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Can we estimate the impact of the Choices package in Pathways to Work?

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Summary

The Pathways to Work programme is an important policy innovation in Britain. It provides greater support, obligations and incentives for claimants of incapacity benefits with the goal of encouraging employment. The programme has various components, including a 'Choices' package. 'Choices' is the collective name for a variety of voluntary schemes intended to improve labour market readiness and opportunities. Previous quantitative research has focused on the overall impact of Pathways to Work while this study was designed to look at the impact of the Choices component.

Estimating the impact of Choices is particularly difficult for two reasons: First, administrative data and surveys of benefit claimants give conflicting accounts of which individuals participated in Choices and, if they did, in which particular Choices scheme they participated. We are, therefore, left with two different accounts of whether or not an individual participated in each particular element of Choices, making it difficult to compare the outcomes of Choices participants with those of non-participants.

Second, and more fundamentally, participation in Choices is voluntary, so it is difficult to know how far different outcomes for participants and non-participants are caused by Choices and how far they reflect pre-existing differences in the type of people who choose to participate. Using propensity score matching techniques, we control for differences between participants and non-participants in a very large set of background characteristics; we thus compare outcomes for Choices participants with those for non-Choices participants who are observably similar in many dimensions. But it remains likely that there are important differences in the unobserved characteristics of the two groups, and it is impossible to know how far the difference in outcomes between the two groups is a result of these unobserved pre-existing differences rather than a result of participating in Choices.

For example, those who are more motivated and more ready to move into employment might be more likely to choose to participate in the voluntary programmes available as part of the Choices package, but they would also be more likely to move into paid work even without participating in Choices. If that is the case, the estimated differences in outcomes between participants and

non-participants would be overestimates of the true impact of the programme. Alternatively, individuals with worse health conditions might be more likely to volunteer for programmes aimed at improving their ability to manage their health problem, but might also be less likely to move into paid work even without participating in Choices. If this were the case then the estimated differences in outcomes between participants and non-participants would be underestimates of the true impact of the programme (and possibly suggest, incorrectly, that the programme had a negative impact on employment outcomes). Another possibility is that individuals assigned personal advisers who more strongly encourage people to enrol in Choices programmes might be either more or less likely to help them move into work in other ways.

Unless one is prepared to make the strong assumption that these unobserved characteristics do not explain both the outcome and self-selection into the programme, it is impossible to provide reliable causal estimates of the impact of the Choices programme. Hence, this study only presents a descriptive analysis of the difference in outcomes between individuals who chose to participate in these programmes and observably similar individuals who did not.

This paper stresses the intrinsic difficulty of evaluating programmes based on voluntary participation when there is no exogenous variation in the availability of the programme. By exogenous we mean no variation in programme participation that is not correlated with other characteristics not taken into account that are also associated with the outcomes of interest. Controlling for a rich set of observed characteristics is unlikely to overcome the fact that participants and non-participants to such programmes might be different for inherently unobserved characteristics, and these unobserved differences might be associated with better or worse subsequent outcomes. Exogenous variations can be used in various evaluation designs, like random eligibility thresholds, piloting based on geographical areas or even more robustly, randomisation at the individual level.

1   Introduction

1.1   The policy background

The Pathways to Work programme ('Pathways', for short) is aimed at encouraging employment among people claiming incapacity benefits; that is, people claiming Employment and Support Allowance (ESA) or its predecessors Incapacity Benefit (IB) and Income Support (IS) on grounds of disability.

Pathways was introduced as a response to the large increase in the numbers claiming incapacity benefits. At the time of the 2002 Department for Work and Pensions (DWP) Green Paper *Pathways to work: Helping people into employment*, there were roughly 2.7 million claimants: more than the combined total number of unemployed people claiming Jobseeker's Allowance (JSA) and lone parents claiming IS. The overwhelming majority of people starting an incapacity benefits claim say they expect to work again (Woodward *et al.*, 2003). Many do – in 2004, almost 60 per cent left benefit within a year. However, for those who remain on benefit beyond this point, the chances of leaving declines markedly – 29 per cent will still be claiming after another eight years (DWP, 2002). A key aim of Pathways is to intervene early so as to reduce the incidence of prolonged benefit dependency.

Pathways was introduced on a pilot basis in three Jobcentre Plus districts in October 2003, and in a further four districts in April 2004 (these will be referred to as the seven 'original areas'). Since then the programme has been extended to the entire country and modified in a number of substantive ways.[1] At its introduction, Pathways was implemented only by Jobcentre Plus (hence, it was called 'Jobcentre Plus Pathways'). In December 2007 some areas of the country have started to introduce Provider-led Pathways (PL Pathways), while in October 2008, ESA replaced IB and IS on grounds of disability for new claimants. Mandatory participation for existing claimants

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[1]   Appendix A presents the timetable of the extensions and changes that occurred to the programme. They include notably the introduction of PL Pathways alongside Jobcentre Plus Pathways as well as the change from IB and IS on grounds of disability, to ESA.

is set to become compulsory nationwide for all existing claimants aged under 25 from April 2009 and for older individuals thereafter (see paragraph 3.22 of DWP, 2008).

The programme analysed in this report corresponds to the Jobcentre Plus Pathways in place in 2004 and 2005, before many of the major changes to the programme. The institutional details we describe below are those in place at that time.

1.2 The Pathways programme at the time of the analysis

Under Jobcentre Plus Pathways, an individual aged between 18 and 60 making a claim for incapacity benefits must attend an initial Work Focused Interview (WFI) eight weeks after making their claim. WFIs are carried out by specially trained IB Personal Advisers (IBPAs). Failure to comply with this requirement can result in benefits sanctions, although these have been rare in practice. Most people remaining on incapacity benefits must attend five further WFIs at approximately monthly intervals. In non-Pathways areas, in contrast, only the initial WFI is required. There are two groups of people for whom the five additional WFIs are not required: those with particularly severe medical conditions and those judged likely to return to work without additional help. However, they could still participate on a voluntary basis.

Those exempted on the basis of the severity of their medical condition are identified through the Personal Capability Assessment (PCA). Under Pathways, the aim is to fast-track this process to take place within 12 weeks of making the initial claim so the results are available by the time of the second WFI. Those with the most extreme illness or disability are exempted from the PCA process itself in addition to the WFIs. Those exempted from further participation on the grounds that they are likely to return to work without the need for any assistance are identified during the first WFI using a 'screening tool'. This consists of a questionnaire, the answers to which are used to rate the probability of an unassisted return to work within 12 months.

Participation in all other provision available under Pathways is voluntary. There are several elements:

• The '**Choices'** package – the focus of this report – offers a range of new and existing programme provision aimed at improving labour market readiness and opportunities. It is described in more detail below.

• The **Return to Work Credit** (RTWC) offers Pathways participants who find work of at least 16 hours a week a payment of £40 per week for a year if their gross annual earnings are below £15,000.

- **In-Work Support** (IWS) is a programme of provision to complement the support provided by IBPAs and New Deal for Disabled People Job Brokers. It is contracted out to providers and includes one or more of the following: mentoring, a job coach, occupational health support, in-depth support, financial advice/debt counselling and an after-care service.

- The **Advisers Discretionary Fund** (ADF) allows IBPAs to make awards of up to £300 until May 2005, and £100 thereafter, per person within a 12-month period, to support activities or purchases to increase the chances of finding work.

1.3   The Choices package

Evaluating the Choices package is particularly interesting as it could allow an assessment of the different impact of Pathways components. Within Pathways, Choices has the greatest gross financial cost per participant (Adam *et al.* 2008) and thus, raises the question of its own cost-effectiveness (and might indeed have an impact on the cost-effectiveness of the overall Pathways package).

The Choices package consists of a number of programmes that existed prior to Pathways and one new one. The two main programmes within Choices are the (pre-existing) **New Deal for Disabled People (NDDP)** and the (new) **Condition Management Programme** (CMP).[2] A number of smaller pre-existing schemes are also available. These include: Work-Based Learning for Adults (in England); Training for Work (Scotland); Programme Centres; Work Trials; Work Preparation; Workstep; and Access to Work. The CMP is only offered in Pathways areas, while the remaining components of the Choices package are available in non-Pathways areas. It was expected, however, that Pathways participants would be encouraged to take part in these programmes by their IBPAs during their WFIs and thus, that participation in them would increase. Previous research has shown, however, that Pathways actually had little impact on participations in programmes that were also available to non-Pathways participants (see Adam *et al.* 2008).

NDDP is the major Government employment programme available to people claiming incapacity benefits. NDDP is delivered locally by 'Job Brokers', a mixture of voluntary, public and private sector organisations. Although Job Brokers vary enormously in size and in how they operate, most help clients with job search and attempt to increase clients' confidence in their ability to work. Many also attempt to develop clients' work-related skills and monitor clients' progress in jobs after they are placed, sometimes intervening when the client encounters problems on the job. Job Brokers receive a payment from DWP for each client they register, for each client they place in a job, and for each placed client who continues to work for at least three months. Greenberg and Davis (2007) recently completed a cost analysis and a cost-benefit analysis of NDDP. They found that the cost of serving

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[2]   CMP was a newly introduced at the time the analysis corresponds to but should not be considered today as a 'new' programme.

an average participant was between £804 and £1,062 (in 2005 prices), with the true cost probably towards to bottom of this range.

The CMP was a new programme introduced as part of Pathways and run in collaboration with the local National Health Service (NHS). The objective of CMP is to help move claimants of incapacity benefits into work by helping them to manage their health problem better in a work context.[3] Arrangements to accomplish this vary somewhat. Most CMP participants are people with mental health or musculo-skeletal problems and tend to have more serious conditions than NDDP participants. These people also make up the bulk of people receiving incapacity benefits. After an initial assessment, a range of services is provided by occupational therapists, physiotherapists, psychologists, counsellors and others. The exact services that are offered to an individual depend on their condition but can include coping skills, advice, information about exercise and confidence building. Services are sometimes arranged on a one-to-one basis and sometimes in a group or classroom setting.

CMP is managed by the NHS and delivered by a mixture of NHS and private providers. The NHS is reimbursed for its expenditures on the basis of contracts negotiated with DWP. As was shown by Adam *et al.* (2008), the cost per referral of CMP is fairly high (£1,034 in the original seven Pathways areas).[4] Greater costs are sustained in delivering the CMP in rural areas, where staff and participants incur travel costs and separate space to provide services must sometimes be rented, than in urban areas.

The other programmes are much more limited in their use within Choices.[5] **Work-Based Learning for Adults** was a voluntary full-time training programme aimed mainly at people aged 25 and over who have been unemployed for six months or longer and are claiming JSA or incapacity benefits. It was designed to help the long-term unemployed, particularly those who are disadvantaged in the labour market, to move into sustained employment. **Workstep** is a different programme targeted toward the disabled to help them find a job and remain in employment. It provides help and advice to the employees as well as assistance to employers. **Access to Work** is designed to pay for costs associated with accommodating disabled workers at the workplace. It can, for instance, pay for equipment or for transport costs to the workplace. The **Work Preparation** programme is another programme targeted at those individuals who have remained out of work for a long period because of illness or disability. It lasts six weeks and provides advice on the type of job and training that might be best suited to the individual. Several other small schemes also exist.

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[3]   See Barnes and Hudson (2006) for a qualitative analysis of CMP in the seven original pilot areas.

