMP	Comparison offices for Hull
M/MWKST	Offered Workstart cards in Medway/Maidstone pilot offices
HULLWKST	Offered Workstart cards in Hull pilot offices
M/MJFG	Told about Jobfinder's Grant in Medway/Maidstone pilot offices
HULLJFG	Told about Jobfinder's Grant in Hull pilot offices

Control variables

Work history

LASTJCONS	Last job before qualifying spell of unemployment in construction industry
LASTJMANF	Last job before qualifying spell of unemployment in manufacturing industry
LASTJSOC5	Last job before qualifying spell of unemployment craft and related occupation
LASTJSOC8	Last job before qualifying spell of unemployment plant or machine operative
LNGTHUNEM	Length of qualifying unemployment spell triggering entry to Project Work or to comparison sample (continuous)
WHCASUAL	Has done mainly casual or short-term work
WHSTEADY	Spent most of working life in steady jobs
WHUNEMP	Spent more time unemployed than in work

Job search (Note: these variables refer to the defined job search period; see text)

ACCPTTEMP	Was willing to accept temporary work
MAXHOURS	Maximum number of hours willing to work per week (continuous)
HRSSPENT	Number of hours spent looking for work per week (continuous)
PARTICJOB	Was looking for a particular job
RANGEJOBS	Looking for a range of jobs
NUMMETHS	Number of job search methods used (continuous; range 0-11)
LIMITED	Job search was limited by the costs involved
RESNWAG	Lowest hourly take-home pay was willing to work for (reservation

| E | wage) |
| LOOKFT | Was looking for a full-time job |

Personal

AGE	Age (continuous)
AGE45UP	Aged 45-51
FEMALE	Respondent is female
DRVNGLIC	Has driving licence
CSE	Highest academic qualification CSE or GCSE grades D-F
OLEVEL	Highest qualification is GCE O Level or GCSE grades A-C
POSTOLEV	Highest academic qualification is above GCSE O Level standard
ACADQUALS	Has academic qualifications (eg GCSE)
VOCQUALS	Has vocational qualifications
HEALTHPRB	Has a long-term health problem or disability
MINORITY	Member of a minority ethnic group

Control variables continued…

Household

LIVESPARS	Lives with parents
MARRIED	Married or living with a partner
NCHILD1	Has one dependent child
NCHILD2	Has two dependent children
NCHILD3	Has three or more dependent children
KIDAGE0-2	Youngest child is aged 0-2 years
KIDAGE3-4	Youngest child is aged 3-4 years
KIDAGE5-10	Youngest child is aged 5-10 years
SPOUSEEMP	Spouse or partner is in employment
OTHEREMP	At least one other adult in the household is in employment (other than spouse/partner)
OWNEROCC	Owner-occupier

First job since qualifying spell of unemployment (main survey) or job at interview (leaver survey)

NONPERM	Temporary or casual
SELFEMP	Self-employed
SOC9	Unskilled occupation
CONSTRCTN	Construction industry
UNION	Trade union at workplace

Attitudes

NOCHANCE	Thinks unemployed people's job chances are very bad

Local labour market conditions

UNEMDIFF	Change in TTWA unemployment rate June 1996-December 1996

Sample selection variable

LAMBDA	Sample selection variable for having worked since the qualifying spell of unemployment

Appendix 2: Workstart cards and Jobfinder's grants

The analyses of the impact of Workstart cards and Jobfinder's grants were carried out at the request of the DfEE and ES. We place the results in an appendix partly because the issues they deal with stand somewhat separate from the main themes of the report, but more importantly because we feel that the data did not permit us to give proper answers to the questions posed.

