Getting the Unemployed Back to Work: Evaluating The Impact of A Temporary Wage Subsidy and Training Programme

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Abstract

This paper examines alternative approaches to wage subsidy programs. It does this in the context of a recent active labour market reform for the young unemployed in Britain. This "New Deal" reform and the characteristics of the target group are examined in detail. We discuss theoretical considerations, survey the existing empirical evidence and propose two strategies for evaluation. The first suggests an *ex-post* 'trend adjusted difference in difference' estimator. The second, relates to an ex-ante evaluation. We present the conditions for each to provide a reliable evaluation and fit some of the crucial parameters using data from the British Labour Force Survey. We stress that the success of this type of labour market programmes hinge on dynamic aspects of the youth labour market, in particular the pay-off to experience and training.

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1 Introduction

In 1997 the newly elected UK government announced a general package of welfare to work reforms directed toward the low wage labour market.. A major component of that reform package was an active labour market programme aimed specifically at the young unemployed.. Entitled "the New Deal for the Unemployed Youth" this was targeted at all young people aged between 18 to 24 years old who had been claiming job seekers allowance (unemployed benefit) for six months or more. All individuals who meet this eligibility requirement, from April 1998 onwards, were eligible for this program. From June 1998 all adults (25 or older) unemployed for over two years were also covered by the scheme. This paper assesses alternative approaches to the evaluation of labour market programmes of this type and provides an initial evaluation of this specific reform.

The programme operates in the following way: After an initial "Gateway" period (see below), four options are presented to the unemployed. A key option is a voucher for a subsidy to a prospective employer of £60 per week (for 6 months) if she hires the job seeker.[1] The New Deal has been financed by a £5.2 billion windfall tax on the privatised utilities[2] and will be spread out over a number of years. The aim of the intervention is to enhance the employability of the long-term unemployed.

Since the programme is still in its initial stages is too early to attempt a full evaluation, but it is important to consider the salient issues involved in performing a proper evaluation. The approach we follow is eclectic. First, we summarise some of the salient details of the initiative and examine the

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[1] As the Chancellor Gordon Brown put it "There will be no fifth option - to stay at home on full benefit", see Budget Statement, Hansard, 2 July 1997.

[2] According to HM Treasury (1999) £2.55bn of the total has been allocated specifically to the 18-24 group (1997-2002). See Chennells, 1997, for details of the windfall tax.

characteristics of those in the target group, the so-called 'New Dealers'. Second, we draw on simple economic models to illustrate how the effects of the New Deal are likely to affect individuals, employers and the labour market as a whole. The crucial point here is the need to take an intertemporal or dynamic approach in order to understand whether the programme will have any long-run effects on the employment probabilities of the target group. Will participants in the program be able to hold on to a job when their subsidy runs out? Third, (after a brief survey of the existing evidence on wage subsidies) we propose an empirical strategy for an ex-post evaluation of the scheme using a 'trend adjusted difference in difference' approach. We stress the difficulties arising from the absence of any obvious *control group* with which to the New Deal *treatment group*. The alternative method is to build an ex ante general equilibrium model of the labour market and use existing information to calibrate the parameters of this model. Although such a task is beyond the scope of this paper we do discuss what would be the key parameters of the structural model. We then use existing data from the Labour Force Survey (LFS) quarterly panel try to estimate upper bounds on the likely effects of the New Deal.

To pre-empt the conclusion, we argue that the effects of the New Deal may be far more modest than its architects have hoped. Fundamentally this is because of the difficulty of raising the skill levels of the unemployed merely by employment spells (although improving the training element will be very important here). More positively, we suggest (i) ways in which the New Deal could be fine tuned in order to better meet its stated objectives and (ii) methodologies to construct the appropriate 'control group' with which to compare the young New Dealers

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2   What is the New Deal Reform?

The British Chancellor announced the details of the welfare-to-work program, called the New Deal, in the Summer 1997 Budget. The initiative was officially launched on April 6th 1998 although there were Pathfinder area pilots as early as January 5th of the same year[3].

For those aged 18-24 and with an unemployment duration of at least six months, four New Deal options are available[4]. These are:

1. A subsidy equal to £60 (U.S. $100 approximately) per week for 26 weeks to be provided directly to an employer.

2. A job for 6 months on the Environment Taskforce with a wage or an allowance equal to JSA (unemployment benefit) plus £400

3. A job for 6 months with a voluntary sector employer with a wage or an allowance equal to JSA (unemployment benefit) plus £400

4. Entry into full-time education or training for 12 months without loss of benefits for those lacking basic qualifications (S/NVQ Level 2 or below).

In the first three options the employer must provide the equivalent of at least one day of education or training per week designed to reach an accredited qualification. A sum of £750 is available to meet these training costs paid in four installments[5].

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[3]There have been other pilot schemes with a very similar design to the New Deal. These Workstart programmes are discussed in the survey section in Annex I below.

[4]Some individuals are allowed to be 'early entrants'. These are usually individuals with particular needs such as those with diabilities, lone parents, etc.

[5]For employers who are identified in areas of particular skill shortage an 'Upfront Skill Shortage Subsidy' (announced in December 1998) is available. 75 per cent of this is paid

Before these options are available to an individual, there is a 'Gateway' period lasting for up to four months. During this period the individual receives extensive help in job search. A specially trained "personal advisor" from the local Employment Service is assigned to the job seeker. They meet at least every two weeks and the personal advisor intensively counsels the job seeker on the best ways to improve their employability.

A substantial proportion of the unemployed are moved off the register during this Gateway period and will not be offered the four options. By January 1999 about 100,000 young people had passed through the Gateway. 44 per cent of these had moved into unsubsidised jobs, 13.7 per cent into subsidised employment, 27.1 per cent into full time education and training and 14.8 per cent into the other options (New Deal Press Release 28.1.99).

Importantly, there is an element of compulsion in the proposals. Individuals who refuse to take a place in one of the options will be required to take up a place identified for them by the Employment Service. Failure to comply without good cause may result in benefit sanctions being applied. Sanctions are initially the withdrawal of benefits for two weeks. Further refusals will result in repeated four-weekly withdrawal of benefits. About 3 per cent of New Dealers had sanctions applied in the first 6 months of the scheme (New Deal Press Release 513/98). The introduction of strictly time limited benefits has several implications for the success of the scheme and also its evaluation which we discuss in more detail below.

For those aged over 25 with an unemployment duration of over 2 years, two options are under the New Deal. These are:

- A subsidy equal to £75 per week for 26 weeks to be provided directly

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at the start of the job (£1730) and the rest (£580) 26 weeks later after the achievement of agreed training objectives. The employer has to guarantee the job lasts for a year and allow at least 15 days training in the first 8 weeks.

to an employer.

- entry into full-time employment related education courses for up to 12 months with no loss of benefit.

There has been an expansion of New Deal provisions for different target groups. There are New Deals for the Disabled, for Lone Parents, for the Partners of the Unemployed, for the Over 50s and even for Schools! These offer many of the same features which have been pioneered on the younger groups (employment subsidies, intensive job search help, training subsidies). Anyone re-entering unemployment after being on a New Deal option will be able to claim JSA under the same conditions but will re-enter a "follow through" scheme similar to the Gateway. There is a Web site which holds up to date information on the programme [www.newdeal.gov.uk].

3   Who are the Young New Dealers?

3.1   Characteristics of the young unemployed in Britain

In this section we use data from the British Labour Force Survey (LFS) to give some indication as to the type of individuals that will be affected by the New Deal. We focus on recent LFS data *before* the introduction of the New Deal in order to provide a benchmark for evaluation. The New Deal will affect both the economy and also how we interpret the statistics (for a discussion of the effect of New Deal on labour market statistics see Wood, 1998). Table 1 uses LFS data from December 1997-February 1998. The long-term unemployed are disproportionately male and low-skilled. 80 per cent left school at 16 compared with 60 per cent in the corresponding employed group. Among the young unemployed there is a similar large gap in educational attainment compared to those in work. Hence the key characteristic

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of those eligible for the New Deal is their low level of skill and consequent low productivity.

