

Working Paper

In-work support for lone parents:

Using the UK ERA demonstration to examine cross-office variation in effectiveness

by Richard Dorsett and Philip K. Robins

Department for Work and Pensions

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Abbreviations

ASA	Advancement Support Adviser
DWP	Department for Work and Pensions
ERA	Employment Retention and Advancement programme
GCSE	General Certificate of Secondary Education
IS	Income Support
JSA	Jobseeker's Allowance
NDLP	New Deal for Lone Parents
OLS	Ordinary least squares
PA	Personal Adviser
RA	Random assignment
UK	United Kingdom
WFI	Work Focused Interview
WTC	Working Tax Credit

Summary

Introduction

The Employment Retention and Advancement (ERA) demonstration tested the effectiveness of an innovative method of improving the labour market prospects of low-wage workers and long-term unemployed people. ERA took place from October 2003 to October 2007 in six Jobcentre Plus districts within Great Britain. Participants in ERA had access to a distinctive set of ‘post-employment’ job coaching and financial incentives, which were added to the job placement services that unemployed people could normally receive through Jobcentre Plus. Once employed, ERA participants could receive up to two years of advice and assistance from an Advancement Support Adviser (ASA) to help them continue working and advance in work. Those who consistently worked full time could receive substantial cash rewards, called ‘retention bonuses’. Participants could also receive help with tuition costs and cash rewards for completing training courses while employed.

The ERA demonstration targeted three groups of disadvantaged people: out of work lone parents receiving Income Support (IS), low-paid lone parents working part time and receiving Working Tax Credit, and long-term unemployed people receiving Jobseeker’s Allowance. The ERA demonstration utilised a random assignment research design, assuring unbiased estimates of the programme’s impacts in the districts where the demonstration took place.

Previous reports on the ERA demonstration have documented that the programme’s impacts vary across the six Jobcentre Plus districts participating in the demonstration, even when there are no statistically significant impacts averaged across all the districts. The purpose of the present study is to try to exploit such variation in order to determine whether certain features of the local programmes’ operations are systematically related to programme impacts.

Comparing the six districts alone would not provide statistically reliable results relating programme operations to programme impacts. However, the presence of data from over 50 individual offices within these six districts can be used to explain variation in programme impacts across the offices.¹ Of course, there must be variation in programme operations among the various offices in order to identify which practices influence programme impacts. For example, offices that emphasise immediate job placement may experience larger impacts during the five-year follow-up period than offices that emphasise longer-term employment goals through increased education and training.

The analysis is performed only for the out of work lone parent group. Lack of office-level data, statistical problems associated with identifying key relationships, and generally uninformative results prevented us from conducting a credible analysis for the other two target groups. This out of work lone parent group is of particular policy interest because over most of the five-year follow-up period no statistically significant average impacts were detected for this group on most of the outcomes studied (Hendra *et al.*, 2011). Hence, if we are able to identify programme features that explain inter-office variation in the impacts for this target group we will have added to the knowledge derived from the evaluation of the ERA programme.

¹ Actually, we combine certain offices based on information pertaining to operational procedures in the various offices, yielding 37 distinct ‘offices’ for the analysis.

Findings in brief

The analysis identifies a few office practices that are systematically related to programme impacts for the out of work lone parent group. In particular, we find increased employment and reduced benefit receipt to arise in offices where a higher proportion of ERA customers (compared to control group members):

- received help with in-work advancement;
- were given support while working;
- were made aware of the employment retention bonus.

Methodological framework for explaining cross-office variation

The approach used to examine variation in impacts across offices is based on the simple notion that the implementation of ERA services within a particular office was related to a set of customer, environmental and office factors. These factors are based on customer needs and experiences and characteristics of the office managers and the ASAs administering ERA in each office. Characteristics of the office managers and ASAs include their technical qualifications (education and experience) as well as their philosophical approaches to providing services to disadvantaged persons. Also important is how these factors were used for programme group members (ERA customers) relative to control group members (New Deal for Lone Parents (NDLP) customers).

Two basic hypotheses are tested in this study. First, we test whether the strength (or effectiveness) of ERA's impacts (as opposed to the direction of impacts) is systematically related to the **intensity** of ERA services (reflected, perhaps, by the amount of time advisers spend with each ERA customer). Second, we test whether the strength of ERA's impacts is related to the **types** of ERA services provided (such as help with advancement or help with finding education and training opportunities). Both of these hypotheses are relevant for policy makers. For example, a finding that the intensity of services matters implies that hiring additional caseworkers may represent an effective use of public funds. Or, a finding that particular types of services lead to greater impacts implies that programme operators who are not currently emphasising such services might find it worthwhile to redirect their programme delivery activities towards favouring such services.