As we saw in Chapter 1, the Workstart wage subsidy, which at the time the first Project Work pilots were running was worth up to £1,500 to employers who took on long-term unemployed workers, was a distinctive feature of both the Medway/Maidstone and Hull pilots.[30] In addition, the discretionary stepped Jobfinder's grant was piloted in Hull simultaneously with Project Work, though flat-rate Jobfinder's grants were available in the comparison offices as well as Medway/Maidstone. There was interest in the impact of these two initiatives, but unfortunately the design of the evaluation study did not really permit this to be tested. As everyone from the pilot offices was supposed to have been offered both Workstart cards and Jobfinder's grants, the impact of these could not be separated from the impact of the wider Project Work programme with which they were bound up. Obviously it was not sensible to compare the progress of people from the pilot offices who did and did not receive the Workstart subsidy or Jobfinder's grant, as both of these were given only if the person actually got a job. The difficulty was increased by the fact that the survey questionnaire did not ask sample members from the comparison offices about Jobfinder's grants, although they were likely to have been offered these in their Restart interviews.

While all sample members in the pilot offices were supposed to have been offered Workstart cards and told about Jobfinder's grants, not everyone recalled this when asked in the survey interview (for example, only two in three recalled being told about Jobfinder's grants). In terms of the evaluation, the best we could do therefore was to see whether there was any difference between the progress of people in the pilot offices who did and did not remember. The exercise was not particularly useful, for obvious reasons. We were not able to distinguish between people who had really not been told about Workstart or Jobfinder's grants, and people who had been told about them but had not understood or had forgotten what they were told. People who did not understand or remember would have been more likely than those who did understand and remember to have had other difficulties that impaired their chances of getting a job. Finally, people who had subsequently used their Workstart cards in a job interview or received a Jobfinder's grant because they got a job were more likely than others to remember being told about these things. For all these reasons, it was not possible to draw any clear inferences from the analysis results.

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[30] The current value of the subsidy is £1,560 for a worker aged 18-24 and £1,950 for a worker aged 25 or more.

Model 1H in Table A2.1 added to Model 1E variables indicating whether people in the pilot offices recalled being offered Workstart cards (M/MWKST and HULLWKST), so the effect of recalling this was assessed relative to the effect of not

Table A2.1
Main survey logistic regression models for economic activity status with additional programme variables (Models 1H and 2H)

	Model 1H Any work since qualifying spell of unemployment *coefficient*	Model 2H In work at interview *coefficient*
Constant	-0.23	0.06
Programme effects		
(relative to pooled comparison sample)		
M/M	0.80***	0.82***
HULL	0.59*	0.78**
M/MWKST	0.84	1.51***
HULLWKST	0.62**	0.50
M/MJFG	-0.80**	-0.68*
HULLJFG	-0.68**	-0.89**
Control variables		
Work history		
WHSTEADY	0.53***	0.60***
WHCASUAL	0.32	0.25
LASTJMANF	0.44**	0.49**
LNGTHUNEM	-0.01****	-0.96****
Job search		
RANGEJOBS	-0.54***	-0.57***
ACCPTTEMP	0.75****	0.62***
NUMMETHS	0.07*	0.09**
MAXHOURS	-0.02**	-0.05****
RESNWAGE	-0.29****	-0.39****
Personal & household		
AGE45UP	-0.70****	-0.61***
MARRIED	0.84****	1.28****
OWNEROCC	0.46**	0.59***
OTHEREMP	0.56***	0.55**
DRVNGLIC	0.52***	0.45***
Log-Likelihood	*-565*	*-475*
Degrees of freedom	*1083*	*1083*

*Significance levels: * 10%;   ** 5%;   *** 1%;   **** 0.1%.*

recalling Workstart (for people in the pilot offices) or of being in a comparison office. It shows that people in the Hull pilot offices who recalled being offered Workstart cards were more likely to have got work. In Medway/Maidstone, however, this association was not statistically significant. Although the coefficient for M/MWKST was bigger than the coefficient for HULLWKST, its standard error was very large, suggesting problems with the data. Model 1H also added two variables for recalling being told about the Jobfinder's grant (M/MJFG and HULLJFG), and similarly the effect of these variables was assessed relative to the effect of not recalling the Jobfinder's grant (for people in the pilot offices) or of being in a comparison office. In this case there was a significant association in both of the pilot areas with getting a job, but the association was in the opposite direction from that expected: people who recalled being offered the Jobfinder's grant were *less* likely to have got work. This result is hard to interpret. It may possibly indicate that client advisers stressed the Jobfinder's grant more heavily when dealing with long-term unemployed people whom they felt needed extra incentives to look for work, but it is more likely that it was simply a product of the way the evaluation was structured, which (in addition to the other problems discussed in Chapter 1), here did not identify people in the comparison offices who were told about the Jobfinder's grant.