Table 1. Characteristics of the Unemployed

	E	U	U > 2yrs	E	U > 6mths
	25-64	25-64	25-64	18-24	18-24
% male	53.66	61.25	75.43	51.95	67.2
% leaving school at 16	60.13	72.31	80.82	38.63	62.80
% with degrees	15.69	8.34	4.76	9.93	5.65
% with no quals	14.92	27.63	40.21	5.42	21.51

Source: LFS Dec 1997-Feb 1998

Table 2 shows the actual numbers eligible for the New Deal in the first quarter of 1998. We report two measures of the numbers of unemployed by duration. The ILO measure uses the standardised definition of unemployment while the claimant count uses the administrative figures of those registered as unemployed and claiming benefit. It is well known that these measures differ significantly. For example, figures reported in *Labour Market Trends* show that in Spring 1998 there were 1.840m ILO unemployed in Great Britain compared with the April claimant count number of 1.390m. Even these comparisons hide dramatic differences since within the total ILO figure, only about 50 per cent are also on the claimant count. Thus even though the aggregate figures are broadly similar under either definition for the 18-24 year olds it would be incorrect to assume that the same people were in both measures.[6] Figure 1 illustrates the different distributions of unemployment durations for the young and older workers. It is clear that long-term employment is a far greater problem amongst the old than among the young.

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[6]The LFS does allow the researcher to condition on claimaint status. Unfortunately there is a significant undercount of claimaints using the LFS data. Published figures in *Labour Market Trends* simply apply a scaling factor to the LFS data to make it consistent with the claimant count.
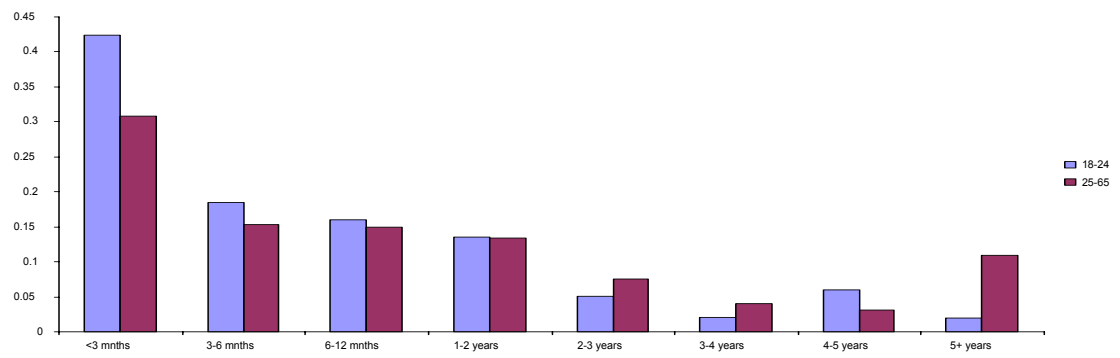
Figure 1: Unemployment by Duration, Winter 1997

Table 2. Unemployment by Duration in GB, January 1998

Duration	Ages 18-24 ILO Measure	Ages 18-24 Claimant Count	Ages 25-64 ILO Measure
<3mth	178,885	175,300	375,017
3-6mth	77,960	81,500	186,683
6-12mth	67,419	64,700	181,610
12-24mth	56,860	35,000	162,771
24+	40,751	18,800	311,778
Total	421,858	375,200	1,217,883

Sources: Labour Force Survey, Dec 1997-Feb 1998
Labour Market Trends Table C12, September 1998

It is of interest to know what would happen to unemployed individuals in the absence of a welfare-to-work programme. Table 3 gives the six-month transition rates for different groups of unemployed people. Individuals who are ILO unemployed are examined 6 months later. We use data between September 1996 through February 1998 from the panel element of the LFS (i.e. pre-New Deal data). We present the data pooled over 4 waves, so we examine ILO unemployed in September 1996 and their labour market state in February 1997, ILO unemployed in December 1996 and their status in May 1997, etc. This is in order to keep a reasonable sample size in each cell[7].

Potential duration dependence is evident in that the transition rate into employment falls significantly for those with long unemployment spells. For example, for those with spells that have already lasted 2 years of more, the probability of moving into employment over the next six months is only 17%. Such low transition rates explain the motivation for concentrating on such long-term unemployed people in the welfare-to-work policy proposals. In contrast, the transition rates into employment for the young unemployed are much higher. For those with 6-12 month spells, 41% will find employment

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[7]We have also examined the nine monthly transitions and the data on a quarter by quarter basis. The qualitative pattern is the same as the pooled results (all results available on request).

within six-months. This gives a rather crude indication of the magnitude of the simple deadweight losses involved in the New Deal. Figures for women show that transitions into employment are the same as for men but there are significantly more movements out of the labour force.

Table 3. Six-Month Transition Rates

	Employed	Unemployed	Out of LF	Student	*Observations*
Men					
All U, 25-60	31.7	55.5	11.9	0.9	*4009*
>6mth U, 25-60	22.4	64.5	12.7	0.5	*2652*
>2yrs U, 25-60	16.8	69.8	13.3	0.07	*1494*
All U, 18-24	39.9	48.5	5.5	6.1	*1179*
>6mth U, 18-24	33.8	58.2	5.8	2.2	*586*
6-12mth U, 18-24	41.2	50.5	5.2	3.1	*192*
Women					
All U, 25-60	34.8	37.5	25.8	1.9	*2697*
>6mth U, 25-60	23.3	49.2	25.6	1.9	*1290*
>2yrs U, 25-60	15.1	58.7	24.7	1.5	*470*
All U, 18-24	43.6	34.0	15.5	7.0	*718*
>6mth U, 18-24	33.1	45.5	18.2	3.3	*242*
6-12mth U, 18-24	38.3	44.9	14.0	2.8	*107*

Source: LFS Panel, September 1996-February 1998; all ILO unemployed individuals sampled in September 1996 through May 1997; transitions are based on employment status two quarters later.

3.2   Tracking a youth cohort

To get more information on the outcomes that unemployed workers experience we followed a set of 18-24 year olds who had been unemployed for six months or more. We sampled those who had obtained employment by the next quarter of observation and then tracked them for another two quarters. It should be noted that the sample sizes involved are rather small so the results should be viewed as no more than indicative.

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Our estimates show that after six months, 64% of the sample are still in employment while 33% have returned to unemployment. This is a surprisingly high re-unemployment rate and suggests that one mechanism by which the New Deal may be effective is in keeping these low skilled young workers in continuous employment for at least six months. Furthermore, we find that only about 30% of those who enter employment receive training of any form and a very small proportion receive significant training toward an accredited qualification. All those who enter New Deal will receive some accredited training.

3.3   How much is a £60 wage subsidy actually worth?

The value of the subsidy of £60 per week for those aged 18-24 clearly depends on the productivity of the target group. Table 4 gives some figures from the LFS. We focus attention on the bottom decile of wages since we know from the above discussion that the vast majority of the unemployed eligible for the New Deal are low-skilled low-productivity workers who are unlikely to find employment in the middle of the wage distribution. From this perspective £60 per week is a large fraction of the gross wage, representing over 40% of the wage at the bottom decile for 21-24 year old males. Another way to look at this is to consider that these are likely to be workers employed at National Minimum Wage introduced in April 1999 minimum wage of £3.20 an hour[8]. For a 35 hour week, the subsidy is worth about 54% of weekly earnings. Workers between 18 and 21 will initially have a lower wage (£3.00 per hour) and lower average earnings so the subsidy is even more generous.

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[8]Until April 1999 there was no National Minimum Wage in the UK. The minimum wage is £3.60 for older workers. There were sector specific mimimum wages set by Wages Councils which covered about 2 milliion workers when they were abolished by the previous Conservative government in 1993.

Against this, there are several hidden costs to the firm. Employees are given a day a week off for training and although the employer is compensated for this with the £750 payment, the disruption and costs may be significant (for example other co-workers will have to provide some on-the-job training). Furthermore, there are the costs of bureaucratic compliance including the government monitoring of training, employment conditions and assorted red tape. This may be part of the reason why take-up of the subsidised employment option has been surprisingly low.