[4]   Given that not all individuals referred onto CMP completed the programme, the cost per participant is likely to be higher than this.

[5]   See Bailey, *et al.* (2007) for detailed evidence concerning the use of Choices.

The structure of this report is as follows. Chapter 2 explains why it is not possible to estimate reliably the causal impact of a voluntary programme like Choices without, for example, being able to exploit piloting by geographical area. Chapter 3 describes the data available and how it was collected, and it also highlights the additional difficulty in measuring whether or not an individual has or has not participated in Choices. Chapter 4 provides the main descriptive analysis of the differences in outcomes between those who participated in Choices and those who did not. Chapter 5 concludes the report.

2 Can we estimate the causal impact of Choices?

The aim of this research was to ascertain what impact Choices participation has on people's chances of leaving benefit, moving into work and other outcomes. In order to do this, we might compare the outcomes of those who participate in Choices with the outcomes of those who do not. This relies on having reliable information on both Choices participation and outcomes: this is discussed in the next section. But there is a more fundamental evaluation problem: those who participate in Choices might typically be different from those who do not, and differences in outcomes between the two groups might reflect these different characteristics as well as the impact of Choices. For example, if Choices participants were more likely than non-participants to move off benefits and into employment, this could be because Choices was effective or it could be because Choices participants were, for example, in any case healthier, more educated or more motivated than non-participants and would, therefore, have been more likely to move into work even in the absence of Choices.

To the extent that we observe the characteristics of the two groups that might affect their outcomes, we can solve this problem by comparing only like with like using 'matching' techniques. In its purest form, matching involves finding a group of non-participants whose observed characteristics are identical to those of the participants, and comparing the outcomes only of these ostensibly similar groups. If observed characteristics were the only pre-existing difference between the two groups that affected the outcomes of interest, any observed difference in outcomes must be caused by participation in Choices.

The technique used in this report, propensity score matching, is a more sophisticated version of this. It relies on the finding (Rosenbaum and Rubin, 1983) that we need not find non-participants who are similar to participants in all observed characteristics; it is sufficient to find non-participants who are similar in terms of a summary measure of their observed characteristics, namely their probability of participating in Choices given their observed characteristics (or 'propensity score'). This makes it much easier to find matching non-participants for each participant:

all but one of the Choices participants has a propensity score which is within the range found among non-participants, making it possible to compare virtually the whole set of Choices participants – rather than a possibly unrepresentative subset – with a 'similar' set of non-participants. This 'similar' set of non-participants is constructed by calculating each individual's propensity score (a relatively straightforward exercise using a probit regression) and reweighting the data on non-participants so that the distribution of propensity scores matches that for participants.[6]

However, propensity score matching will only identify the true impact of Choices if observed characteristics were the only difference between the participants and non-participants that affected the outcomes of interest. In this case, that seems extremely unlikely. For example, we have information on the nature of individuals' health problems, but we do not have very precise information on the severity of each condition and this might be crucial in explaining both participation in Choices and the labour market outcomes. For instance we may know that an individual has experienced depression but we lack substantive information on how severe or durable it has been. More importantly, Choices is a voluntary programme, and participation is likely to reflect psychological factors such as motivation, which in turn could also be crucial in determining outcomes. Those who are convinced that they will never be able to move into paid work might be unlikely to enrol in a Choices programme, but are also unlikely to find work anyway. In this case we should not interpret their lower subsequent employment rate – even given their age, sex, etc – as entirely due to their not participating in Choices.[7] Personal advisers could also prove to be crucial to explain participation in Choices and, even more importantly, to which programme to enrol, and might also independently affect other outcomes such as how soon an individual moves off an incapacity benefit. Various other unobserved factors could determine both participation in Choices and outcomes, including family support or specific personal expectations.

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[6]   See, for example, Blundell and Costa Dias (2000, 2002) for a fuller presentation of the matching methodology and Heckman, Ichimura and Todd (1998) for a comparison between matching estimators and results from experimental data.

[7]   Heckman *et al.* (1998) demonstrate that matching techniques based only on observed characteristics fail in many cases, to remove the selection bias that plagues the evaluation of social programmes. Some researchers are content to assume that controlling for a rich set of background characteristics is enough to remove selection bias: for instance, Dolton *et al.* (2008) assume that a set of control variables similar to those used in this study allow them to recover causal estimates of the impact of the New Deal for Lone Parents (NDLP). However, we find such assumptions unconvincing, at least in the case of Choices: participation in Choices depends on many unobserved factors that could easily be correlated – positively or negatively – with our outcomes of interest.

Furthermore, Choices comprises several different programmes, which were likely to attract rather different kinds of people. The degree – and even direction – of bias entailed in attributing a causal interpretation to differences in outcomes might, therefore, vary between programmes.

Unless the reader believes that the observed characteristics for which we control are the only ways in which participants and non-participants differ that affect the outcomes of interest, the descriptive findings presented in Chapter 4 should not be interpreted as causal impacts of Choices. We describe how outcomes for Choices participants differ from those for non-participants with similar observed characteristics; but we do not show that the difference in outcomes is caused by Choices participation.

In this study we compare individuals in Pathways pilot areas who chose to participate in Choices to those in these areas who chose not to participate in these programmes. As was previously noted, a large number of Choices programmes are available in non-Pathways areas. It is important, therefore, to bear in mind that we do not compare those who could have done Choices (regardless of whether or not they did) with those who could not have done Choices. We also do not compare those who are doing Choices in the Pathways areas with those who are not in the Pathways areas. We compare instead outcomes among those doing Choices within a Pathways area with outcomes of those not doing Choices within a Pathways area.

But would it have been possible to design the evaluation in a way that could have achieved reliable causal estimates? This is an important methodological question that can influence the way further evaluations should be carried out.

What we argue in this report is that it is impossible to rely uniquely on observational data, however rich they can be, in order to estimate causal impact of voluntary programmes like Choices. The only way to obtain confident causal estimates in such cases is by using exogenous variation of the availability of the programme. By exogenous we mean no variation in programme participation that is not correlated with other characteristics not taken into account that are also associated with the outcomes of interest. Exogenous variations can be used in various evaluation designs, like random eligibility thresholds, piloting based on geographical areas or even more robustly, randomisation at the individual level.[8]

By piloting the programme – as was the case with the overall Pathways package – it would, in principle be possible to evaluate its impact as effects due to unobserved characteristics will be cancelled out by comparing control and treatment groups. A difference-in-differences matching methodology would lead in that case to

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[8]   Another possible approach is through the use of a factor that is correlated with whether or not an individual participates in Choices but which is not correlated with the outcome of interest (instrumental variables). In this case there was no factor that was available and convincing.

relatively reliable causal estimates as was demonstrated by Heckman, Ichimura and Todd (1998). Another, more costly but even more reliable, evaluation design, would be to randomise availability of the programme. Randomisation, if carried out with care, would remove the selection bias in a systematic way. The evaluation of Pathways was not designed to make possible an evaluation of the components of Pathways as effort had been directed at the evaluation of the overall package of reforms rather than the individual components.

The difficulty in evaluating components of Pathways, like Choices, is reinforced by the fact that each component (each scheme within Choices) would need to have been randomised or piloted. If each scheme self-selects different individuals with different aims (as it is evident within Choices) it does not make much sense to randomise availability of Choices and not of each scheme.

As a result this report argues that causal impacts should be aimed at only when one is confident that the evaluation design is sufficiently robust to produce unbiased results. In the case of programmes like Choices there was no exogenous variation (either in the form of randomisation, pilots or else) that could allow researchers to be confident that they have dealt with the inherent difference between participants and non-participants.

3   Data

Two types of data are used for this analysis: administrative data and survey data. The administrative data come from various sources: benefits history is drawn from the National Benefits Database (NBD), employment history from the Work and Pensions Longitudinal Study (WPLS), demographic and education information from the Screening Tool (ST) dataset and Pathways-specific information from the Pathways Evaluation Database (PED). The survey data was collected in face-to-face interviews with three groups of people: Choices participants, a matched group of non-Choices participants, and a random group of non-Choices participants. These three samples were themselves identified using an older version of the administrative data.[9] This chapter presents the two-step approach taken to data collection (Sections 3.1 and 3.2) before discussing the issue of measurement of Choices participation (Section 3.3). We finally describe the set of background characteristics for which we are able to control (Section 3.4).

3.1   A two-step approach

The approach taken in this study is based on a two-step matching procedure. The first step consisted of selecting Choices participants and a sample of matched non-participants, along with a smaller sample of random non-participants, for interview. The second step consisted of using both the survey data collected in these interviews and administrative data to implement propensity score matching as described in the previous chapter. The advantage of this two-step approach is that the sample of non-participants for whom rich survey data were collected was already similar to the sample of participants, making the matching exercise in the second step more precise. This methodology is called sequential matching and has been implemented previously in analysis of other active labour market policies (Lechner, 2003).

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[9]   We refer to the dataset that was used to select interviewed individuals as the 'old' administrative data. This dataset was later updated and modified; we call this later version the 'new' administrative data.

In the first step of the procedure we selected individuals who had started Pathways between July and December 2004 from administrative data. This was done in three phases according to the dates individuals had claimed incapacity benefits.[10] The first phase selected individuals who had a Pathways start date recorded between July and August 2004, the second phase covered September and October 2004 and the third phase covered November and December 2004.[11]

For each phase, we began by selecting those individuals who were recorded in the administrative data as starting Pathways in the relevant months and as subsequently starting a Choices option. We restricted ourselves to individuals who were mandated onto Pathways, i.e. who moved onto incapacity benefits after the pilot start dates and who were aged 18 to 59. For the first phase, this gave us 1,154 individuals, of whom around 800 were randomly chosen for the face-to-face survey, with the remainder constituting a 'reserve sample' to replace any that could not be contacted.

There were 10,520 individuals with a Pathways start in July or August 2004 who were not recorded as starting a Choices option. We selected 1,154 of these as the matched sample. These were chosen by running a probit for Choices participation on the 11,674 July-August Pathways entrants and thereby constructing a propensity score for Choices participation for each individual. 1,154 non-participants were chosen as the matched counterparts for the 1,154 participants using nearest-neighbour matching on the propensity score, with the participants with the highest propensity scores matched first.

The explanatory variables used in the probit included district and various demographic, health, education, benefit history, work history and Pathways participation variables recorded in the administrative data.[12] Finally, we selected a further 200 of the remaining 9,366 non-participants at random. These were intended to supplement the Choices and matched samples with a group who have different characteristics on average and may, therefore, have had different experiences of Pathways.

This procedure was repeated for the second and third phases. The number of individuals in each sample is recorded in Table 3.1. A full list of explanatory variables, the probit results, and summary measures of the quality of the resulting match, are shown in Appendix B.

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[10]   See Table 3.1 for the data sample in each of the three phases.

[11]   The samples for these three phases were selected using administrative datasets available at June 2005, August 2005 and November 2005 respectively.

[12]   These data are described more thoroughly in the next section.