Model 2H in Table A3.1 added the same four new variables to Model 2E, for the probability of being in work at the time of interview. In this model remembering being offered Workstart was significantly and positively associated with being in work in Medway/Maidstone, but was non-significant in Hull. This instability in the results again casts doubt on their interpretability. For the Jobfinder's grants, the association with being in work was again negative.

Appendix 3: Preparation of JUVOS flow file and training file data
for analysis

The ES supplied, for both the pilot and comparison samples, a file containing the date of the invitation to attend the Restart interview that triggered sample members' entry to Project Work or to the comparison sample, the start date of the spell of unemployment that made them eligible for entry (the 'qualifying spell'), and a limited number of demographic variables. They also supplied, for both samples, the start dates of all subsequent claims for unemployment related benefits (extracted from the JUVOS on-flow file), plus a separate file with claim end dates and the reason why each claim ended (extracted from the JUVOS off-flow file). For the pilot sample only, a further file was supplied containing start and end dates for all claims for a training allowance under the work experience element of Project Work together with some administrative details of the claims (extracted from the JUVOS training file).[31]

Table A3.1 lists all the JUVOS exit codes, and shows how they were grouped to form the classification of labour market status used in the analyses in Chapters 6 and 7.

Our first task was to construct a unified history for each person from these separate files, which sorted all dates into chronological order. This history covered the period from the start of the qualifying spell of unemployment up to June 12th 1997, the latest date for which we had information in all files. Checks on this unified history enabled us to detect inconsistencies in the dates of claims. These checks revealed a great number of problems, and so, in consultation with experts in the ES, procedures were developed to edit out inconsistencies, thus allowing us to retain most cases in the analysis. This process was particularly important as inconsistencies occurred much more frequently in the pilot sample than in the comparison sample, for the simple reason that there were four different types of dates to be reconciled in the pilot sample data but only two types of dates to be reconciled in the comparison sample data.

Our edits involved a number of assumptions which need to be made explicit. They were of two kinds: computerised rules, applied via a (very complex) computer program, and hand edits, which could in principle be specified by computer program but where too few cases were involved for the programming effort to be worthwhile. To simplify description, we refer to dates by letter, as follows:

A   start date of a claim for unemployment related benefits
B   end date of a claim for unemployment related benefits
C   start date of a claim for training benefit (pilot sample only)
D   end date of a claim for training benefit (pilot sample only).

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[31] People can receive a training allowance for reasons other than being on the work experience element of Project Work, but such cases were excluded from the file sent to us.

The first date in the history was always the start of the qualifying spell of unemployment. For example, the history of someone who did a spell on work experience under Project Work and then returned to claimant unemployment was represented by the sequence A-B-C-D-A.

Table A3.1
JUVOS exit codes and their relationship to the classification of labour market status used in Chapters 6 and 7

JUVOS code	JUVOS description	Labour market status classification
A*	Ceased claiming	No information
B	Found work	Paid work
C	Gone abroad	Censored
D	Claimed another benefit	Claiming other benefits
E	Started full-time education	Education/training
F,W,Y*	Not known	No information
G	Deceased	Censored
H*	Failed to attend	No information
I,U	In training	Education/training
J	Reached retirement age	Censored
K,O	Auto Jobseeking period	Censored
L	Claimed sickness benefit	Claiming other benefits
M	Transferred to Government-supported training	Education/training**
N	Working on average 16 hours per week or more	Paid work
Q	Claimed INCAP	Claiming other benefits
R	Claimed Income Support	Claiming other benefits
S*	Defective claim	No information
T	In prison	Censored
V	Attending court	Censored
X	Transferred to clerical***	-

* These codes often occur when there are other inconsistencies in the apparent sequence of events.