Table 4. Gross Weekly Wages, 1997/8

	Mean	D1	D5	D9	*Obs*
Males					
Age 18-20	163	99	150	239	*679*
Age 21-24	241	140	221	357	*1441*
<1yr Tenure, Age 21-24	226	133	209	342	*541*
<1yr Tenure, Left School at 16, Age 21-24	212	125	200	323	*228*
1-2yr Tenure, Age 21-24	246	150	223	369	*296*
Females					
Age 18-20	153	90	150	205	*518*
Age 21-24	209	130	196	347	*1238*
<1yr Tenure, Age 21-24	211	130	196	308	*520*
<1yr Tenure, Left School at 16, Age 21-24	177	108	173	268	*108*
1-2yr Tenure, Age 21-24	205	125	192	307	*270*

Source: LFS, June 1997-Feb 1998; >30 hours per week

This concludes our description of the New Deal target group. Before formally analysing the effects of experience on the productivity of the target group using our data we will first examine a methodology for evaluating the likely effects of the New Deal.
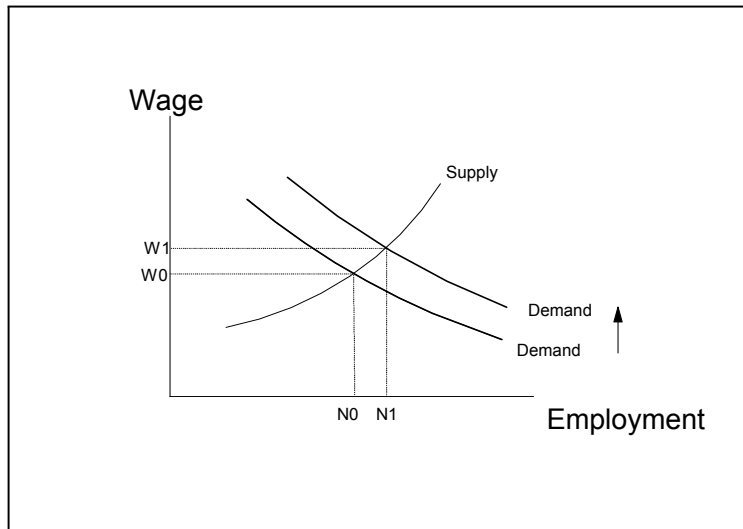
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Figure 2: Effect of subsidy on Employment in Youth Labour Market

4 Wage subsidies

There is a large literature on the effects of wage subsidies, but until relatively recently it was mostly of a static and partial equilibrium nature[9] Although highly stylised it is a good way of beginning the analysis. Consider a proportionate subsidy of s, on any group of individuals. The effect of the subsidy on wages and employment is illustrated in Figure 3.

Notice that in general both wages (W) and employment (N) rise by less than the value of the subsidy. Because employers will compete for the subsidised workers their wage will rise and this will engender a higher labour supply. This can be formalised by an upward shift of the labour demand curve. So, in the general case of a proportional subsidy of s with labour

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[9]The classic references are Kaldor (1936) and Pigou (1933) who were motivated by calls for wage subsidies during the Great Depression. A recent survey is contained in Katz (1996) or Hamermesh (1993).

demand elasticity η and labour supply elasticity ϵ the effects on wages and jobs are

$$\frac{d \ln N}{ds} = \frac{\eta \epsilon}{\eta + \epsilon} \text{ and } \frac{d \ln W}{ds} = \frac{\eta}{\eta + \epsilon} \text{ respectively.} \tag{1}$$

A special case of this is the 'world trade' model. Increases in employment do not depress marginal products as new firms enter the market to absorb the workers and there is an international market for the product (so output increases by a small country will not decrease price). Essentially in terms of Figure 1 the labour demand curve is flat. Under these conditions *all* the subsidy is passed on to workers in the form of higher wage and the employment effect depends only on the size of the subsidy and the elasticity of labour supply[10]

The main issues that have concerned empirical work using this framework (e.g. NERA, 1995) are the size of the substitution effect, deadweight and displacement costs. Substitution could occur between the New Dealers and other groups with similar characteristics such as the short-term unemployed, employed young workers and older less educated workers. Deadweight losses are incurred because some of the target group would have moved into employment without any intervention. Displacement occurs because firms using subsidised workers may steal market share from their unsubsidised counterparts. Estimates of these elements suggest that the number of net new jobs created by subsidisation *may* be quite small. Deadweight (and therefore Exchequer cost) may be particularly large for young individuals because their transition rates between unemployment and employment are particularly high. Our analysis of the LFS in the previous section suggested that about 33 per cent of the target group would have transited into employ-

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[10]as $\eta \to \infty$, $\frac{d \ln L}{ds} = \epsilon$; $\frac{d \ln W}{ds} = 1$. See Minford (1997) for an example of such a model.

ment even in the absence of the New Deal.

Most evaluations taking this partial equilibrium approach find that dead-weight and substitution are substantial. Many supporters of low wage subsidies argue that these are underestimates of the net employment effect. They emphasis that the substitution of the long term unemployed for short term unemployed will have beneficial general equilibrium effects in a labour market characterised by imperfect competition of various sorts (e.g. Layard, 1997; Snower, 1997; Richardson, 1997a,b). The argument is essentially that the long term unemployed are 'outsiders' in the labour market. They exert little downward pressure on nominal wages (which in these models is the economic function of unemployment). By making these 'outsiders' into 'insiders' the equilibrium rate of unemployment is lowered because effective labour supply is higher. Thus, so the argument goes, although substitution in the short term mitigates the employment generating effects of subsidies, in the long-run it is the mechanism by which the 'natural rate of unemployment' (or NAIRU) is reduced.

This argument is an important one, but needs some unpacking. It is very optimistic in the sense of suggesting that the program will actually raise GDP[11] by reducing a negative externality. The best evidence for this view is based on the empirical importance of duration dependence in unemployment rates. The longer an individual stays unemployed the less likely he is to leave unemployment (negative duration dependence). The most likely explanation for this is the deterioration of (broadly defined) human capital when unemployed. Even if one accepts this argument several notes of caution must be added. In the first place, most estimates of duration dependence

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[11] As we discuss below, this is an increase in GDP in the long run. It is not merely a short-term Keynesian style demand boost and it is net of any distortionary effects induced by raising the revenue to fund the scheme.

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only find important effects after about 1 year rather than six months (Van den Berg and van Ours, 1994). Furthermore, complete substitution of the young unemployed for older unemployed workers who would have got jobs will do nothing to reduce the NAIRU. The 18+ group with 6-24 months or more of unemployment are particularly vulnerable in this respect. Finally, controlling for observed and unobserved characteristics (such as skill, gender and race and worker quality) often reduces the effects of duration dependence to zero[12]. Indeed, in a comprehensive survey of the literature Machin and Manning (1998) conclude: "Our impression is that, overall, the results for Europe on duration dependence do not seem to suggest any marked negative duration dependence once one controls for a few readily observable characteristics" (p.32). They do point out, however, that most UK studies find evidence of duration dependence even after controlling for heterogeneity.

A dynamic model of employment should recognise that the unemployed pool will generally contain workers with lower productivity, some of whom have lost work skills through long-term unemployment others of whom will have had little skills even to begin with. The only way that a temporary subsidy can have a permanent effect on the employability of this group is to raise their productivity through their experience of work. The critical question with regards to the New Deal is the extent to which participants will genuinely have their productivity raised so that an employer will have an incentive to keep them on after the subsidy runs out.

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[12]There is also evidence from macro-economic wage equations suggesting that the proportion of long term unemployed exerts no downward pressure on earnings growth, although the proportion of short term unemployed does (e.g. Layard, Nickell and Jackman, 1991). There is some doubt over the econometric reliability of such models, however, as a more general distributed lag on unemployment levels often does as well as short-term unemployment terms (e.g. Nickell, 1987).

To formalise this notion in a companion paper we consider a dynamic model of the labour market (Blundell, Dias, Meghir and Van Reenen, 1998). In a simplified version of this model there are two periods, three sectors and two types of workers, type I (high productivity) and type II (low productivity). A worker increases his or her probability of acquiring a real increase in productivity with tenure (e.g. learning by doing). Variation in the individual's reservation wage generates lower probabilities of the less productive workers being in employment at any given time. After a period in employment type II workers look identical to skilled worker with probability, p.