Table 3.1   Number of individuals in the chosen samples (first step)

	Choices participants	All non-Choices	Matched non-Choices	Randomly chosen non-Choices
Phase 1 (July-August 2004)	1,154	10,520	1,154	200
Phase 2 (September-October 2004)	1,299	10,299	1,299	300
Phase 3 (November-December 2004)	1,147	9,174	1,147	300

Once the survey data had been collected, the second step of the methodology involved combining it with old and new administrative data to create a new propensity score that took into account a more comprehensive set of observed characteristics. Propensity score matching was used to control for observed characteristics when comparing how various outcomes (specifically employment, earnings, exit from incapacity benefits and self-reported health) differed with participation in various Choices programmes.

3.2   Description of the data

Where possible, two face-to-face interviews were conducted for each person in the sample. The first wave of interviews took place between August 2005 and March 2006 (13 to 15 months after the individuals' incapacity benefits claim started). It was designed to compile information on their background characteristics, previous work and health status, as well as checking whether or not individuals had registered for Choices. The second interview took place between September 2006 and February 2007 in order to collect information on outcome variables (employment status and health measures), as well as checking whether individuals had really followed the Choices programmes. Not all individuals who responded to the wave 1 survey took part in a second interview, and not all individuals interviewed appear in the new (revised) administrative data. Table 3.2 summarises the number of wave 1 survey participants appearing in other datasets.

Table 3.2   Number of individuals in the survey (waves and sample)

	Total	Choices participants	Matched non-Choices	Randomly chosen non-Choices
In wave 1 survey	3,507	1,679	1,538	290
In wave 1 and new administrative data	3,404	1,634	1,489	281
In wave 1 and 2 surveys	2,136	1,045	914	177
In waves 1 and 2 and new administrative data	2,081	1,020	886	175

If the survey data merged with administrative data are the most valuable source of information, it is still possible to use the larger administrative datasets on their own. There is a trade-off here between the breadth of observed characteristics and the number of individuals available. The outcomes of interest are also more limited in the administrative datasets (only exit from incapacity benefits is recorded). Table 3.3 shows the number of Choices participants and non-participants in the administrative datasets. A small number of individuals present in the old administrative dataset were not included in the new dataset.[13]

Table 3.3   Number of individuals in the administrative datasets (old and new)

	Only old administrative	Both old and new administrative
Non-Choices	2,295	26,649
Choices	170	3,271

3.3   Measurement of participation in Choices

One large problem is that the information in administrative data and survey data does not always tally. The recording of participation in Choices particularly raises concerns. The 'Choices participants' identified so far in this section refer to those recorded as starting Choices in the (old) administrative data. But people were also asked about their Choices participation as part of the survey, and the answers do not always correspond.

Table 3.4 shows the proportions of the samples participating in the different Choices programmes according to the different data sources. If one looks at the first two columns of the table, the proportion of individuals who are identified as participating in Choices at all is identical in the administrative and survey data, at 47.8 per cent. But this hides the problem: these are not the same 47.8 per cent of people in each dataset, and their recorded participation is not in the same programmes. In the 'Choices sample' everyone, by definition, had registered with Choices according to administrative data, but only 72 per cent confirmed this in the survey. NDDP seems to be the most accurately measured in the survey while CMP and other Choices programmes are less likely to overlap. One-quarter (26 per cent) of the survey respondents in the non-Choices matched samples report participating in Choices, with a higher proportion of those reporting other Choices programmes than NDDP and CMP.

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[13]   The request of the administrative data was based on dates of incapacity benefits claim that might have been corrected in the new administrative data. Individuals have been selected if their first incapacity benefits spell had been between July and December 2004, according to the old administrative datasets. With the revisions of the claim dates in the new administrative datasets, some individuals have been left aside.

Table 3.4   Propensities to have been on Choices

Source	All survey		Matched sample		Matched non-Choice		Random sample	
	Administrative %	Survey %	Administrative %	Survey %	Administrative %	Survey %	Administrative %	Survey %
Choices	47.8	47.8	100	71.9	0	25.9	0	25.5
NDDP	25.6	25.1	53.4	42.9	0	9.0	0	7.2
CMP	24.8	16.9	51.8	28.9	0	6.2	0	4.1
Others	4.4	23.6	9.2	31.9	0	16.0	0	16.2

Note: The administrative dataset used here is the old one used to select the sample for the face-to-face interview. The sample of data used for this table is the sample of individuals observed in the old administrative data and in the second wave survey interview.

Table 3.5 provides a different view of this issue by presenting the degree of overlap between the two datasets directly. In the first column, the variable considered is defined as participation in Choices (any programme) at all, whereas the last three columns look at participation in any one of the specific programmes. For example, 17.2 per cent of the survey respondents have reported to have participated in NDDP programme and have been found registered in NDDP according to administrative sources. On the other hand 7.9 per cent reported participation in NDDP that was not recorded in administrative NDDP data, and 8.4 per cent declared in the survey that they did not participate in NDDP while being recorded administratively as registered for it.

The discrepancy between the datasets is very large indeed. If three-quarters of survey respondents assess their participation in the Choices package in a similar way as the administrative data had recorded, a quarter reports contradictory information. Almost as many people are participating according to one dataset but not the other as are participating according to both datasets. Within the various components of Choices there are some more specific mismatches. The survey records far more participation in 'other programmes', and far less in CMP, than the administrative data.

Table 3.5   Overlap between administrative and survey data

Administrative/Survey	Choices %	NDDP %	CMP %	Others %
Overlap	73.10	83.70	83.30	77.30
No/No	38.7	66.5	70.8	74.6
Yes/Yes	34.4	17.2	12.5	2.7
No overlap	26.80	16.30	16.70	22.60
No/Yes	13.4	7.9	4.4	20.9
Yes/No	13.4	8.4	12.3	1.7

Note: The administrative dataset used here is the old one used to select the sample for the face-to-face interview. The sample of data used for this table is the sample of individuals observed in the old administrative data and in the second wave survey interview.

It is difficult to assess which of the two sources is more reliable. Survey respondents might give erroneous answers if, for example, they confuse the names of different components of Pathways. On the other hand, the administrative data record CMP referrals rather than starts and do not cover some of the smaller Choices programmes at all; and Jobcentre Plus office staff might not have completed the administrative databases fully and completely. Some discrepancy could also be entirely 'innocent': individuals might have started Choices after the administrative data stop but before they were interviewed. There are many possible explanations for the discrepancy, but after some investigation we were unable to establish

a single preferred measure of Choices participation, and we report descriptive findings using both measures.[14]

Measurement error in Choices participation is a serious issue. In general the sign of the resulting bias is unknown (i.e. whether this is positive or negative and therefore whether the bias will lead to an over or underestimation). But if the error is uncorrelated with observed characteristics explaining both Choices participation and the outcome of interest, it is then likely that the bias will be downward, i.e. an attenuation bias (Battistin and Sianesi, 2006).

3.4   Background characteristics

The matching method used in this study relies heavily on the range of observed characteristics for which we can control. It is, therefore, crucial to present the control variables available in detail and assess how much they might be able to explain Choices participation.

We have a rich set of background characteristics from both the administrative data and the survey data. They include all the background information which is typically available in surveys: sex, age, marital status, ethnicity, years of education, qualifications, type of accommodation, number of children and Jobcentre Plus district. Using the administrative data we can also add information about benefit claim history, job history, disability history and previous income. The survey data also contains health measures. Employment history and health measures are particularly crucial as they are the most likely to capture characteristics correlated with both Choices participation and outcomes. Previous literature has stressed that employment and benefit history generally do a good job of explaining the large variations in employment unexplained by observed current characteristics.[15] In addition to this extensive list of control variables, we add interactions between sex and various other characteristics (years of education, age band and ethnicity).

Table 3.6 presents summary statistics of selected background characteristics, split by Choices participation according to the two different datasets. It gives an overview of the type of variables for which we control. Along with basic demographic characteristics we include full sets of dummy variables for whether the individual has claimed key benefits in the years immediately before the current claim. We also have information on employment history (less robust as the information relies on the survey respondents' recollection). The full set of variables can be found in Table B.1, where we present the results of the probit regression which is used to construct the propensity score.

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[14]   We are grateful to Deborah Pritchard and Graham Oliver at DWP for extensive correspondence and discussion about the construction of the administrative datasets. The full details and their implications for reliability and interpretation of the data are not reported here, but were inconclusive.

[15]   See for instance Bitler, Gelbach and Hoynes (2008).

Table 3.6   Summary statistics of selected background characteristics

Characteristic	Choices participation measure from survey data		Choices participation measure from administrative data	
	Non-Choices	Choices	Non-Choices	Choices
Age (mean)	43.34	40.83	42.42	41.82
Proportion who were:				
Male	0.52	0.56	0.53	0.55
White	0.94	0.95	0.95	0.95
Children	0.29	0.26	0.29	0.26
Married	0.41	0.41	0.41	0.41
Depression	0.20	0.27	0.21	0.26
Mental problems	0.07	0.09	0.08	0.08
Drug or alcohol problems	0.03	0.02	0.02	0.02
Pain	0.30	0.31	0.30	0.31
Back or neck problems	0.15	0.17	0.15	0.17
Expect to work within six months	0.16	0.20	0.16	0.20
Does not expect to work in foreseeable future	0.26	0.24	0.26	0.25
Work impact on health: little worse	0.24	0.24	0.23	0.25
Work impact on health: a lot worse	0.20	0.24	0.21	0.23
JSA last year	0.26	0.31	0.28	0.29
JSA last five years	0.43	0.47	0.44	0.46
IB last year	0.76	0.78	0.76	0.77
IB last five years	0.80	0.83	0.81	0.82
IS with disability premium last year	0.15	0.14	0.14	0.14
IS with disability premium last five years	0.20	0.19	0.19	0.20
Disablity Living Allowance (DLA) last year	0.14	0.12	0.13	0.13
DLA last five years	0.14	0.13	0.14	0.13
Last job within the last six months	0.26	0.30	0.26	0.30
Last job between six and 12 months ago	0.10	0.13	0.11	0.12
Last job between 12 and 24 months ago	0.04	0.04	0.04	0.04
Last job more than 24 months ago	0.10	0.08	0.08	0.09
Never had a job	0.02	0.01	0.01	0.01

Note: The sample of data used for this table is the sample of 2,081 individuals observed in the old administrative data and in the second wave survey interview.

The effect of reweighting the data to produce a matched sample can be seen graphically in Figures 3.1 and 3.2. These graphs present the distributions of propensity scores used in the second-step matching (with all the controls from administrative and survey sources), using the survey-based measure of Choices participation. Figure 3.1 shows the distributions of propensity scores for Choices participants and non-participants. The distributions are already quite similar, partly because the first-step matching selected non-participants to match the observed characteristics (from administrative data) of the participants.[16] However, the samples are not a perfect match, partly because the propensity scores used here are estimated using additional information from the survey data that was not available for the first-step matching and partly because we have included the random sample which was not included in the first-step matching.