** This includes signing off to start work experience under Project Work, but our criterion for Project Work work experience is the receipt of the Project Work training allowance.

*** These are claims that are too complex to be handled by the computerised system. If the client leaves the register while his or her claim is still being

handled clerically, then the JUVOS off-flow files contain no information on the date the claim ended or the exit code.

For members of the comparison sample, the only legitimate sequence was A-B-A-B-A, prolonged indefinitely or curtailed at any point. Comparison sample members had no C or D dates, as no data were extracted from the JUVOS training file for them. For those who never left claimant unemployment, the whole history was represented by a single A.

For members of the pilot sample, the sub-sequences A-B and C-D could be repeated several times and in any combination. However, to be consistent, every B date needed a preceding A date, which had to be the last date before the B date, and every D date needed a preceding C date, which had to be the last date before the D date.

Computerised rules to correct inconsistencies in dates

<u>Rule 1</u>: Sunday dates are used in the JUVOS off-flow file to close a claim which has ended if the exact end date is not known. Thus the inconsistent sequence C-A-D, where D was a Sunday date, was changed to the consistent sequence C-D-A, by changing the date of D to the day before the date of A.

<u>Rule 2</u>: There was a frequently occurring inconsistent sequence C-C-D-D, where the two Cs were identical dates and the second D date was a Sunday about a month after the first D date. Sometimes a single A date or the sub-sequence A-B intervened between the two D dates. We assumed that this sequence represented duplicate entries for the same spell on training benefit, with the start date indicated by C and the end date indicated by the first D date.

<u>Rule 3</u>: The inconsistent sequence C-A-D, where A and D occurred in the same calendar week, was assumed to be caused by a slight delay in closing the training benefit claim, and was changed to C-D-A by inverting the A and D dates.

<u>Rule 4</u>: The inconsistent sequence A-C-D occurred frequently, and often when the C and D dates were very close together. We assumed that in these cases there had been a failure to up-date the JUVOS off-flow file. We reconciled the inconsistency by imputing a new B date for the day before the C date, thus creating the sequence A-B-C-D. The similar inconsistent sequence A-C-D-B was reconciled by additionally inserting a new A date for the day after the D date, thus creating the sequence A-B-C-D-A-B.

Hand edits to correct inconsistencies in dates

Hand edits, based on individual inspection of the remaining problematic cases after the computerised rules had been applied, were carried out only if they involved very plausible assumptions or made a negligible difference to the person's history. The corrections were based on all available information, including Sunday dates, the JUVOS unemployment exit code, the fact that the maximum period on work experience under Project Work was 13 weeks, and the fact that if people signed on again as unemployed they were sent back onto work experience until they had completed their 13 weeks. The policy was to make the minimum number of changes compatible with rendering the case consistent. The range of errors we found suggested a degree of confusion in local ES offices over whether and how people should be signed off claimant unemployment before entering work experience under Project Work.

Common problems in the data extracted from the JUVOS training benefit file included:
• missing end dates for the work experience spell (especially if the compulsory 13 weeks on work experience were due to expire just before Spring Bank Holiday 1997);

- missing intermediate start and end dates of repeated short spells on work experience when these were interleaved with short spells of claimant unemployment.

Common problems in the data extracted from the JUVOS on-flow and off-flow files included:
- missing claim end dates before the start of a spell on work experience;
- a claim end date shortly after the start of work experience, with a JUVOS exit code indicating that the reason for leaving claimant unemployment was unknown;
- two claim end dates very close together without an intermediate claim start date, the first with JUVOS exit code M suggesting transfer to work experience, the second with a quite different JUVOS exit code;
- (a variant of the above) two claim end dates very close together without an intermediate claim start date, the first with JUVOS exit code B (found work); the second with JUVOS exit code M suggesting transfer to work experience and followed by a spell of work experience;
- (another variant) a claim end date followed by the start of work experience, but shortly afterwards a second claim end date with JUVOS exit code B (found work).