Several issues arise from this simple model. First, it implies a long-run effect from short-run subsidy. By getting some low skill workers into jobs their productivity is raised and so their chances of moving out of unemployment is enhanced. Second, the model raises question of why the unemployed do not take jobs as an 'investment' because of long term gains. The answer is potentially because of credit constraints (like borrowing to finance training). Since these are more likely to effect the young than the old, liquidity constraints theories give a rationalisation of targeting the policy on the young at least in the first instance. Nevertheless, there is some doubt over the importance of credit constraints in the market for human capital. Cameron and Heckman (1998) for example, argue strongly against their importance in the U.S. Finally, the model focuses attention on the question of the returns to the first job for the young , for example the importance of training.

One empirical strategy is to close the model and calibrate it (e.g. Richardson, 1997; Orszag and Snower, 1997; Heckman, Lochner and Taber, 1998[13]). We take a more modest approach below in section 6. Notice however that the parameters necessary to do this require:

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[13]Only the latter paper really allows for unobserved heterogeneity and endogenous skill acquisition.

- pay-off to experience/training for the target group

- participation elasticities for the target group

- labour demand elasticities (especially substitution between young workers and other groups of different skills)

It is difficult to get accurate measures of these and a natural question to ask is whether an 'ex post' evaluation of this scheme (or others) would be more valuable in shedding light on the New Deal. We now turn explicitly to this evidence.

5 Ex Post Evaluation: A Differential Trend Adjusted Approach

In Annex I we present a survey of the evidence on the effects of different wage subsidy schemes. We focus on those which are most relevant to the New Deal. There are several important lessons from this brief survey. Firstly, it seems that employee-based subsidies seem to have a larger employment impact than employer-based subsidies, contrary to simple economic theory which argues for symmetry. This is also consistent with the evidence from the Work Trials in Britain. One rationalisation of this finding is that stigma effects may be very important as employers treat the holding of a voucher as a signal that the prospective employee is of low quality. It is often argued that these stigma effects can be ignored because (i) the New Deal covers a much wider group than is typical in the U.S. and (ii) long-term unemployment is a worse signal in the U.S. than in Europe because there are relatively few American long term unemployed. This might be too optimistic. Workers failing to get a job in the 'Gateway' may be perceived

to be 'the worst of the lot' and therefore become stigmatised (the education and training options have proven more popular amongst New Dealers than the subsidised employment route). Additionally, employers may believe that the unemployed with the best potential will self select themselves into the education and training category.

A crucial feature of any ex post evaluation is the need for a valid control group to compare with the treatment group. There are two key features needed in order to make a quasi-experiment useful. First, the composition of the group must be stable over course of experiment. Second, the groups must be subject to (and react in the same way to) common trends. Since the New Deal is a nationwide initiative there is no obvious control group[14]. One might think of using the unemployed in different age groups (e.g. 25-30 year olds) since their productivity is likely to be close to the New Dealers and consequently they may be expected to be subject to similar macro shocks. Unfortunately this group is likely to be most affected by substitution effects so fails the first criterion (stable composition). So why not use older groups instead? Older groups, however, have very different productive characteristics and may respond differently to shocks such as the state of the business cycle. Thus they fail on the second criterion (common trends). One possible solution is to examine how the older group have responded differentially to the state of the cycle compared to the younger group in similar periods of the business cycle to control for this effect. These considerations suggest that examining in detail the pre-programme behaviour of the New Deal target group and the various possible control groups.

The removal of common trends or time effects is worthy of further discussion. The assumption will be that these time effects, that affect both control

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[14]There are Pathfinder pilots in selected areas but since they began in January 1998 they had very little time to run before the New Deal began in April 1998.

and treatment groups, at the time of the reform are not a consequence of the reform itself. It is therefore validly to abstract from them as part of the quasi-experimental evaluation method. However, there are good reasons why this may not be the case. In particular, if the program induces global effects on wage pressure the there may be a consequent increase in overall demand for labour and employment. To the extent to which these are common to all the groups being considered, the quasi-experimental method will remove these effects along with all other common time effects. Thus it will *underestimate* the impact of the reform.

We have already argued that such global wage pressure effects - or general equilibrium effects - may be an important feature of this reform. As a consequence the quasi-experimental evaluation will only recover differential effects between treatment and comparison groups and will not capture these potentially important consequences of the reform. In the absence of these general equilibrium effects we will argue that the quasi-experimental method developed in this section provides an upper bound on the impact of the reform. This will not be the case in the presence of such general equilibrium effects. It will still be possible to assess whether the reform has had an impact, but the complete evaluation will require a model of the feedback effects on the whole economy as presented in section 4.

To formalise the quasi-experimental method we propose, suppose E_{it} represents the outcome variable of interest. The most obvious candidate is employment status, but we could also consider duration of unemployment or earnings for individual i in period t. Suppose we select individuals according to some common eligibility criteria: e.g. aged 18-24, 12 months after a period of 6 months unemployment. This set of individuals will also have observable characteristics X_{it} which will include such variables as age, gen-

der, education, prior work experience, etc. The objective of the evaluation described here is to find the effect of the New Deal on eligible participants. Suppose using the participant and nonparticipant data sources we measure E_{it} for a group who satisfy the eligibility criteria but who are observed before the program begins. Label this period $t = t^1$. We also have available data for eligibles (all 18-24 year olds unemployed for 6 months are eligible and participation is essentially compulsory). Label this period $t = t^2$. [15]

Write the relationship between the new deal and E as

$$E_{it} = \gamma_i D_{iN} + \beta' X_{it} + u_{it}$$

where D_{iN} is a new deal "dummy" that takes the value unity for those individuals who are eligible and have participated in the program. We then ask the question: when will the simple difference estimator

$$\widehat{\gamma}_D = \widetilde{E}_{t^2} - \widetilde{E}_{t^1} \tag{2}$$

where

$$\widetilde{E}_{t^j} = E_{it} - \widehat{\beta}' X_{it^j}$$

consistently estimate the average γ among those that enter the New Deal? The answer depends on the structure of the unobservables u_{it}.

In line with the standard approach in this area the unobservables are assumed to have two additive components, an individual effect uncorrelated across individuals that is possibly time varying ε_{it} and a *macro* effect that may differ in its impact across individuals $k_i m_t$:

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[15]In the present context t^1 is around April 1998 and t^2 around April 1999. We have to allow at least nine months after April 1998 for the Gateway (up to four months) and the subsidy period itself (six months). An extra three months is a minimum smoothing over the transitional period. Clearly there are likley to be longer term affects so individuals need to be followed through their working lives.

$$u_{it} = \varepsilon_{it} + k_i m_t.$$

For (2) to consistently estimate γ we require

$$E(u_{it}|t = s, X_{is}) = \mu. \tag{3}$$

That is that the unobservables need not have mean zero but must be constant over the before and after periods. This is equivalent to requiring that the group we choose before the New Deal has the same composition of unobservables as the actual New Deal participants and that the macro conditions remain the same. The first of these assumptions can be made more realistic by carefully choosing the period t^1 group according to the eligibility criteria and controlling for any remaining observable differences through X_{it}.[16] The second requirement is more difficult to meet and will clearly not be satisfied if there is any systematic change in the macro economy that affects the New Deal target group. This is the reason for choosing a comparison group and using 'difference in differences' techniques.

Suppose there is a control group, say an older cohort of mature men or women, that is considered less likely to be directly affected by the New Deal. Clearly, younger cohorts only a little older than the target group are not suitable due to the substitution effects discussed above. Suppose we label this comparison group C and label the target group T. If we can measure E_{it} and X_{it} for this comparison group a difference in differences estimator would have the form:

$$\widehat{\gamma}_{DID} = (\widetilde{E}_{t^2}^T - \widetilde{E}_{t^1}^T) - (\widetilde{E}_{t^2}^C - \widetilde{E}_{t^1}^C). \tag{4}$$

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[16]For example, that they are the same age, had a spell of at least 6 months unemployment, etc.

This would be a consistent estimator of γ if the unobservables satisfy

$$E(u_{it}|t = s, i \in g, X_{is}) = \varepsilon_g + km_t, \qquad (5)$$

where $i \in g$ indicates an individual in group C or T. Condition (5) generalises (3) by allowing a macro or general trend effect.[17] However, this macro effect is required to be the same across the target and comparison groups. Since the young and the old typically attract differential macro effects over a cycle (see below) this requirement is unlikely to be met. However, a further adjustment to correct for this is possible.