Figure 3.1   Distribution of estimated propensity scores before reweighting, by Choices participation as recorded in survey data

Note: Propensity scores estimated using a kernel density function with an Epanechnikov kernel and a bandwidth of 0.03.
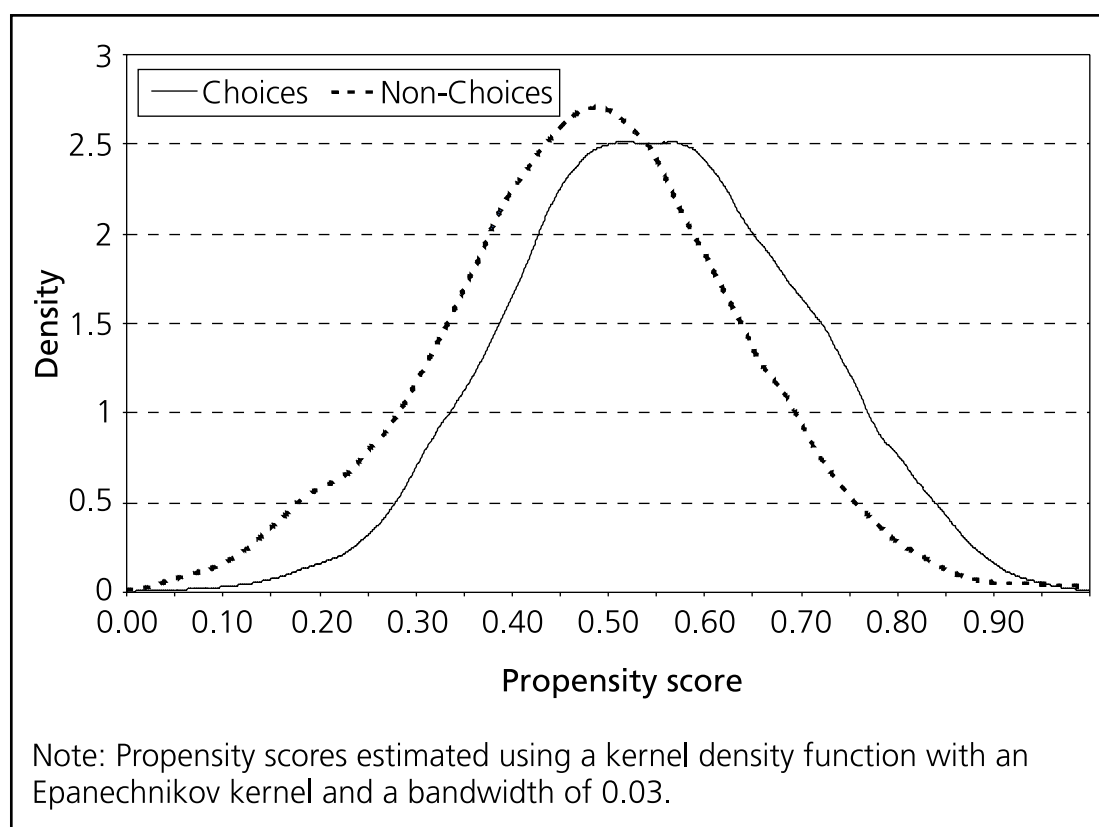
Figure 3.2 shows the weighted distribution of propensity scores as the matching is carried out. The data for non-participants are weighted so that the distribution of their 'propensity scores' is brought into line with the distribution of the

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[16]   The issue of 'common support' – participants for whom there are no 'similar' non-participants, making comparison impossible – is, therefore, not a major problem in this study. Only one Choices participant was excluded as falling outside the common support.

characteristics among Choices participants. Hence, the two distributions are almost identical. This is the mechanical result of the matching technique. If we were controlling for all characteristics that differed between the two groups and affected the outcome of interest, we could confidently draw inferences about the effects of Choices participation.

Figure 3.2 Distribution of estimated propensity scores after reweighting, by Choices participation as recorded in survey data

Note: Propensity scores estimated using a kernel density function with an Epanechnikov kernel and a bandwidth of 0.03.
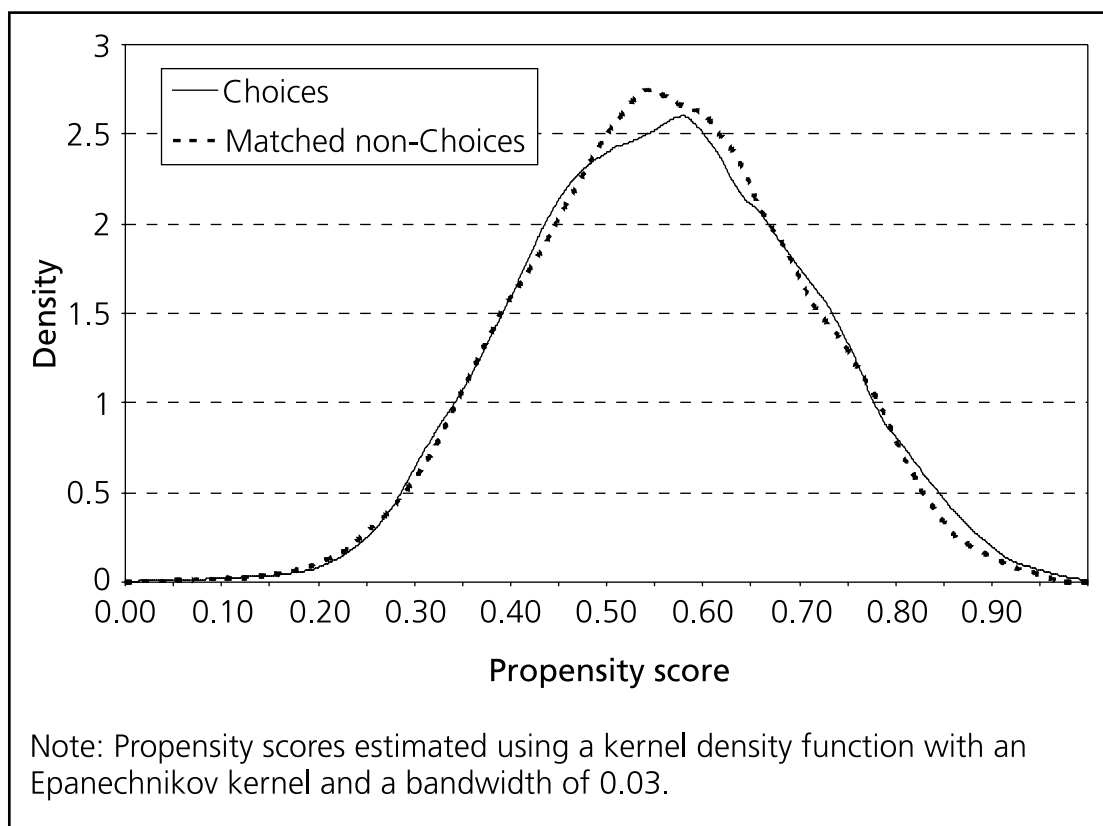
4   Outcomes for Choices participants and non-participants

All the descriptive findings presented in this chapter show the difference in outcomes between Choices participants and non-Choices participants both before and after taking into account all of the observed background characteristics described in Chapter 3. As set out in Chapter 2, the estimates can only be interpreted as the causal impact of the policy under the strong assumption that no other, unobserved, characteristics are correlated with both Choices participation and the particular outcome of interest. Section 4.1 describes the timing of the outcome measures as reported in the survey data. The descriptive findings on employment status and earnings are presented in Section 4.2, while those relating to exit rate from incapacity benefits are presented in Section 4.3 and Section 4.4 concentrates on health outcomes. Sections 4.5 and 4.6 present descriptive findings for the individual components of Choices (NDDP and CMP) and Section 4.7 discusses interpretation issues in more detail.

4.1   Outcome measures

The survey and administrative data include various outcome measures of interest. From the administrative data, we look at the exit rate from incapacity benefits at each month, using information on benefit spells. From the survey data, we measure employment status, earnings and health measures (described later) at the time of the second interview.

Figure 4.1 presents the distribution of time from incapacity benefits claim to second survey interview. For most respondents, the main outcomes are measured about two years after the claim, slightly later than was studied in the main impact assessment of Pathways as a whole.[17]

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[17]   In Bewley *et al.* (2007) employment is measured around 18-22 months after the claim.

Figure 4.1 Distribution of time from incapacity benefits claim to second interview, by Choices participation as recorded in survey data
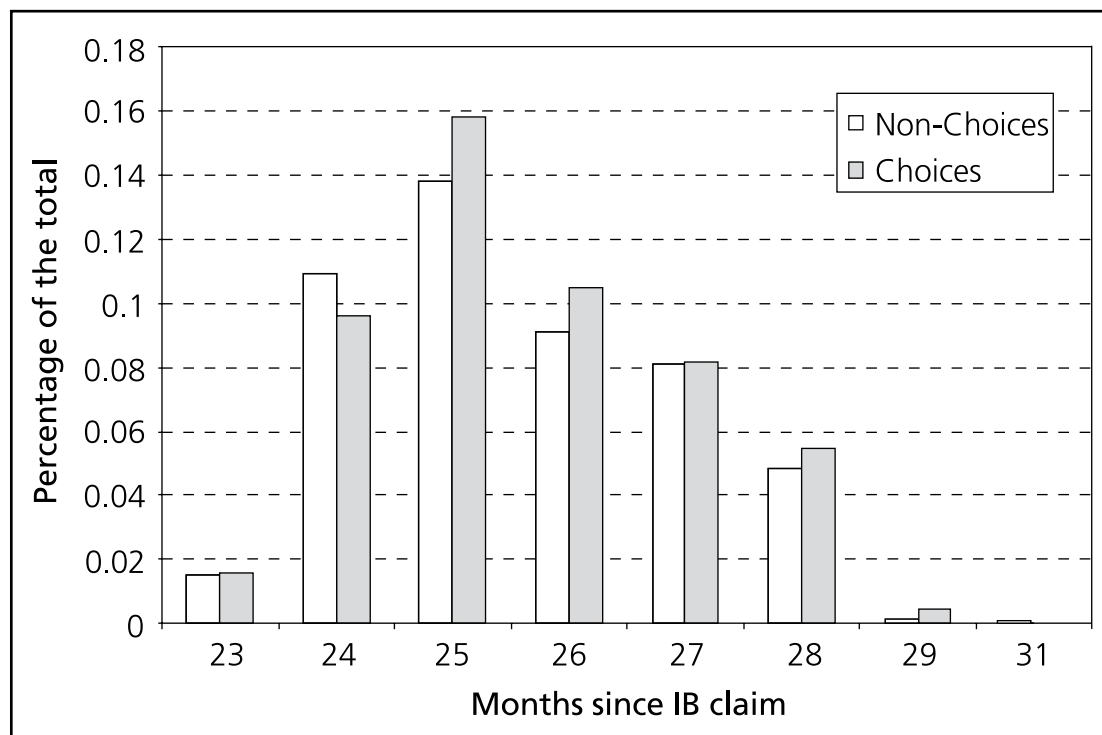
While we look at employment outcomes in all the Choices programmes, it may be that specific schemes, like CMP, had not explicit early labour market objectives. CMP was designed to help those further from the labour market improve their management of their health condition, with a longer-term trajectory towards moving into work. Outcome measures are also potentially different in the case of CMP, relative to NDDP, as CMP was still in its early stages at the timing of the analysis.[18]

4.2   Employment and earnings

All the tables in this section are based on the same methodology and provide the same type of estimates. Table 4.1 presents the descriptive findings where the outcome variable is employment status as measured by the survey at the second interview. Given that we have two measures of Choices participation that partly contradict each other, we present estimates based separately on each of the two measures as well as a third estimate based on the subset of individuals for whom both sources of data do agree, with those individuals for whom there is a discrepancy being dropped from the analysis.

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[18]   The CMP was introduced over December 2003-January 2004 in the first three pilot areas (six months before the first phase of the analysis) and from July 2004 in the remaining four pilot areas.