The large majority of inconsistent cases were corrected by applying the computerised rules, and a further 189 cases were corrected by hand editing. In total, three per cent of the original 11,829 people for whom we received data had to be excluded from the analysis because of irreconcilable inconsistencies in the date sequence, or, in a small number of cases, because of missing information on key variables. Many of these insoluble problems - including the majority of problems in the data for the comparison sample - are likely to be explained by the procedure whereby continuing claims for unemployment-related benefits that become very complicated are transferred out of the JUVOS system to be dealt with manually. On the full JUVOS data base these claims are coded X ('transferred to clerical'), but this code is not carried across to the off-flow files (because at this point the client has not yet left the register), and no record appears in the off-flow files if the client signs off while the claim is still being handled clerically. Thus it appears on the flow files as though the claim has started but not ended, leading to apparent inconsistencies in the sequence of dates.

Once the data had been cleaned, we constructed a week-by-week event history for each person that ran from the start of the qualifying spell of unemployment up to June 12th 1997. This history used the information in the JUVOS exit codes, as explained in Chapter 6. It was based on the main activity in each week, defined as the activity that occupied the most days in the week, or as the first activity, if two or more activities in one week occupied an equal number of days. With the data set up in this format, further analysis became relatively straightforward.

Appendix 4: Additional tables

Table A4.1
Data for Graphs 6.1 - 6.4

	Week 5	Week 10	Week 15	Week 20	Week 25	Week 30	Week 35	Week 40	Week 45	Week 50
(a) Graph 6.1 (Hull - all with data)										
Pilot sample										
% unemployed/on PW work experience	92.6	83.2	77.0	63.6	53.3	50.4	50.4	51.9	52.8	52.8
% in work	3.3	7.9	11.0	13.9	16.4	16.5	16.8	16.9	16.8	17.4
Comparison sample										
% unemployed/on PW work experience	94.8	89.1	84.8	81.6	79.5	77.3	75.0	73.0	72.0	70.7
% in work	2.6	5.5	7.5	8.6	9.4	10.6	12.0	13.2	13.2	14.4
(b) Graph 6.2 (Hull - those with week 50 data only)										
Pilot sample										
% unemployed/on PW work experience	91.9	81.8	76.2	57.1	48.5	48.8	49.8	51.2	52.2	52.8

% in work	3.7	8.6	11.4	14.9	16.5	16.3	16.3	16.5	17.0	17.4
Comparison sample										
% unemployed/on PW work experience	96.0	90.6	86.5	82.8	80.0	77.9	76.0	73.5	71.6	70.7
% in work	2.1	4.6	6.1	8.2	9.5	10.6	11.8	12.8	13.6	14.4

Table A4.1 continued…

	Week 5	Week 10	Week 15	Week 20	Week 25	Week 30	Week 35	Week 40	Week 45	Week 50
(c) Graph 6.3 (Medway/Maidstone - all with data)										
Pilot sample										
% unemployed/on PW work experience	94.9	86.8	81.5	63.8	55.0	53.9	53.1	52.9	50.5	50.6
% in work	1.5	5.0	8.0	12.3	14.6	14.7	16.0	16.4	17.3	16.9
Comparison sample										
% unemployed/on PW work experience	94.0	86.5	80.0	74.2	70.5	68.7	66.0	66.0	64.9	65.9
% in work	1.6	3.6	5.9	8.4	9.1	10.8	13.0	13.1	13.2	12.6
(d) Graph 6.4 (Medway/Maidstone - those with week 50 data only)										
Pilot sample										
% unemployed/on PW work experience	95.3	86.1	81.7	56.5	51.2	50.9	53.8	51.8	51.2	50.6
% in work	0.9	3.8	6.8	13.0	14.5	15.1	15.7	16.3	16.0	16.9
Comparison sample										
% unemployed/on PW work experience	96.2	90.5	83.9	79.2	73.8	71.9	68.8	67.5	67.5	65.6
% in work	0.9	2.2	4.1	5.7	7.9	9.1	11.4	12.6	11.7	12.6

Note: Graphs 6.3 and 6.4 are based on the uncontaminated Medway/Maidstone sample.