The analysis uses both individual-level data and office-level data and, therefore, a multi-level statistical framework is required. We utilise the multi-level framework developed by Bryk and Raudenbush (1992) and Bloom *et al.* (2005). The outcomes examined in this study are the number of months on IS and the number of months employed during the five-year follow-up period (roughly 2005 to 2009). Both outcomes were taken from administrative records – the DWP's Work and Pensions Longitudinal Study database. Information on IS receipt and employment status is available on a monthly basis.

Table 1 presents the means and the cross-office range of the office variables used in the multi-level analysis. The caseload averages about 29 customers per adviser and about 42 per cent of these advisers, on average, work with ERA customers. There is significant variation in the caseload across offices (from about three customers per adviser to 110 customers per adviser) and in the proportion of advisers working with ERA customers (from about 20 per cent to 94 per cent).

For each of the service type measures, Table 1 presents the mean proportion for the control (NDLP) group, the mean proportion for the programme (ERA) group, and the mean ERA-control group difference in the proportion. The first and third of these (control group value and ERA-control group difference) are used as variables in the multi-level model. The second (ERA value) is not directly

included in the multi-level model (except for the retention bonus awareness variable) and is shown for informational purposes only.

On average, for every service type, the ERA group had a higher proportion receiving that service than the control group. This is as would be expected, however the differential is not always that great. In some offices, a greater proportion of the control group received the services, as reflected in the negative minimum values of the differential in the cross-office ranges. In no office were less than three-quarters of the ERA customers aware of the retention bonuses and in some offices all of the ERA customers surveyed were aware of the bonuses.

Table 1 Office characteristics (measured during the follow-up period)

Office variable	Mean	Cross-office range	
		Minimum	Maximum
Caseload (per adviser)	29.47	3.27	109.99
Proportion of advisers working with ERA customers	0.42	0.20	0.94
Proportion of customers advised to think long term			
Control group	0.30	0.00	0.74
ERA group	0.49	0.02	1.00
ERA-control differential	0.19	-0.49	0.93
Proportion of customers receiving help finding education course			
Control group	0.43	0.00	1.00
ERA group	0.64	0.14	0.99
ERA-control differential	0.21	-0.17	0.54
Proportion of customers receiving help with in-work advancement			
Control group	0.20	0.00	0.67
ERA group	0.40	0.00	1.00
ERA-control differential	0.20	-0.69	1.00
Proportion of customers receiving support while working			
Control group	0.45	0.00	1.00
ERA group	0.65	0.21	1.00
ERA-control differential	0.21	-0.49	1.00
Proportion of ERA customers aware of retention bonuses	0.87	0.75	1.00
<i>Sample size</i>	37	37	37

Effects of office characteristics on ERA programme impacts

Table 2 shows how the office characteristics affect ERA programme impacts for the two outcomes examined in the study (months on IS and months employed over the five-year follow-up period). The effects of the office characteristics represent deviations from the average programme impacts across all offices, which are also presented in the table (grand mean impact of ERA). In addition, the table presents the interquartile range of the impacts across offices. The interquartile range is the predicted impact from the 25th percentile of the office characteristic to the 75th percentile. The interquartile range provides an indication of how the programme impacts vary across offices possessing the middle 50 per cent range of values of a particular office characteristic.

Table 2 Effects of office characteristics on office programme impacts over five years for out of work lone parents

	Coefficient	Standard error	Interquartile range across offices
Months on IS			
Grand mean impact of ERA	-0.79	0.48	-2.18 to 0.41
Proportion of advisers working with ERA customers	-4.78	3.41	-0.30 to -1.19
Proportion of customers advised to think long term	3.79	4.09	-1.23 to -0.36
Proportion of customers receiving help finding education course	4.61	3.34	-1.65 to -0.25
Proportion of customers receiving help with in-work advancement	-7.74*	4.38	-0.18 to -1.42
Proportion of customers receiving support while working	-3.77	2.35	-0.14 to -1.33
Proportion of ERA customers aware of retention bonuses	-26.27***	8.42	0.75 to -2.19
Months employed			
Grand mean impact of ERA	0.11	0.50	-0.69 to 1.04
Proportion of advisers working with ERA customers	3.16	3.39	-0.21 to 0.37
Proportion of customers advised to think long term	-2.57	4.03	0.41 to -0.18
Proportion of customers receiving help finding education course	-5.35	3.31	1.11 to -0.51
Proportion of customers receiving help with in-work advancement	9.32**	4.39	-0.62 to 0.87
Proportion of customers receiving support while working	4.32*	2.37	-0.64 to 0.73
Proportion of ERA customers aware of retention bonuses	15.86*	8.24	-0.82 to 0.95

Notes: Interquartile range is the predicted impact from the 25th percentile of the office characteristic to the 75th percentile.

*Significant at 10 per cent level.