To allow each group to respond differentially to business cycle effects we write

$$E(u_{it}|t = s, i \in g, X_{is}) = \varepsilon_g + k_g m_t, \qquad (6)$$

where the k_g acknowledges the differential macro effect across the two groups. Now it can be seen that the diff-in-diff estimator $\widehat{\gamma}_{DID}$ in (4) consistently estimates

$$p\lim \widehat{\gamma}_{DID} = \gamma + (k_T - k_C)[m_{t^2} - m_{t^1}] \qquad (7)$$

which clearly only recovers the true effect of the program when $k_T = k_C$.

Now suppose we take another time interval t^0 to t^*, over which a similar macro trend has occurred. Precisely, we require a period for which the macro trend matches the term $(k_T - k_C)[m_{t^2} - m_{t^1}]$ in (7). For example, we could compare a period in the early 1990s. This can only be chosen when the macro environment facing the participants leaving the program in period t^2 is revealed. The differentially adjusted estimator

$$\widehat{\gamma}_{DADID} = \left\{ (\tilde{E}_{t^2}^T - \tilde{E}_{t^1}^T) - (\tilde{E}_{t^2}^C - \tilde{E}_{t^1}^C) \right\} - \left\{ (\tilde{E}_{t^*}^T - \tilde{E}_{t^0}^T) - (\tilde{E}_{t^*}^C - \tilde{E}_{t^0}^C) \right\} \qquad (8)$$

will now consistently estimate γ.

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[17]See Blundell, Duncan and Meghir (1998).

Which comparison period is chosen for this trend adjustment to difference in differences requires careful consideration. It is likely that the most recent cycle is the most appropriate, since earlier cycles may have systematically different effects across the target and comparison groups. However, using time series information from several business cycles will give greater credibility to this method. In fact the comparison over several points of different business cycles is likely to be a good specification test of the model.[18] In general if there exists sufficient time series history on each group then it may be possible to model secular movements in employment as well as cycle effects. These can then be removed from the difference in difference measure as in (8).

Finally, the analysis could be repeated for a number of alternative control groups. In principle we could get different answers from using different age groups. If these differences are large then it is indicative of a misspecification of the original model.

To investigate this approach further Figure 4 plots the employment-population ratios for men in two groups. Britain experienced a boom in the late 1980s, a recession in the early 1990s and a subsequent recovery after 1992. Although both younger and older men are affected by the cycle it is obvious that youth unemployment is much more cyclically sensitive. Failure to account for the differential trend over the cycle would severely bias our estimates of the effects of the New Deal.

The critical assumption in all this analysis is that the comparison group is unaffected by the New Deal. There are three reasons why this condition may fail. The first is the substitution effect which will tend to exaggerate the impact of the New Deal. As mentioned above, this is because similar groups

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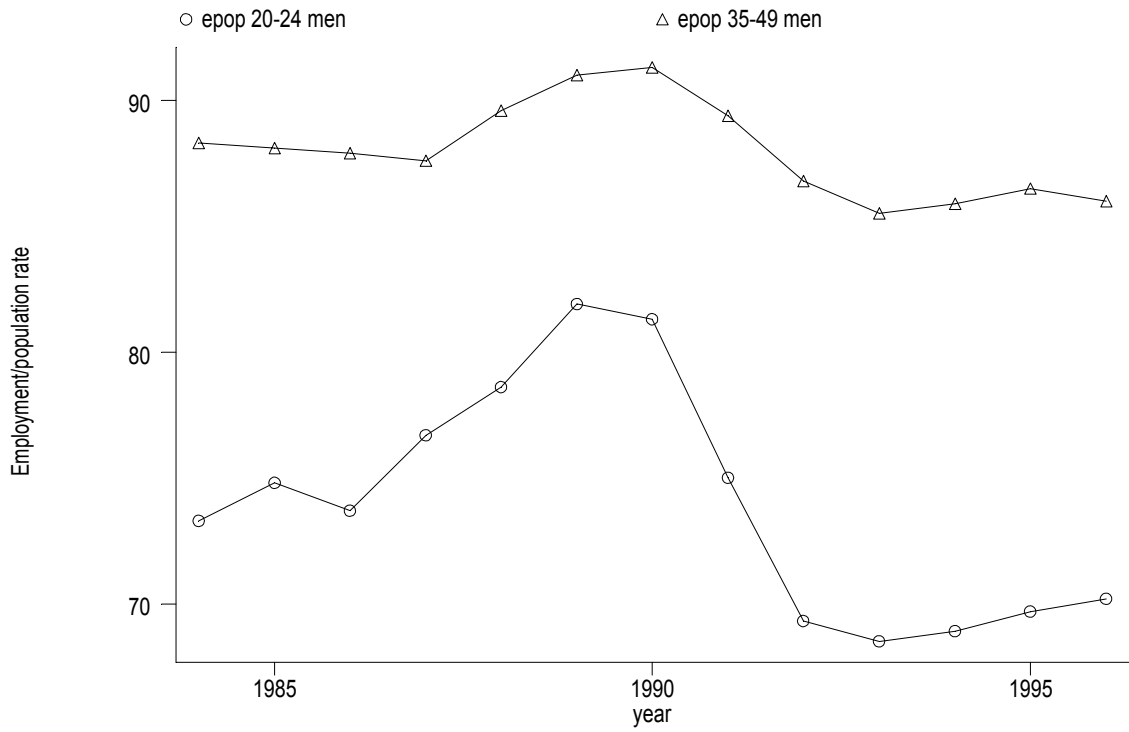[18]Analogous to an overidentification test.

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Figure 3: Male Employment-Population ratios for different age groups

to the target group will suffer by finding employment more difficult due to increased competition from New Dealers.

A second important issue is whether the New Deal induces compositional changes in the stock of individuals who reach the 6 month unemployment mark. For example, if the Gateway period is perceived as 'tougher' that the previous regime individuals may be more likely exit just before start of the Gateway. The U.S. evidence on time-limited unemployment benefits suggests that there is indeed such a spike in hazard rates (e.g. Katz and Meyer, 1990; Meyer, 1990, 1995). On the other hand, it may be that the promise of job help, subsidised work and cheaper training encourages more short-term unemployed to stay on for longer durations. In either case the composition of the young long term unemployed will be different pre and post New Deal. How large these effects are can only be gauged by examining the changing transitions rates for the short term unemployed (as well as the long term unemployed) pre- and post- program[19].

Finally, we can return to the discussion of likely importance of general equilibrium issues for the New Deal evaluation. Proponents of the New Deal argue that there will be a beneficial effect on aggregate unemployment. This can work through the standard wage pressure arguments discussed in Section 4 whereby the New Deal participants act more like short term unemployed and place more downward pressure on wages.

What can we say to this critique? First, notice that these general equilibrium arguments imply that there must be a greater effect on New Dealers than others so the size of the coefficient $\widehat{\gamma}_{DADID}$ is still a valid indication that there is a positive impact from the program. Second we have some reason to doubt how large these effects are likely to be - it seems highly unlikely

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[19]Early qualititive results suggest that the New Deal is viewed more more positively by young people than the previous job search regime (IES, 1999).

that older skilled workers, for example will be much affected by the New Deal Group. Thirdly, it may be that the distortions induced by the New Deal actually create greater deadweight losses than expected so the general equilibrium effects are not so beneficial. Finally, and most importantly, the discussion illustrates the need to supplement our ex post analysis with an ex ante model of the sort discussed in Section 4. In the next section we explicitly address whether the New Deal is likely to have a large productivity effects on the young unemployed.

6 Empirical Analysis of the effects of experience on productivity for New Dealers

The most important parameter in the ex ante model is the effect of a spell of employment on worker productivity. To keep their job after the subsidy runs out, it seems likely that the workers will have to have improved their productivity to the point at which the employer is prepared to continue to pay a wage that is higher than the worker's reservation wage[20]. Measuring individual productivity directly is difficult, so as is standard we resort to using employee gross wages as an index of productivity.