For each measure of Choices participation, the first line in the table shows the simple, unadjusted employment rate at the time of second interview among Choices participants and non-participants. In the case of the survey measure of participation, there is a difference of 9.4 percentage points between the two groups. The second line shows descriptive findings once the observed characteristics are accounted for (Matched[19]): we find a 6.4 percentage point higher employment rate among Choices participants than for **observably similar** individuals who did not participate in Choices. The reduction of the association between Choices participation and subsequent employment from 9.4 to 6.4 percentage points is due to the fact that individuals who participated in Choices tended to have observed characteristics that were also correlated with higher probability of moving into work. Controlling for these observed characteristics therefore reduces the positive association between Choices participation and subsequent employment.

Table 4.1   Employment status by Choices participation

Choices participation measure	Sample	Choices	Non-Choices	Difference	S.E.	T-stat
Choices (survey)	Unmatched	41.02	31.62	9.40**	2.08	4.52
	Matched	41.06	34.69	6.37**	2.54	2.51
Choices (administrative)	Unmatched	40.85	32.17	8.68***	2.08	4.17
	Matched	40.93	30.90	10.04***	2.28	4.40
Choices – excluding observation not overlapping	Unmatched	43.02	30.45	12.57***	2.43	5.17
	Matched	43.21	31.62	11.59***	2.93	3.96

Note: kernel matching, standard errors (S.E.s) obtained by bootstrapping (500 repetitions). The employment rate is expressed in percentage points. *** indicates statistical significance at the 1% level, ** at the 5% level and * at the 10% level.

The second line of Table 4.1 presents equivalent descriptive findings using the measure of Choices participation from the administrative data. On this measure, we find that those who participated were subsequently 10.0 percentage points more likely to be in paid work. One concern with this estimate is that this difference is actually larger than the raw difference (i.e. before taking into account observed characteristics). We saw in Chapter 3 that there are arguments in favour of either survey or administrative data regarding which source of data to trust in terms of correctly measuring whether or not an individual participated in Choices. However we would expect the inclusion of the large set of observed characteristics to lead to a reduced association between these outcomes and the likelihood of having participated in Choices: it seems likely that characteristics associated with Choices participation would be associated with a higher, not lower, chance of moving into work. The estimates based on administrative data seem, therefore, more dubious than those based on survey data.

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[19]   'Matched' in fact means the average treatment effect on the treated (ATT), i.e. the average difference in outcomes for a group with the characteristics observed in Choices participants.

The last line of estimates in Table 4.1 presents the same methodology restricting the samples to the individuals where the survey and the administrative data agree on whether or not the individual participated in the Choices programme. If the measurement error is not correlated with the characteristics which explain participation onto Choices and the outcome variable, then removing these individuals does not cause any problem. But if this is not the case, the estimates are likely to be more biased. We find here an even bigger association between participation in Choices and subsequent likelihood of being in paid work, 11.6 percentage points. As with the administrative data, however, the observed characteristics do not seem to account for any of the difference between these two groups. This potentially casts further doubt on whether these differences can genuinely be considered to be the causal impact of the Choices programme on subsequent employment outcomes.

Table 4.2 presents descriptive findings using the same methodology but looking at weekly gross earnings as measured in the second survey.[20] Using the Choices participation measure from survey data, all the difference in earnings between participants and non-participants disappears once observed characteristics are accounted for. Using the administrative measure of participation, taking into account the observed characteristics does not change the raw difference between the two groups. These suggest that participation in Choices is associated with increased subsequent gross earnings of £14.80 per week. If one excludes the individuals with contradictory information, the positive association between Choices participation and subsequent earnings fades away.

Table 4.2   Average earnings by Choices participation (including those with zero earnings)

Choices participation measure	Sample	Choices	Non-Choices	Difference	S.E.	T-stat
Choices (survey)	Unmatched	77.29	61.69	15.61***	5.84	2.67
	Matched	77.36	75.80	1.57	8.36	0.19
Choices (administrative)	Unmatched	76.85	62.62	14.23**	5.84	2.44
	Matched	77.00	62.16	14.84**	6.63	2.24
Choices – excluding observation not overlapping	Unmatched	82.15	61.55	20.60***	6.97	2.96
	Matched	82.62	67.39	15.23	10.05	1.52

Note: kernel matching, standard errors (S.E.s) obtained by bootstrapping (500 repetitions). The employment rate is expressed in percentage points. *** indicates statistical significance at the 1% level, ** at the 5% level and * at the 10% level.

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[20]   Missing values for earnings (non-employment) are treated as zeros to differentiate the impact on employment for the one on earnings.

These descriptive findings seem highly sensitive to whether participation in Choices is taken from the survey or from the administrative data. It seems, however, that the estimates based on the administrative measure of participation are less credible given that the expected reduction of the positive association between Choices participation and subsequent employment arising from controlling for observed characteristics, is only present when the survey measure of Choices participation is used. If one were to rely on the estimates based on survey data, an employment 'effect' of 6.4 percentage points, statistically significant at the 5% level, would be the main result, with no association between Choices participation and subsequent earnings. These estimates would still rely on the assumption that unobserved differences between participants and non-participants do not affect outcomes. If that were not the case, one should take these estimates as overestimates of the true effect of the programme. We discuss further in Section 4.5 how to interpret these descriptive findings.

4.3   Receipt of incapacity benefits

To assess the potential impact of Choices on the exit rate from incapacity benefits, we use administrative data on benefit receipt. Given we have data for all benefit spells, we can calculate the proportion who have left incapacity benefits at different intervals after the claim. Table 4.3 presents the main descriptive findings using the measure of Choices participation from the survey data. The individuals who chose to participate in Choices had lower exit rates from incapacity benefits than those who did not, being 5.0 percentage points more likely to be on benefits six months after the claim. This negative outcome fades to zero after one year since the claim, and becomes positive after two years (4.9 percentage points less likely to be on benefits after two years and 5.1 percentage points after 30 months).

Table 4.3   Benefit exits by Choices participation (participation as measured in the survey)

Month after IB claim	Sample	Choices	Non-Choices	Difference	S.E.	T-stat
6 months	Unmatched	18.78	21.53	-2.75	1.74	-1.58
	Matched	18.80	23.83	-5.04**	2.06	-2.44
12 months	Unmatched	36.74	33.56	3.18	2.07	1.53
	Matched	36.77	37.09	-0.32	2.26	-0.14
18 months	Unmatched	43.30	37.63	5.67***	2.13	2.66
	Matched	43.34	41.04	2.30	2.26	1.02
24 months	Unmatched	48.13	40.45	7.69***	2.15	3.57
	Matched	48.18	43.25	4.92**	2.27	2.17
30 months	Unmatched	51.60	43.65	7.95***	2.16	3.68
	Matched	51.64	46.59	5.06**	2.36	2.15

Note: kernel matching, standard errors (S.E.s) obtained by bootstrapping (500 repetitions).
*** indicates statistical significance at the 1% level, ** at the 5% level and * at the 10% level.

Table 4.4 presents similar estimates to Table 4.3 but uses the measure of Choices participation from the administrative data. Contrary to the employment outcomes previously surveyed, the descriptive findings on benefit exits seem to be relatively consistent between the two measures of Choices participation. Using the measure from the administrative data, we find a slightly less negative association between participation in Choices and benefit receipt in the first six months after the claim (-3.4 percentage points). We also find a positive association after one year (not significant at the 5% level) and a significant positive association thereafter (+6.3 percentage points after two years and lasting). The association is precisely estimated, significant at the 1% level.

Table 4.4   Benefit exits by Choices participation (participation as measured in the administrative data)

Month after IB claim	Sample	Choices	Non-Choices	Difference	S.E.	T-stat
6 months	Unmatched	18.39	21.72	-3.33*	1.74	-1.91
	Matched	18.33	21.70	-3.37*	1.87	-1.80
12 months	Unmatched	37.08	33.46	3.62*	2.07	1.75
	Matched	37.05	33.04	4.01*	2.20	1.82
18 months	Unmatched	43.27	37.95	5.33**	2.13	2.50
	Matched	43.26	37.78	5.47**	2.25	2.44
24 months	Unmatched	47.63	41.25	6.38***	2.15	2.96
	Matched	47.62	41.28	6.34***	2.21	2.87
30 months	Unmatched	51.02	44.55	6.47***	2.16	2.99
	Matched	51.02	44.92	6.10***	2.21	2.76

Note: kernel matching, standard errors (S.E.s) obtained by bootstrapping (500 repetitions).
*** indicates statistical significance at the 1% level, ** at the 5% level and * at the 10% level.

One can reproduce these estimates for each month since IB claim to assess the timing of the potential impact of the programme. Figure 4.2 represents the association between Choices participation and subsequent benefit receipt from the sixth month after the claim up to the thirtieth month, based on the survey measure of participation. Ninety-five per cent confidence intervals based on bootstrapped standard errors are also shown in the graph. Up to the first year the association between Choices participation and subsequent benefit receipt seems to be negative. Individuals who participated in Choices were more likely to stay on benefits during that time. After that date exit rates from benefits for Choices participants steadily increase, up to two years after the claim when the positive association between Choices participation and subsequent benefit receipt stabilises.

Figure 4.2 Exit rates from incapacity benefits over time for Choices participants relative to the matched sample of non-participants (survey measure of participation)

Note: The effect is measured in percentage points and the horizontal axis represents the number of months since IB claim. Dashed lines represent the 95% confidence interval, using bootstrapping to compute standard errors (500 replications).
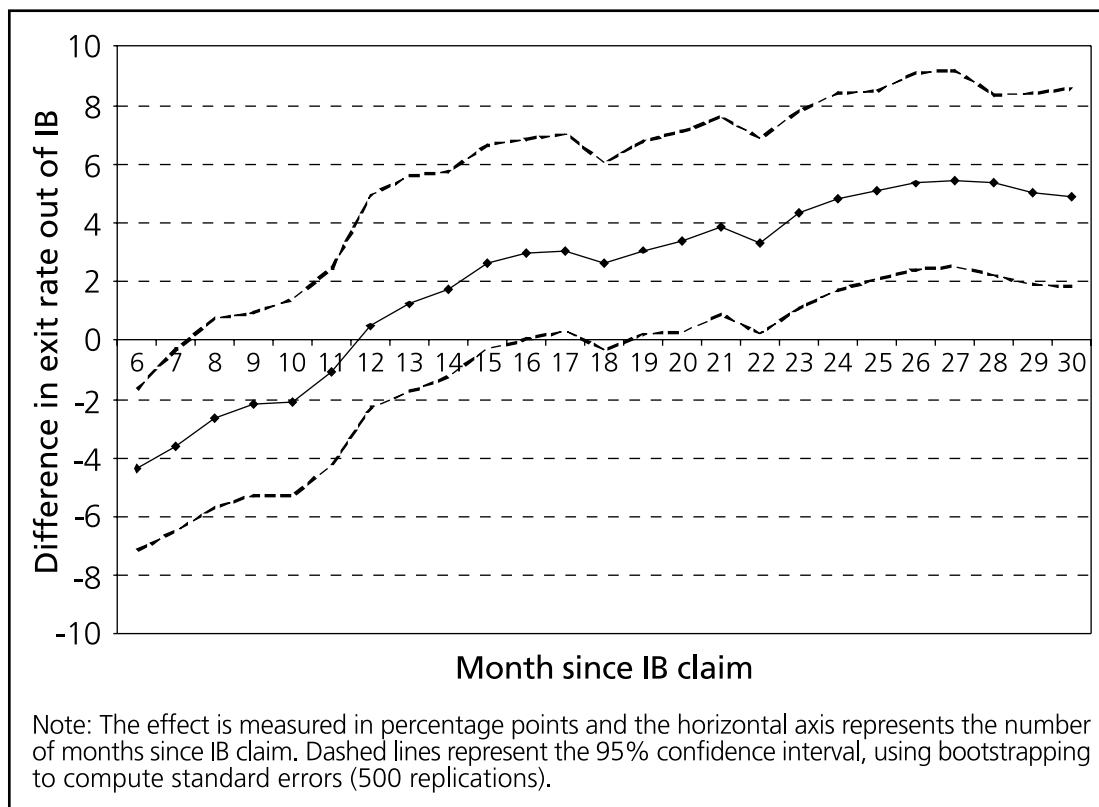
Figure 4.3 presents the same monthly estimation using the measure of Choices participation from the administrative data. The patterns are similar, but with a slightly faster increase in rates of benefit exit. Twelve months after the claim we find a significant positive association between Choices participation and subsequent benefit receipt on exit rates, whereas using the survey measure of participation this is only true after the sixteenth month. After 12 months there is a break in the trend, with a slower increase up to two years after the claim. From 24 months onwards after the claim the estimates from the two sources of data are both stable.