Table A4.2
Administrative data logistic regression models for unemployment probabilities at specified intervals after the triggering Restart invitation, Hull April 1996 to January 1997 sample (Models 20A-20D)

	Model 20A	Model 20B	Model 20C	Model 20D
	Week 20 All with valid data	*Week 20* Survivors to week 40	*Week 35* All with valid data	*Week 40* All with valid data
	coefficient	*coefficient*	*coefficient*	*coefficient*
Constant	0.26	0.00	0.73	0.56
Programme effects:				
Pilot sample	-1.06****	-1.44****	-1.18****	-1.03****
Date of Restart invitation:				
Spring	0.02****	0.08*	-0.01	-0.01
Length of qualifying unemployment spell:	0.39****	0.29**	0.27***	0.32**
Log weeks				
Demographic characteristics:				
Male	0.29***	0.29	0.25*	0.33*
Aged under 25	-0.00	-0.02	-0.07	-0.14
Aged 35-44 years	-0.07	-0.22	0.06	0.06
Aged 45+	0.19*	-0.17	0.11	0.09
Married	-0.31***	0.03	-0.25*	-0.01
Separated/widowed/divorced	0.04	-0.09	-0.26	-0.26
Marital status not known	-0.03	0.57	-0.77*	-0.54
Log likelihood	*-2760*	*-593*	*-1182*	*-623*
Sample size	*4,801*	*943*	*1,840*	*943*

Significance levels: * 10%; ** 5%; *** 1%; **** 0.1%
Reference categories: Hull comparison offices (Fakenham, Barton, Newport, Chapletown, Great Yarmouth, Hillsborough); Restart invitation received in summer, autumn or winter 1996; female; aged 25-34 years; single.

Table A4.3
Administrative data logistic regression models for work probabilities at specified intervals after the triggering Restart invitation, Hull April 1996 to Jan. 1997 sample (Models 21A-21D)

	Model 21A	Model 21B	Model 21C	Model 21D
	Week 20 All with valid data	*Week 20 Survivors to week 40*	*Week 35 All with valid data*	*Week 40 All with valid data*
	coefficient	*coefficient*	*coefficient*	*coefficient*
Constant	-1.45	-1.86	-1.32	-1.22
Programme effects:				
Pilot sample	0.63****	0.83***	0.36**	0.48**
Date of Restart invitation:				
Spring	-0.00	0.02	-0.01	0.00
Length of qualifying unemployment spell:	-0.60****	-0.88****	-0.52****	-0.77****
Log weeks				
Demographic characteristics:				
Male	-0.20	-0.06	0.16	0.04
Aged under 25	-0.02	0.18	-0.09	-0.33
Aged 35-44 years	-0.16	0.04	-0.18	-0.00
Aged 45+	-0.40**	0.39	-0.34	0.17
Married	0.62****	0.33	0.31*	0.03
Separated/widowed/divorced	0.18	0.46	0.12	0.18
Marital status not known	0.55	-0.04	0.25	0.42
Log likelihood	*-1711*	*-349*	*-751*	*-390*
Sample size	*4,801*	*943*	*1,840*	*943*

Significance levels: * 10%; ** 5%; *** 1%; **** 0.1%
Reference categories: Hull comparison offices (Fakenham, Barton, Newport, Chapletown, Great Yarmouth, Hillsborough); Restart invitation received in summer, autumn or winter 1996; female; aged 25-34 years; single.

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