The evidence from Table 4 regarding 1-2 year tenure workers suggests that there is a return to experience of about 8.8% for men in their early twenties ((£246-£226)/£226). Table 6 attempts to examine this slightly more systematically for the particular group affected by the New Deal: those with some recent experience of unemployment. We selected all employees aged between 18 and 24 who had reported that they were unemployed in any

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[20]The government is requiring employers to undertake 'moral contracts' that oblige firms not to lay off New Dealers when their subsidy runs out. These have no standing in law so it is doubtful how effective they will be in practice.

one of the four previous quarters. Until mid 1997 LFS only asked the wage question in the final fifth wave before an individual leaves the panel. A series of 'job duration dummy variables' were defined as follows:

$D_0 = 1$ if unemployed $t - 1$ and employed in t

$D_1 = 1$ if unemployed $t - 2$ and employed in $t - 1$ and t

$D_2 = 1$ if unemployed $t - 3$ and employed in $t - 2, t - 1$ and t

$D_3 = 1$ if unemployed $t - 4$ and employed in $t - 3, t - 2, t - 1$ and t

Since each t is one quarter of the LFS, on average we are picking up information on the earnings distribution after one and half months of a job (D_0), four and a half months (D_1), seven and a half months (D_2) and ten and a half months (D_3). The 'effect' of a six month spell of job duration is then the difference between D_2 and D_0. The raw differentials in columns (1) is very large, particularly for the lowest decile (column (2)). Some of this is simple due to hours: column (3) shows that wage growth is about 9% in the first 6 months.

Earnings Growth for 18-24 year olds with an unemployment spell

Duration of employment	(1) Wkly Earnings Mean	(2) Wkly Earnings lowest decile	(3) Hrly Wage Mean	(4) Hourly Wage truncated mean
D_0 $(0-3$ months)	145.05 (400)	51.85 (400)	4.40 (398)	5.23 (267)
D_1 $(3-6$ months)	142.11 (356)	54.96(356)	4.21 (356)	4.79 (267)
D_2 $(6-9$ months)	154.51 (278)	68.44(278)	4.44 (277)	4.54 (268)
D_3 $(9-12$ months)	164.83 (266)	67.41(266)	4.82 (266)	4.82(266)
$(D_3 - D_0)/D_0$	6.5%	30%	9.1%	-13.2%
$(D_3 - D_0)/D_0$	13.6%	30%	9.5%	-7.8%

Notes: Number of observations in parantheses; all workers full time (at least 30 hours a week); 1998 Q1 prices; sample is all 18-24 year olds with at who were unemployed in at least one of the 5 QLFS waves 1992-1997

There are strong reasons to think that we may be overestimating the size of the experience related wage gains. The sample size declines as we examine the longer duration dummies and this is probably because the least productive workers are unable to keep their jobs for long periods. Thus the 'duration effect' is a actually mixture of a causal effect of experience on wages and a spurious effect arising from a failure to properly control for individual productivity. Since it is probably an 'upper bound' we attempt two ways of controlling for productivity. First we do this is a non-parametric way by estimating the number of workers who drop out each period of employment and delete this proportion from the lower end of earnings distribution. This assumes that failing to keep their jobs are all drawn from the lower tail of the wage distribution (the lowest productivity workers). This is unlikely to be completely true so we are actually estimating a lower bound to the tenure effect. The re-calculations of the means of this truncated distribution is given in column (4) of Table 5. The lower bound at the mean is actually negative. Negative returns are not credible, and we interpret the result to suggest that the upward biases from selectivity may be large, but are difficult to estimate non-parametrically with any precision.

A second way of dealing with different productivity effects is to control for observable characteristics of individuals. This allows us to systematically test the significance of the association of earnings growth with job duration. To do this we run a log(hourly wage) equation of the following form :

$$LnWage_i = \alpha_0 + \alpha_1 D_{1i} + \alpha_2 D_{2i} + \alpha_3 D_{3i} + \beta' X_i + \varepsilon_i$$

where the observables ($X_i's$) in the model include quarter dummies, age, gender, years of schooling, log hours[21]. All unobservables (ε) are assumed to

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[21] We also experimented with many other variables and interactions which were insignif-

be uncorrelated with the control variables. To give some idea of the effects Table 6 reports the estimated a coefficients for a model with no X variables in column (1). As seen in Table 5 there is no duration effects until 6-9 months and the big increase comes in the 9-12 month period. The second column conditions on the observables. As expected there are significant and positive effects of age, being male, and having more education. More interestingly, the duration effects are reduced from column (1) although the longest duration remains significant.

In the LFS it is possible to distinguish the length of the spell of unemployment as individuals are asked directly the question. We use this information to distinguish which individuals in the sample were $U > 0$ (unemployed for 6 months or more). D_0 can be identified for those with long unemployment spells. Interacting the length of the prior unemployment spell with employment duration through up an interesting pattern. At face value the coefficients suggest that the long-term unemployed begin employment at a wage about 6% lower than the short-term unemployed. Their wage growth 6-12 months after gaining employment appears faster than those who were unemployed for shorter periods. By the end of the first year of employment their wages are identical to those who got a job after a short unemployment spell[22]. Although the standard errors are large around these estimates the qualitative pattern holds if we go to a more parsimonious model in column (4)[23].

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icant at conventional levels. These included schooling-female interaction, age-female interaction, training dummy, female-hours interaction. Dropping the potentially endogenous hours variable also makes little difference to teh results.

[22]Gregory and Jukes (1997) use the matched NES/JUVOS data to examine the effects of unemployment on wages. They find that the post-employment wages of the unemployed young (21-24) catch up with the continuously employed young after a year's employment. This is not true for older workers.

[23]Compared to the previous column we have dropped $D1$, $D2$, $(U > 6 * D3)$ and

$U > 6 * D4$. Also the interactions between $U > 6$ and $D0$ and $D1$ are constrained to be the same. These restrictions are not statistically rejected by the data (F-test = 0.304)

Table 6: ln(Hourly Wage) Regressions for New Deal Group

	(1)	(2)	(3)	(4)
D_1	-0.002	0.002	0.017	-
	0.034	*0.032*	*0.039*	
D_2	0.043	0.035	0.028	-
	0.036	*0.035*	*0.041*	
D_3	0.117	0.095	0.070	0.067
	0.037	*0.036*	*0.044*	*0.033*
$(U > 6) * D_0$	-		-0.062	-
			0.048	
$(U > 6) * D_1$	-		-0.103	-
			0.048	
$(U > 6) * D_2$	-		-0.043	-
			0.059	
$(U > 6) * D_3$	-		0.017	-
			0.057	
$(U > 6) * (D_0, D_1)$	-			-0.084
				0.031
Years of Schooling	-	0.026	0.025	0.025
		0.006	*0.006*	*0.006*
Female	-	-0.060	-0.063	-0.062
		0.025	*0.026*	*0.026*
Age	-	0.056	0.056	0.056
		0.007	*0.007*	*0.007*
Quarterly dummies (18) ?	No	Yes	Yes	Yes
R^2	0.013	0.126	0.132	0.131
N	1012	1012	1012	1012

Notes:- Robust standard errors in parantheses; individuals aged 18-24 between 1992Q4 and 1997Q4 (ends Feb 1998) who had at least one unemployment spell; at least 30 hours worked a week; estimation by OLS. $U > 6$ indicates if the unemplyment spell was of at least 6 months duration

From this analysis we might infer that, on *extremely* generous assumptions, the New Deal target group could have 15% growth in their productivity if they remain in the job for a year. Since in section 3.3 we estimated that the rate of subsidy given to a typical New Dealer was 40-50% of her wage it seems likely that New Dealers whose productivity is well below the wage will not be able to keep their jobs when their employment subsidy runs out. Furthermore, notice from our estimates that after six months (when the subsidy runs out) wage growth is still small - in fact, insignificantly different from zero. Existing empirical work which examines experience effects over longer time periods than we consider here also suggests that experience returns are low for less educated workers (who form the majority of New Dealers as we showed in Section 3)[24]. For such low productivity workers, unless there are ways to keep the young workers in work for much longer than six months, it is unlikely that the New Deal can have large effects from productivity growth alone[25]. The main impact of the reform will be on workers whose productivity levels are only slightly below the offered wage and for whom the firm is extracting a reasonable "rent" over the period of the subsidy. For these the productivity growth may be sufficient for them to remain in employment once the subsidy is removed. How many of these workers are likely to be in the eligible pool of unemployed after the Gateway period is difficult to tell, but it maybe quite small.

To complete the model we need to assess the impact of the element of compulsion in the New Deal on participation in work. If welfare recipients

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[24]For example, Gosling, Meghir and Machin (1998).