Figure 4.3 Exit rates from incapacity benefits over time for Choices participants relative to the matched sample of non-participants (administrative measure of participation)

Note: The effect is measured in percentage points and the horizontal axis represents the number of months since IB claim. Dashed lines represent the 95% confidence interval, using bootstrapping to compute standard errors (500 replications).
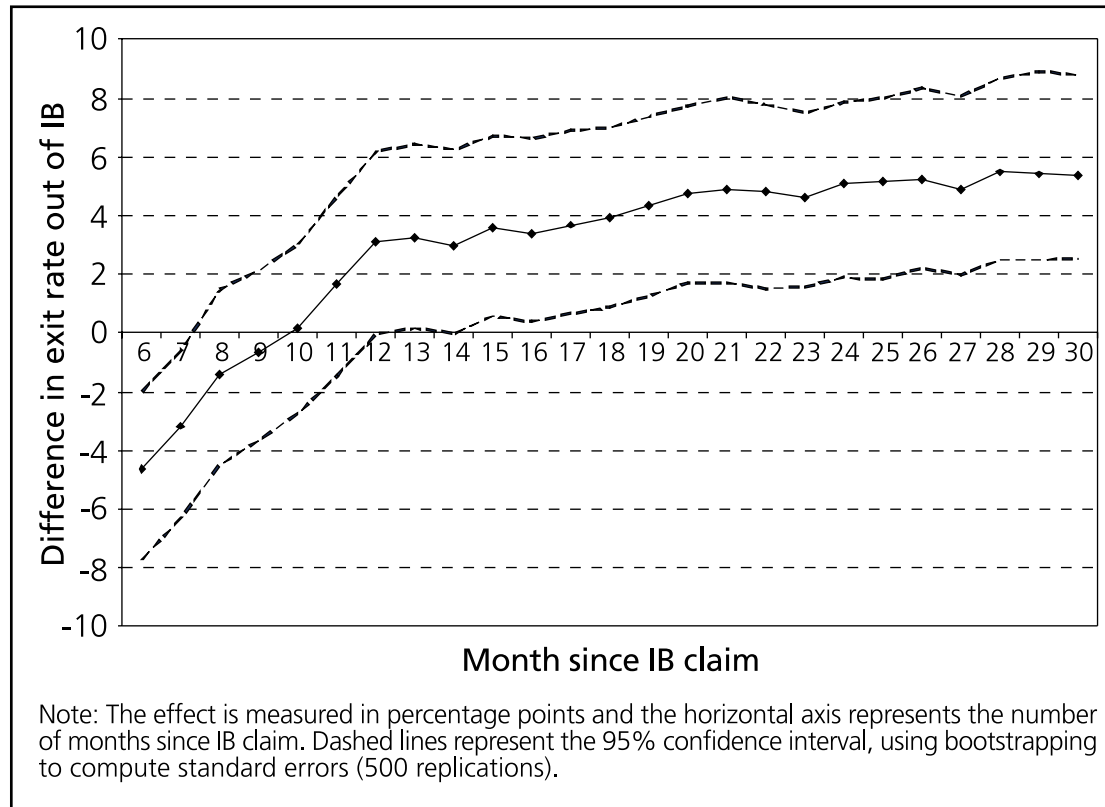
To confirm these descriptive findings we use the full sample of administrative data to look at benefit exits (using the administrative measure of participation). The advantage of this approach is a much larger sample size; the drawback is that one cannot use the richer set of observed background characteristics from the survey data. Table 4.5 presents the findings from this analysis. They do not differ very much from the estimates on the survey sample. If anything, the descriptive findings are more precisely estimated, with some negative associations between Choices participation and subsequent benefit receipt becoming statistically significant earlier, as soon as 12 months after the claim. The estimate after 30 months is very close to the previous estimate on the survey sample (+5.7 percentage points compared to +5.9).

Table 4.5   Benefit exits by Choices participation (full administrative sample, administrative controls and treatment measure only)

Month after IB claim	Sample	Choices	Non-Choices	Difference	S.E.	T-stat
6 months	Unmatched	21.19	26.27	-5.09***	0.81	-6.29
	Matched	21.19	24.89	-3.71***	0.83	-4.49
12 months	Unmatched	40.08	36.34	3.74***	0.89	4.19
	Matched	40.08	37.18	2.90***	0.97	3.00
18 months	Unmatched	46.62	39.98	6.64***	0.91	7.30
	Matched	46.62	42.06	4.56***	0.99	4.63
24 months	Unmatched	50.72	42.58	8.14***	0.92	8.88
	Matched	50.72	45.68	5.04***	0.99	5.09
30 months	Unmatched	54.39	44.69	9.69***	0.92	10.52
	Matched	54.39	48.73	5.66***	0.99	5.73

Note: number of observations 29,878. Kernel matching, standard errors (S.E.s) obtained by bootstrapping (500 repetitions). *** indicates statistical significance at the 1% level, ** at the 5% level and * at the 10% level.

These descriptive findings on exit rates from benefits are somewhat positive for the long-term possible impact of the Choices programme, but they also imply that the real up-front cost of Choices is actually higher than one might have thought, since individuals who participated in Choices tended to stay longer on benefits at first before exhibiting higher off-flow rates from IB. But it should be noted that it is still not necessarily the case that participation in Choices causes individuals to be more likely, and then less likely, to be in receipt of incapacity benefits. For example, it could be that individuals who expected to stay ill for a short time and/or those who expected to get better after a year or so were more likely than other incapacity benefit claimants to choose to participate in Choices.

4.4   Self-assessed health

The Choices package within Pathways should not be assessed only in terms of employment and benefit outcomes. It was designed specifically to help individuals cope better with their health problems and thus, might lead to improvement in how individuals assess their health difficulties. The survey asked respondents in the second wave interviews, their self-assessment of health. We can use these variables as outcomes. The first two variables correspond to questions on the current health status ('bad' or 'good') and the two following variables measure improvements (or deterioration) in health ('worse' or 'better'). Tables 4.6 and 4.7 present the findings from this analysis, again, using two different measures of participation in Choices.

In Table 4.6 where the measurement of whether or not the individual participated in Choices is from survey data, respondents who participated in Choices are less likely to say that they are in bad health (-6.9 percentage points) but not more likely to say that they are in good health than individuals who had not participated in Choices. An imprecisely estimated, but still positive, association between an individual participating in Choices and their likelihood of subsequently reporting that they are in better health is also found (+3.0 percentage points).

Table 4.6   Self-assessed health by Choices participation as measured in survey data

Variable	Sample	Choices	Non-Choices	Difference	S.E.	T-stat
Bad health	Unmatched	31.08	41.22	-10.14***	2.07	-4.89
	Matched	31.08	37.98	-6.90***	2.31	-2.95
Good health	Unmatched	29.44	27.64	1.80	1.96	0.92
	Matched	29.44	31.93	-2.49	2.25	-1.14
Better health	Unmatched	19.51	14.35	5.15***	1.63	3.17
	Matched	19.51	16.53	2.98*	1.96	1.66
Worse health	Unmatched	22.79	29.19	-6.41***	1.90	-3.38
	Matched	22.79	26.17	-3.38	2.13	-1.57

Note: kernel matching, standard errors obtained by bootstrapping (500 repetitions). Statistical significance of the differences is shown only for the samples Matched (the ATT). *** indicates statistical significance at the 1% level, ** at the 5% level and * at the 10% level.

Table 4.7 shows similar estimates using the administrative measure of Choices participation. The same finding appears to hold, with a more precise estimate of the association between participation in Choices and subsequent health improvements (+3.4 percentage points).

These descriptive findings could be interpreted in another way. The fact that even controlling for observed characteristics, individuals who participated in Choices have lower propensities to state that they are in bad health might mean that unobserved characteristics (for instance severity of health problem) do explain both participation in the programme and outcomes. This interpretation cannot be completely set aside and should be kept in mind when considering the findings for other outcomes.

Table 4.7   Propensity score matching on survey data (choices variable from admin)

Variable	Sample	Choices	Non-Choices	Difference	S.E.	T-stat
Bad health	Unmatched	32.62	39.14	-6.51***	2.08	-3.13
	Matched	32.62	39.21	-6.59***	2.19	-3.01
Good health	Unmatched	29.04	28.05	0.99	1.96	0.51
	Matched	29.04	28.78	0.26	2.02	0.13
Better health	Unmatched	18.88	15.22	3.66**	1.63	2.25
	Matched	18.88	15.46	3.42**	1.70	2.02
Worse health	Unmatched	24.10	27.59	-3.48*	1.90	-1.83
	Matched	24.10	27.26	-3.16	2.10	-1.50

Note: kernel matching, standard errors obtained by bootstrapping (500 repetitions).
*** indicates statistical significance at the 1% level, ** at the 5% level and * at the 10% level.

4.5   Outcomes for NDDP participants

The evaluation of the Choices package so far has been based on the aggregation of the potential effects of multiple schemes with very different properties and possibly very different effects. Even within some programmes, different Job Brokers might provide differentiated services that could, in principle, be evaluated. It is however impossible to compare outcomes for each scheme separately given the very small number of individuals in the smaller schemes. Instead, we compare outcomes separately for the two main components of the Choices package: the NDDP and the CMP.[21]

The estimation strategy is similar to the one used previously except that here we compare outcomes between those who participated in a specific Choices programme and those who participated in none.

Table 4.8 presents these estimates for the NDDP scheme – the most popular scheme in Choices – measuring whether or not the individual participated in NDDP from survey data.[22] The differences between groups are much larger than in the case of overall Choices participation. The difference in employment status (Matched) stands as high as 12.2 percentage points while the difference in exit rates from incapacity benefits follows a similar pattern to that shown in Section 4.2. Thirty months after the incapacity benefits claim, the exit rate is 9.0 percentage points higher in the treated group. Health outcomes also show more pronounced differences than previously: NDDP participants are less likely to be in bad health and more likely to state an improvement in their health condition.

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[21]   We leave out the small number (less than five per cent) of individuals who participated both in NDDP and CMP.

[22]   The results using administrative sources are reproduced in Appendix B.

Table 4.8   Differences in outcomes between NDDP participants and non-participants (participation measure from survey data)

Variable	Sample	NDDP	Non-Choices	Difference	S.E.	T-stat
In work	Unmatched	48.60	31.61	16.99***	2.50	6.80
	Matched	48.68	36.50	12.18***	3.43	3.55
Earnings	Unmatched	93.20	61.88	31.32***	7.22	4.34
	Matched	93.37	81.60	11.76	11.93	0.99
Exit rate from IB – 6 months	Unmatched	18.42	21.56	-3.14	2.11	-1.49
	Matched	18.45	24.85	-6.40**	3.03	-2.11
Exit rate from IB – 12 months	Unmatched	39.82	33.46	6.36**	2.50	2.54
	Matched	39.89	39.15	0.74	3.37	0.22
Exit rate from IB – 18 months	Unmatched	46.67	37.56	9.11***	2.56	3.56
	Matched	46.75	42.97	3.78	3.42	1.10
Exit rate from IB – 24 months	Unmatched	52.81	40.39	12.42***	2.58	4.81
	Matched	52.90	45.13	7.77**	3.31	2.35
Exit rate from IB – 30 months	Unmatched	56.67	43.61	13.06***	2.59	5.04
	Matched	56.77	47.75	9.02***	3.27	2.78
Bad health	Unmatched	26.67	41.37	-14.70***	2.48	-5.92
	Matched	26.71	38.10	-11.39***	3.79	-3.01
Good health	Unmatched	31.75	27.71	4.05*	2.37	1.70
	Matched	31.63	29.60	2.04	3.39	0.60
Better health	Unmatched	21.40	14.34	7.06***	1.95	3.62
	Matched	21.44	15.36	6.08**	2.65	2.30
Worse health	Unmatched	22.46	29.27	-6.81***	2.31	-2.95
	Matched	22.50	24.73	-2.24	3.06	-0.73

Note: kernel matching, standard errors obtained by bootstrapping (500 repetitions).
*** indicates significant estimation at the 1% level, * at the 10% level.

Measuring whether or not the individual participated in NDDP from administrative sources (see Table C.1), we find even stronger effects on employment and on benefits exits.

4.6   Outcomes for CMP participants

We repeat this analysis to look at outcomes by CMP participation.

Table 4.9 presents these differences using the participation variable from the survey data. The estimates are strikingly different from those for the NDDP scheme and for the overall Choices package. No difference in employment can be seen for those individuals who participated in the CMP while benefits receipt is higher for a longer period, with exit rates from IB remaining significantly negative (-9.5 percentage points) up to 12 months after the claim and the differences

remaining negative (though not significant) thereafter. In contrast those who choose to participate in CMP are much more likely to state an improvement in their health condition (+6.1 percentage points).

Measuring whether or not the individual participated in CMP from administrative sources, the main descriptive findings are unchanged (see Table C.2). We find positive employment differences, but only significant at the 10% level and no positive differences in benefits outflow rates.

There two separate reasons that can explain the outcome differences for CMP: First, the selection process into CMP is likely to be different as individuals with worse health and who are further away from returning to the labour market are more likely to enrol on this programme. Another possible reason is that CMP was a relatively new programme at the time of the evaluation, whereas NDDP was well established well before the introduction of Pathways.

Table 4.9   Differences in outcomes between CMP participants and non-participants (participation measure from survey data)

Variable	Sample	CMP	Non-Choices	Difference	S.E.	T-stat
In work	Unmatched	27.52	31.56	-4.04	2.70	-1.5
	Matched	27.53	27.64	-0.11	3.54	-0.03
Earnings	Unmatched	45.38	61.90	-16.52**	7.47	-2.21
	Matched	45.64	55.25	-9.61	9.59	-1.00
Exit rate from IB – 6 months	Unmatched	9.58	21.53	-11.95***	2.24	-5.34
	Matched	9.34	20.16	-10.82***	2.79	-3.88
Exit rate from IB – 12 months	Unmatched	23.83	33.73	-9.89***	2.70	-3.67
	Matched	23.74	33.26	-9.52***	3.55	-2.68
Exit rate from IB – 18 months	Unmatched	31.94	37.76	-5.82***	2.81	-2.07
	Matched	31.82	37.10	-5.28	3.68	-1.44
Exit rate from IB – 24 months	Unmatched	36.86	40.41	-3.56	2.87	-1.24
	Matched	36.87	39.13	-2.26	3.65	-0.62
Exit rate from IB – 30 months	Unmatched	42.01	43.66	-1.64	2.91	-0.57
	Matched	42.17	43.40	-1.23	3.84	-0.32
Bad health	Unmatched	42.01	41.10	0.91	2.89	0.32
	Matched	42.17	45.25	-3.07	4.11	-0.75
Good health	Unmatched	17.44	27.53	-10.09***	2.52	-4.01
	Matched	17.17	22.96	-5.79*	3.24	-1.79
Better health	Unmatched	20.39	14.26	6.14***	2.15	2.86
	Matched	20.71	14.58	6.13**	3.12	1.964
Worse health	Unmatched	25.31	28.91	-3.60	2.63	-1.37
	Matched	25.51	29.04	-3.53	3.65	-0.97

Note: kernel matching, standard errors obtained by bootstrapping (500 repetitions).
*** indicates significant estimation at the 1% level, ** at the 5% level, * at the 10% level.

4.7   Can we attribute differences in outcomes to participation in Choices?

The descriptive findings presented throughout this chapter can be interpreted as the causal impact of the Choices programme only under the very strong assumption that unobserved differences between participants and non-participants have no effect on the outcomes of interest. This assumption is highly questionable. Even with the very large set of observed characteristics this study relies on, it is very likely that, conditional on observed characteristics, some unobserved characteristics are associated with an individual's decision to participate in Choices and the subsequent outcomes of interest. In particular, if the information on employment and benefits history are deemed to be excellent controls for an individual's work preferences, the information we have at our disposal in terms of health could still be viewed as limited as regards to the heterogeneity of individual situations. Within each type of health condition controlled for, there is scope for wide variations. And of course we cannot fully know people's feelings, attitudes and personalities.

This is best seen in the descriptive findings for NDDP and CMP. NDDP participants tend to be relatively less severely disabled individuals than CMP participants in terms of how work-ready they are. This is captured, in part, in how the matching process reweights the control groups relative to the treated: the difference in terms of outcomes is reduced by accounting for observed characteristics in the case of NDDP whereas it is increased in the case of CMP. It is likely that there are unobserved characteristics which at least, in part, explain the remaining differences in outcomes. Their exclusion is likely to lead to overestimation of the impact of the policy in case of NDDP and underestimation in the case of CMP. To be clear, it would be wrong to interpret our descriptive findings as proof of the effectiveness of NDDP and ineffectiveness of CMP given they have to deal with individuals who are likely to differ greatly on their unobserved characteristics.

In light of this major caveat it is best not to interpret these descriptive findings as causal but as careful descriptions of the characteristics and outcomes of the individuals who have chosen to participate in the Choices packages of the Pathways programme.

5 Conclusions

In this study we applied propensity score matching to various measures of participation in the Choices package within Pathways to Work. Using both administrative and survey data we have been able to control for a very large set of observed characteristics including demographics, employment and benefits history as well as health measures. But the voluntary nature of the Choices programme means that we have been unable to estimate causal effects of the policy, as unobserved characteristics of individuals are likely to play a role in explaining both participation in the programme and also the outcomes of interest.

We found that individuals who chose to participate in Choices were more likely than non-participants to stay on benefits in the first year after their incapacity benefits claim, but were more likely to leave incapacity benefits thereafter. Compared to similar non-participants, those who took part in a Choices programmes had higher exit rates from incapacity benefits two years after the claim. Individuals who chose to participate in Choices were also subsequently more likely to report improvement in their health condition. This seems to be robust to the different sources of data. However, as previously stated, we cannot tell how far these differences were a result of different unobserved characteristics of Choices participants, such as expectations, motivation, family support and severity of health condition and so on: they cannot simply be interpreted as the result of participating in Choices.

Differentiating the descriptive findings by the different Choices components requires even stronger care in interpretation as unobserved characteristics might matter even more in the self-selection into various programme components. We find that individuals who registered for NDDP tended to leave benefits at higher rates than individuals who were similar in terms of their observed characteristics but who did not participate in Choices and were more likely to move into work by the time of second interview. On the other hand, CMP participants, who are typically more severely disabled, did not exhibit higher employment rates than those individuals with similar observed characteristics who did not participate in Choices. This is perhaps not surprising since CMP is designed to help those further from the labour market improve their management of their health condition, with a longer-term trajectory towards moving into work.

Individuals who participated in Choices stayed longer on benefits in the first year after their incapacity benefits claim and only thereafter are found to have left benefits at a higher rate. However, this could be due to the fact that individuals who expect to move off an incapacity benefit relatively quickly might be less likely to choose to participate in such a programme than those who expect to continue receiving an incapacity benefit for at least a slightly longer period of time.

Estimating causal impacts of voluntary programmes like Choices, where individuals might self-select in different ways, cannot, in our view, rely uniquely on observed characteristics. Randomisation at the individual level, geographical pilots or any other source of exogenous variation seem to be a requirement for any evaluation design that aims to establish reliable causal impact estimates in those particular settings.

Appendix A
Timetable of Pathways implementation

October 2003	Jobcentre Plus Pathways for new claimants in three pilot areas (Renfrewshire, Inverclyde, Argyle and Bute; Bridgend, Rhondda, Cynon and Taff; and Derbyshire).
April 2004	Jobcentre Plus Pathways for new claimants in four pilot areas (Essex; Gateshead and South Tyneside; Lancashire East and Somerset).
February 2005	Jobcentre Plus Pathways for existing claimants (for less than two years) in the seven pilot areas.
October 2005	Jobcentre Plus Pathways for new claimants in 14 additional areas.
April 2006	Jobcentre Plus Pathways for existing claimants (for less than six years) in the seven pilot areas.
December 2007	PL Pathways for new claimants in 15 additional areas.
April 2008	PL Pathways for the new claimants in 16 remaining areas of Great Britain, so now covering the whole country.
October 2008	Introduction of ESA (replacing IB).
April 2009	Jobcentre Plus/PL Pathways extended to existing claimants aged under 25 across the whole of Great Britain (apart from initial seven areas where already in place).

Note: For details of the timetable of the roll-out of Pathways in Northern Ireland see http://www.northernireland.gov.uk/news/news-del/news-del-june-2007/news-del-210607-helping-those-with.htm

Appendix B
Probit estimation to construct propensity score

In order to construct the propensity score, i.e. the index used to match non-participants to participants, we run a probit regression. Probits are used when the outcome variable is discrete (either 1 or 0) and makes the assumption that the probability distribution of the underlying outcome variable follows a normal distribution.