[25]We also experimented with examining the effects of training. There were positive interactions between training and job duration but again, they were very imprecisely determined. Blundell, Dearden and Meghir (1996) find evidence of strong effects of employer provided training on wages. However, this was for workers with a good deal more education and work experienece than is likely for the participants in the New Deal.

are faced with a new regime of severe cuts in their benefits if they refuse a New Deal position[26], then this will increase the incentives to find work. In this case effective labour supply is increased not simply through the increase in human capital, as we have been assuming, but in addition by reducing the reservation wage. In practice, the 'stick' of sanctions have not been heavily used[27].

7 Conclusion

In this paper we have examined the likely effects of the UK Government's New Deal for unemployed youth. The initiative is a major program and is being closely watched by other European countries as a possible way out of the problem of wide scale joblessness. The target group were identified as having characteristics associated with low productivity and we argued that a key rationale of the scheme is to enhance their employability by making them more productive. Potentially, productivity could increase through the experience/tenure effect and training opportunities associated with having a job. This dynamic effect could make a temporary subsidy have a permanent effect on unemployment.

We considered two evaluation methodologies. We proposed an ex post evaluation based around a 'trend adjusted difference in differences' approach which could potentially deal with many of the econometric problems associated with evaluation. We emphasised, however, that this must be complemented with an ex ante model based approach. Suggesting a simple model we

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[26] There is a view that there are already severe penalties in place. The young unemployed have been under substantial pressure to actively job search with the threat of benefit withdrawal since the early 1980s.

[27] The Chancellor announced moves to toughen sanctions in his 1999 Budget (HM Treasury 1999).

tried to estimate a key parameter: the effect of job duration on productivity (as measured by the wage) for the target group using micro data from the LFS. Our conclusion was that the productivity effects are relatively modest compared to the size of subsidy deemed necessary to get the group into jobs. Thus it is likely that the effects of the policy will be far more modest than its proponents have hoped for. Although this conclusion must await more detailed analysis and the closing of the model we believe it is a useful first attempt to tackle the evaluation problem.

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8   ANNEX I: A Survey of wage subsidy schemes

The empirical evidence can be neatly divided into two sections. First we examine experience with subsidies to employers that take on a particular group of workers. These generally take the form of a tax credit that firms can offset against their tax liabilities or a direct payment to the firm from the government. More general employment subsidies and marginal subsidies are outside of the scope of this paper since welfare-to-work schemes are clearly targeted toward particular groups. Second, we look at schemes that provide subsidies to particular workers to take employment. These tend to be offered as tax credits on an individuals income tax liability (e.g. Earned Income Tax Credit in the US) or as direct payments through the social security system (e.g. Family Credit in the UK). There are now several surveys of such subsidy

schemes (see NERA, 1994, or Gardiner, 1997 for example). In this section we focus on those policy experiments most relevant for the U.K experience.

8.1 Targeted Subsidies to Employers

8.1.1 The US Targeted Jobs Tax Credit

The Targeted Jobs Tax Credit (TJTC) was introduced in the US in 1978 and remained in effect until 1994. It offers a tax credit to employers who hire certified target group individuals. The target groups eligible for the credit have varied over time but have included economically disadvantaged youths, public assistance and SSI claimants and certain ex-convicts. Eligible individuals were issued with vouchers which they gave to employers who could then apply for the tax credit. As of late 1986, the scheme provided a tax credit amounting to 40% for the first $6000 of wages for 12 months.

An analysis of the employment implications of the TJTC has been conducted by Bishop and Montgomery (1993). Using a sample of about 3,500 firms, they examine whether the growth in employment in an establishment is related to the growth in the proportion of TJTC subsidised workers in the firm. Their results suggest that for every ten subsidised hirings in the firm, approximately three new jobs are created. Of course this implies that seven out of ten TJTC payments are for workers in jobs that would have existed without the subsidy, implying a large deadweight loss. However, Bishop and Montgomery do find evidence to suggest that the subsidy encourages employers to hire those eligible for the scheme, possibly at the expense of non-eligible workers.

A significant problem with much of this evidence is that it is difficult to isolate the true effect of the tax credit on employment from the fact that firms that are expanding employment may be more aware of the availability of the tax credit. Katz (1996) argues that changes in the eligibility criteria of the TJTC provide exogenous sources of variation that helps to identify the impact of the tax credit on the labour market outcomes of the target group. In 1989, 23-24 year olds were made ineligible for the program while those aged 18-22 maintained their eligibility. Katz analyses the employment rates of disadvantaged 23-24 year olds before and after the eligibility change.

Comparing this change with the change in employment rates of non- disadvantaged 23-24 year olds provides a differences-in-differences estimate of the impact of TJTC eligibility on the employment rate of the target group. This produces an estimate of -0.030 (s.e. 0.017), indicating a 3 percentage point decline in the employment of disadvantaged to non-disadvantaged 23-24 year olds after elimination of their eligibility for TJTC. To control for the possibility that other labour market factors have differential effects on disadvantaged and non-disadvantaged workers, Katz examines the employment rates of disadvantaged and non-disadvantaged workers for those who were unaffected by the legislative change. He finds that over the time period considered, the employment rate of the disadvantaged actually rose relative to the non-disadvantaged by 0.013. Hence the difference-in-difference estimate underestimates the effect of removing TJTC eligibility. When this is corrected for, Katz estimates that the impact on disadvantaged 23-24 year olds was to reduce their employment rate by 4.3 percentage points. Controlling for observables using a regression model reduces this effect to 3.4 (s.e. 1.9).

8.1.2 Australia: The Job Compact

Finn (1997) reports on the experience of the Australian government in offering Jobstart subsidies to the long-term unemployed (18 month+ unemployed). The evidence suggests that employers were not keen on taking on the LTU even with subsidies as there were concerns about "the low-skill levels, poor attitudes to work and low levels of motivation" of the LTU. Furthermore, the Australian government concluded that it was unclear whether LTU individuals who were recycled into short-term unemployment were really competitive job seekers.

8.1.3 The UK Workstart Pilots

In the 1993 Budget, the Chancellor announced the introduction of a pilot scheme to provide subsidies to those who had been continuously unemployed for more than 2 years. The subsidy provided employers with £60 per week for 26 weeks and then £30 per week for 26 weeks. The pilots were run in Tyneside, Devon and Cornwall, East Kent and South West London.

The results of a survey of participating employers was reported by Atkinson and Meager (1994). They sampled 399 firms distributed across the four pilot regions. They find that participating employers tended to be small, private firms in the service sector and tended to be offering low-skill employment. Many of the employers claimed that they traditionally recruited from the long-term unemployed which suggests that there is a certain amount of deadweight loss associated with the subsidy. Three quarters of respondents believed that the subsidy had some influence on their staffing levels, though only 42% thought it very important. Furthermore, a smaller percentage thought that the subsidy influenced them toward employing the long-term unemployed, even though these were the only workers eligible for the subsidy. Most employers reported that those employed with the subsidy were of an adequate standard and were on average as productive as other new recruits. Atkinson and Meager suggest that only 17% of the workstart vacancies represented new employment that would not have existed without the subsidy. They also conclude that substitution was significant with much of the employment of the long-term unemployed occurring at the expense of the shorter term unemployed.