Table B.1   Probit estimation to create propensity score matching (treatment from survey data)

	Coefficient	Marginal effects	Standard errors of marginal effects
Screening score	-0.04	0.05	-0.33
Screening score (2)	0.07	0.03	0.83
Have a job, but couldn't get Sick Pay	-0.05	0.03	-0.70
Claim another benefit (e.g. JSA)	-0.05	0.04	-0.50
Last job in last year	0.08	0.03	1.02
Last job between one and two years	-0.16	0.05	-1.29
Last job more than two years ago	-0.17	0.04	-1.56
Has still an employment contract	0.05	0.03	0.66
Does not know about contract	0.29	0.14	0.83
Expect to work within six months	0.06	0.04	0.68
Expect to work within 12 months	0.09	0.05	0.78
Expect to work after 12 months	-0.26	0.09	-1.07
Expect to work after 24 months	0.03	0.07	0.18
Does not expect to work in foreseeable future	-0.05	0.04	-0.52

Continued

Table B.1   Continued

	Coefficient	Marginal effects	Standard errors of marginal effects
Professional Occupations	0.26	0.06	1.74
Ass Prof and Tech Occupations	-0.08	0.04	-0.72
Admin and Secretarial Occupations	-0.08	0.04	-0.77
Skilled Trades Occupations	0.02	0.04	0.19
Less than three months health problem	-0.15	0.05	-1.25
Between three and six months health problem	-0.07	0.03	-0.82
Between six and 12 months health problem	-0.12	0.03	-1.52
Work impact on health: little worse	-0.18	0.04	-1.90
Work impact on health: a lot worse	-0.28	0.04	-2.97
Work impact on health: no	0.01	0.05	0.08
Work impact on health: don't know	0.06	0.03	0.72
More than £20,000 pa income	0.07	0.03	0.97
Never Had Paid Work	0.08	0.05	0.66
First start	0.00	0.00	-0.54
Number of days from last hospital admission to PTW start	0.00	0.00	1.66
Last hospital admission unknown	0.06	0.03	0.74
Benefit claim recorded?	0.01	0.03	0.11
Initial WFI recorded?	0.54	0.08	2.62
Initial WFI booked?	-0.42	0.06	-2.85
Initial WFI deferred?	-0.27	0.05	-2.28
Initial WFI waived?	-0.43	0.07	-2.26
Days to initial WFI	0.00	0.00	-0.69
Days to initial WFI (2)	0.37	0.19	0.74
Person in JSA in last year	0.19	0.02	3.40
Person IS in last year	-0.13	0.03	-1.86
Person IS with disability premium in last year	0.14	0.03	1.70
Getting IS enhanced disability premium in last year	-0.77	0.11	-1.97
Getting IS severe disability premium in last year	0.57	0.10	1.94
Getting IS carer premium in last year	0.13	0.10	0.49
IB DLA care type 1	-0.17	0.10	-0.64
IB DLA care type 2	0.06	0.10	0.22
IB DLA care type 3	-0.07	0.11	-0.24
IB DLA mob1	-0.03	0.06	-0.17
IB DLA mob2	-0.16	0.06	-0.99

Continued

Table B.1   Continued

	Coefficient	Marginal effects	Standard errors of marginal effects
Getting IB last year	0.01	0.03	0.13
Person IB dep add in last year	0.39	0.19	0.79
Person IB not credited in last year	0.10	0.03	1.56
Person IB DLA care in last year	-0.02	0.10	-0.08
Age	-0.01	0.00	-1.45
Age between 18 and 25	-0.10	0.04	-0.99
Over 50 years old	0.01	0.04	0.09
Male	0.06	0.09	0.25
Sex unknown	-0.13	0.10	-0.50
child16_d	-0.12	0.02	-2.15
child1618_d	0.03	0.04	0.33
childdep_d1	-0.25	0.06	-1.68
childdep_d2	-0.18	0.07	-0.95
childdep_d4	-0.72	0.09	-2.46
childdep_d5	-0.12	0.07	-0.71
Single	-0.06	0.06	-0.37
Married or living as married	0.00	0.06	-0.01
Divorced	-0.01	0.06	-0.07
Married but separated	-0.07	0.07	-0.40
Live with partner	-0.05	0.04	-0.50
Private residence	0.00	0.10	-0.01
In accommodation for less than a year	-0.07	0.06	-0.49
In accommodation from one year to ten years	-0.01	0.03	-0.20
Lived more than ten years in accommodation	-0.07	0.02	-1.27
White	0.54	0.25	0.75
Black Caribbean	0.72	0.25	0.92
Black African	0.51	0.31	0.58
Black Other	0.19	0.30	0.25
Indian	0.30	0.29	0.41
Pakistani	0.88	0.27	0.94
Bangladeshi	1.39	0.17	1.37
Chinese	0.54	0.26	0.74
Mixed/Other	0.71	0.29	0.80
Prefer not to say ethnicity	0.97	0.23	1.13
Jobcentre district – Bridgend	0.17	0.04	1.87
Jobcentre district – Derbyshire	0.01	0.04	0.09
Jobcentre district – Somerset	-0.04	0.04	-0.35

Continued

Table B.1   Continued

	Coefficient	Marginal effects	Standard errors of marginal effects
Jobcentre district – East Lancs	-0.10	0.04	-1.07
Jobcentre district – Gateshead	0.07	0.04	0.77
Jobcentre district – Essex	-0.08	0.04	-0.87
Tranche 2	0.06	0.04	0.65
Tranche 3	0.18	0.06	1.19
Higher degree (ma, Msc, PhD)	-0.34	0.27	-0.48
Degree (e.g. BA, BSC)	-0.40	0.25	-0.57
Diploma in higher education	-0.61	0.23	-0.87
Matriculation, school certification or higher	-0.48	0.25	-0.66
GCE A level	-0.43	0.25	-0.62
A/S level	-0.24	0.28	-0.32
GCE O level	-0.68	0.23	-0.98
GCSE grades A-C	-0.62	0.24	-0.89
GCSE grades D-G	-0.53	0.24	-0.77
CSE grade 1	-0.53	0.24	-0.76
CSE grades 2-5/ungraded	-0.48	0.25	-0.68
SCE/SLC/supe higher grade	-0.29	0.30	-0.36
Supe/slc lower or ordinary grad	-0.35	0.30	-0.43
SCE grades a-c or 1-3	-0.31	0.29	-0.40
SCE grades d or e or 4	-0.26	0.34	-0.29
Other degree	-0.52	0.27	-0.74
Has passed an examination	-0.30	0.05	-2.26
Left FTE before 16	-0.03	0.04	-0.36
Left FTE after 18	-0.08	0.06	-0.58
Female who left FTE before 16	-0.07	0.05	-0.55
Female who left FTE after 18	0.19	0.08	0.93
Female aged 50 +	-0.07	0.04	-0.71
White female	0.05	0.09	0.24
Health assessment	0.00	0.00	0.54
Depression	0.25	0.02	4.17
Mental problems	0.13	0.03	1.50
Drug or alcohol problems	-0.08	0.06	-0.51
Pain	0.07	0.03	1.06
Back or neck problems	0.18	0.03	2.46
Health problem with hand and legs	-0.02	0.03	-0.22
Other progressive illness	-0.01	0.04	-0.09
Internal illness	0.03	0.04	0.38
Problem to see	0.15	0.03	1.88

Note: Marginal effect is evaluated at the mean of the continuous independent variables and at the value 0 for the discrete variables.

Appendix C
Estimation of components of choices using participation in Choices from administrative sources

Table C.1 **Differences in outcomes between NDDP participants and non-participants (treatment from administrative sources)**

Outcome variables	Sample	NDDP	Non-Choices	Difference	S.E.	T-stat
In work	Unmatched	51.31	32.56	18.75***	2.54	7.39
	Matched	51.31	34.91	16.40***	3.32	4.94
Earnings	Unmatched	99.33	63.45	35.88***	7.34	4.89
	Matched	99.33	73.19	26.14***	9.44	2.77
Exit rate from IB 6 months	Unmatched	23.32	21.95	1.37	2.21	0.62
	Matched	23.32	23.70	-0.38	3.03	-0.13
Exit rate from IB 12 months	Unmatched	47.01	33.86	13.15***	2.55	5.16
	Matched	47.01	36.95	10.07***	3.30	3.05
Exit rate from IB 18 months	Unmatched	53.73	38.42	15.31***	2.60	5.90
	Matched	53.73	41.38	12.35***	3.31	3.73
Exit rate from IB 24 months	Unmatched	58.21	41.77	16.44***	2.61	6.30
	Matched	58.21	44.56	13.65***	3.33	4. 10
Exit rate from IB 30 months	Unmatched	61.38	44.93	16.45***	2.61	6.29
	Matched	61.38	47.68	13.70***	3.25	4.22
Bad health	Unmatched	25.37	38.88	-13.51***	2.49	-5.42
	Matched	25.37	35.68	-10.30***	3.55	-2.90
Good health	Unmatched	36.57	28.28	8.29***	2.44	3.40
	Matched	36.57	31.47	5.10	3.88	1.31
Better health	Unmatched	20.15	15.16	4.99**	1.98	2.52
	Matched	20.15	16.10	4.05*	2.31	1.76
Worse health	Unmatched	23.13	27.72	-4.59**	2.32	-1.97
	Matched	23.13	25.24	-2.11	2.89	-0.73

Note: kernel matching, standard errors obtained by bootstrapping (500 repetitions).
*** indicates significant estimation at the 1% level, ** at the 5% level, * at the 10% level.

Table C.2   Differences in outcomes between CMP participants and non-participants (treatment from administrative sources)

Outcome variables	Sample	CMP	Non-Choices	Difference	S.E.	T-stat
In work	Unmatched	31.38	32.26	-0.88	2.45	-0.36
	Matched	31.38	25.95	5.43*	2.72	1.99
Earnings	Unmatched	53.36	62.79	-9.43	6.80	-1.39
	Matched	53.36	50.66	2.70	8.06	0.34
Exit rate from IB 6 months	Unmatched	12.84	21.78	-8.94***	2.04	-4.38
	Matched	12.84	18.87	-6.03***	2.25	-2.68
Exit rate from IB 12 months	Unmatched	28.44	33.55	-5.11**	2.44	-2.09
	Matched	28.44	30.02	-1.58	2.66	-0.59
Exit rate from IB 18 months	Unmatched	35.23	38.05	-2.82	2.54	-1.11
	Matched	35.23	35.20	0.03	2.80	0.01
Exit rate from IB 24 months	Unmatched	40.18	41.36	-1.18	2.58	-0.46
	Matched	40.18	38.79	1.39	2.90	0.481
Exit rate from IB 30 months	Unmatched	44.40	44.67	-0.27	2.61	-0.10
	Matched	44.40	42.94	1.46	2.98	0.492
Bad health	Unmatched	40.00	39.06	0.94	2.57	0.37
	Matched	40.00	44.97	-4.97*	2.90	-1.71
Good health	Unmatched	22.39	28.13	-5.74**	2.30	-2.49
	Matched	22.39	21.91	0.48	2.62	0.18
Better health	Unmatched	20.37	15.26	5.11***	1.97	2.60
	Matched	20.37	15.13	5.24**	2.47	2.12
Worse health	Unmatched	24.22	27.48	-3.26	2.31	-1.41
	Matched	24.22	29.46	-5.24*	2.88	-1.82

Note: kernel matching, standard errors obtained by bootstrapping (500 repetitions).
*** indicates significant estimation at the 1% level, ** at the 5% level, * at the 10% level.

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