8.2 Tax Credits to Workers

8.2.1 The US Earned Income Tax Credit

The Earned Income Tax Credit (EITC) is given to individuals whose taxable income is below a defined threshold (about 28,000 in 1996)and who have a dependent child. The amount of the credit depends upon the individuals earned income and number of children. For example, for a family of two or more children in 1996, the credit is phased in at a 40% rate on the first $8890 of income, giving a maximum credit of $3556. In the income range $8890 to $11610 the credit remains at this maximum level. The credit is then phased out on income above $11610 at a 21% rate so that individuals are no longer eligible for the credit when income reaches the defined threshold. Figure 2 shows how the EITC affects the budget constraint. Compared to the no-EITC budget constraint given by AE, EITC produced kinks in the constraint. Between A and B, the value of the tax credit rises as hours
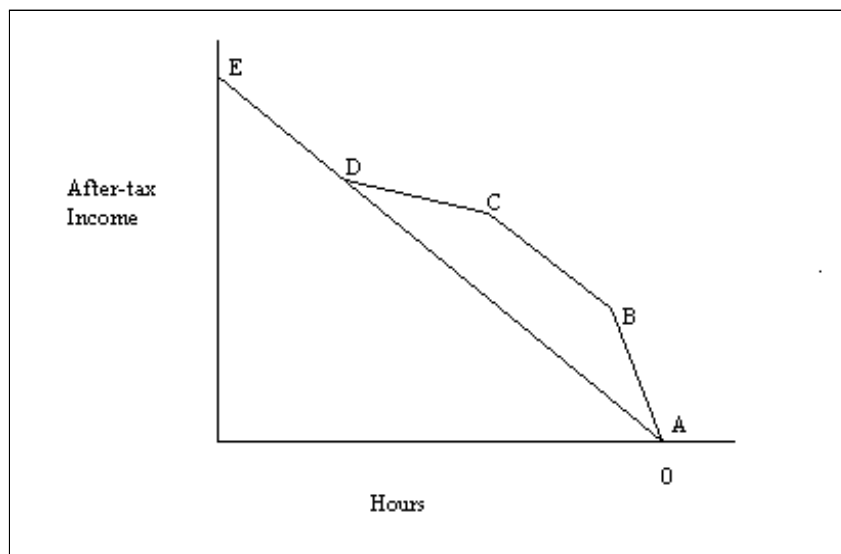
Figure 4: The Impact of EITC on After-Tax Earnings

increase up to the maximum credit of $3556. Between B and C the credit remains at the maximum level and the budget constraint is parallel to the non-EITC constraint. Over the section C to D, the credit is slowly withdrawn until it is zero at point D. It is clear that any eligible taxpayer who was working prior to EITC would still prefer to work (though possibly for fewer hours) and that some individuals may be induced to work because of the credit. Hence the impact of EITC on participation of eligible taxpayers is unambiguous.

Eissa and Liebman (1996) attempt to estimate the labour supply response to the EITC. They examine the response of single women with children to an expansion of the EITC in 1987. The 1987 expansion raised the phase-in rate of the credit and increased the maximum income to which the subsidy rate was applied. This had the effect of creating a set of individuals who became eligible for the first time. At all levels of earnings the EITC amount after the expansion was at least as large as it was before. They suggest comparing the participation rates of single women with children before and after the tax change to assess the impact of EITC. However since other factors

were likely to have changed at the same time e.g. the state of the business cycle, it is necessary to have a control group. Eissa and Liebman suggest using single women without children as the control group since they are not eligible for EITC but otherwise may react in a similar way to the treatment group to other general economic shocks. Hence comparing the change in participation rates for the treatment group relative to the change experienced by the control group gives a difference-in-differences estimate of the effect of EITC. Since the identification strategy relies upon a timing effect it is crucial that the treatment group response to other shocks is the same as the response of the control group and that there are no other contemporaneous shocks that affect the treatment group but not the control group.

Eissa and Liebman find that the participation rate of the treatment group increased by 2.4 percentage points (72.9% to 75.3%) at the time of the tax change. In contrast there was no change in the participation rate of the control group (95.2%). Hence the difference-in-differences estimate suggests that EITC increased participation by 2.4 percentage points, with a standard error of 0.6. Of course the treatment and control group do not have the same distribution of observable characteristics and this may distort inference. When observable characteristics are controlled for, the estimated participation response falls to 1.9 percentage points but is still statistically significant. While these results suggest significant incentive effects from the EITC on participation, two points should be borne in mind. First, it is arguable as to whether the identification assumptions are reasonable. The participation rate of single women with children may well differ in it's cyclical sensitivity to that for single women without children. In this case the identification assumption is invalid and the claimed causal effect cannot be sustained. Second the authors find no evidence of an hours effect from the EITC change even though the standard model would predict that EITC recipients who are already working should have reduced their hours of work. The absence of this effect again casts doubt on the identification strategy.

Dickert, Houser and Scholz (1995) also find positive effects from EITC expansion on labour force participation. They estimate a joint model of labour force participation and welfare recipients that depends, upon other things, on net predicted wages. The impact of changes in EITC can then be simulated

by estimating the change in net wages produced by the EITC change. They estimate that effect of an expansion of EITC that occurred in 1993. This dramatically increased the phase-in credit rate so that an individual with 2 or more children experienced a rise in the rate from 19.5% in 1993 to 30.0% in 1994. The maximum possible credit for the same individual rose from $1511 to $2528. Their simulations suggest that this change increased the labour force participation of single parents by 3.3 percentage points as a result of increasing the net wage of such individuals by 15%. They also suggest that the combined effect of individuals leaving welfare programs and using EITC generates savings of over $2bn, since mean EITC payments are significantly smaller than welfare program payments.

8.2.2   The Canadian Self-Sufficiency Project

A major welfare-to-work experiment is currently being undertaken by the Canadian government. The Self-Sufficiency Project (SSP) is an earnings supplement program for single parents who have been on welfare for at least 12 of the last 13 months. The scheme is very generous, providing a supplement equal to one-half of the difference between a participant's gross labour earnings and a target earnings level. The target earnings level was set above average earnings for full-time females in the two provinces in which the experiment was conducted.[28] So, for example, the target earnings level in British Columbia was set at $37,000. An SSP participant who worked 30 hours per week (1,500 hours per year) at $7 per hour would earn $10,500 per year and would collect a $13,250 supplement. Unearned income and the earnings of other family members do not affect the supplement payment. Finally the scheme imposes a full-time work requirement (30 hours per week).Those who participate in the scheme are eligible for SSP payments for up to 36 months.

The scheme was implemented as a randomised experiment and has been analysed by Card and Robbins (1996). A random sample of welfare recipients who met the eligibility criteria in British Columbia and New Brunswick were selected and were then randomly assigned into either the control group (1,056 individuals) or the program group (1,066 individuals). The individ-

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[28]Note that while SSP is aimed at single parents regardless of sex, 95% of those eligible were women.

uals provided retrospective information on labour market experience at the baseline survey and a second survey was conducted 18 months after the date of random assignment. There was a clear trend toward both higher earnings and employment rates for both the control and program group. However the trend is far more pronounced for program participants. For example, program participants have significantly higher monthly earnings within 5 months of the baseline and are earning roughly $100 per month more than the control group 18 months into the program. Similarly positive effects are realised for the employment rate. Card and Robbins also show that the program group are significantly more likely to have ceased welfare recipiency and to receive smaller welfare payments.

Although all the results discussed do suggest that the SSP is having significant success on moving individuals off welfare and into work, there are two important provisos. First, those in the program group receive SSP supplements for 36 months from the baseline. At present we do not know what the long-run effect of the program will be when these payments cease. It is unfortunately possible that the program recipients will return to welfare upon the expiry of the program and there may be no long-run difference between the program and control group. Second, when welfare and SSP payments are combined, it is found that the program group costs about $100 per month more in government transfers than the control group. Against this we should note that the program group have average gross incomes that are about $230 more than the control group as a result of both higher earnings and welfare payments so that the scheme is having substantial anti-poverty effects.

8.3   Comparing employer and employee subsidies

To conclude this Annex we also consider some papers which set up experiments comparing employer versus employee subsidies. Woodbury and Spiegelman (1987) discuss an interesting experiment in Illinois. New claimants were randomly assigned into three groups of about 5,000 each. One group was offered a voucher worth $500 that they would receive if they found a job within 11 weeks. For the second group the $500 voucher could be re-

deemed by the employer. The third group was the control group. Woodbury and Spiegelman found small but significant effects from the employee-based subsidy, but *no* significant effects from the employer based subsidy. The participation of employees and employers in the latter scheme was particularly low (only about 3 percent of the sample claimed employer bonuses). This could be due to administration costs of red tape, but it could also be because of stigma effects - skilled workers were much less likely to refuse to participate in the scheme. Dubin and Rivers (1993) argue that one accounts for self-selection wage subsidies can have a more substantial effect. Burtless (1985) describes an experiment in Dayton, Ohio in 1980-81 where some welfare recipients were given vouchers which employers could redeem and another randomly assigned group were given nothing. The treatment group actually fared worse than the control. Holonbeck and Willke (1991) report a similarly negative result from a Wisconsin randomised experiment for particularly disadvantaged groups. Again, because these groups were particularly disadvantaged it is likely that stigma effects are important in explaining the findings.