**Significant at 5 per cent level.

***Significant at 1 per cent level

As indicated in the table, three of the six office characteristics are estimated to be significantly related to ERA lone parent programme impacts, although in one case significance does not exist for both outcomes. First, in offices where ERA customers were given help with in-work advancement, the average programme group member spent eight fewer months on IS and nine more months in employment over the five-year follow-up period than in offices where ERA customers were not given such help. More precisely, if 100 per cent of ERA customers in these offices received help with in-work advancement and no control group members received this help, ERA would have reduced the amount of time spent on IS by a further eight months relative to the impact in an office where there is no difference in the proportion of ERA versus control group members receiving this kind of help. Or, to put it another way, an individual in an office with a ten percentage point higher proportion of ERA customers versus control group members receiving help with in-work advancement will have .8 fewer months on IS than an individual in an office where there is no such difference in the proportion receiving this help. The information on interquartile ranges is very important because, in practice, few of the differences in receiving this kind of help were very large, so the effect translates to only about a 1.2 month interquartile range across offices in the impact on IS receipt and about a 1.5 month interquartile range across offices in the impact on months employed.

Second, in offices where all ERA customers received support while working during the follow-up period, an average of about four months fewer were spent on IS and about four months more employed than in offices where no ERA customers received such help (although the effect on IS is not statistically significant). As was the case for in-work advancement, the cross-office variation in the programme-control group differential in the proportion getting this help is not very great, so the effect translates to only about a 1.2 month interquartile range across offices in the impact on IS receipt and a 1.4 month interquartile range across offices in the impact on months employed.

Finally, the strongest estimated effect on programme impacts is given by the ERA customers' awareness of the employment retention bonus. In offices where all ERA customers were aware of the bonus, the coefficient implies that they would have worked 16 more months and would have spent 26 fewer months on IS than in offices where no ERA customers were aware of the bonus. This effect illustrates the integral part played by the financial bonus as part of the overall ERA programme design. Fortunately, almost all ERA customers were aware of the bonus (no office had fewer than 75 per cent aware), so while the bonus was apparently an important part of the ERA programme design, it translated into a moderately small interquartile range of ERA programme impacts across offices (about three months for the interquartile range of impacts on IS and about two months for the interquartile range of impacts on employment).

Though not statistically significant, two additional results in Table 2 are worth mentioning. It appears that in offices where ERA customers received help in finding education courses and thinking long term, they are estimated to have spent, if anything, more months on IS and fewer months in employment than in offices where ERA customers did not receive such help. As was the case for the control group under the traditional NDLP programme, such a result may have more favourable long-term benefits if the human capital investment stimulated by the adviser intervention eventually leads to more stable employment and higher earnings beyond the follow-up period. Additional data beyond the current follow-up period would be needed to shed light on this possibility.

Conclusions and policy implications

For out of work lone parents, the ERA demonstration did not have statistically significant impacts over the five-year follow-up period on the two outcomes examined in this study (months receiving IS and months employed). However, there is some evidence that the impacts of ERA on these outcomes varied across the offices participating in the demonstration.

The findings from this study provide previously unavailable evidence to policy-makers concerned with encouraging employment among lone parents on welfare. Most obviously, they point to the ingredients likely to be important when designing future in-work support programmes. They also suggest which elements may be less effective. In line with the finding in the ERA final report that ERA increased training participation without leading to an overall effect on employment, the results of this analysis suggest that the impact of ERA does not vary according to the extent to which offices emphasised training and planning for the long term. It is, of course, possible that the additional human capital resulting from training will prove to be beneficial in the long run. Definitive evidence on whether it eventually translates into more favourable long-run outcomes requires additional data beyond the five years examined in this report.

In conclusion, it is relevant to mention that, as with any long-term study, the economic and policy environment changes. Most obviously, the results relate to a period marked by severe recession and associated increases in unemployment. Equally relevant though is the fact that the last few years have seen a number of policies introduced that directly affect lone parents in the United Kingdom. Lone parents have been increasingly required to attend work-focused interviews and those with a youngest child aged seven or over now have to actively seek work. Furthermore, In-Work Credit was introduced in 2008, providing weekly subsidies to lone parents entering work of 16 or more hours per week. The effect of such policy developments is to reduce the contrast between the service available to the ERA group and that available to the control group and has an important bearing on how to view the overall effect of ERA. However, despite this, the analysis has still been successful in identifying those particular implementation features that tended to strengthen the impact of ERA.

1 Introduction

1.1 The ERA demonstration

The United Kingdom (UK) Employment Retention and Advancement (ERA) demonstration tested the effectiveness of an innovative method of improving the labour market prospects of low-wage workers and long-term unemployed people. ERA took place from October 2003 to October 2007 in six Jobcentre Plus² districts throughout Great Britain.³ Participants in ERA had access to a distinctive set of ‘post-employment’ job coaching and financial incentives, which were added to the job placement services that unemployed people could normally receive through Jobcentre Plus. Once employed, ERA participants could receive up to two years of advice and assistance from an employment adviser to help them continue working and advance in work. Those who consistently worked full time could receive substantial cash rewards, called ‘retention bonuses’. Participants could also receive help with tuition costs and cash rewards for completing training courses while employed.

The ERA demonstration targeted three groups of disadvantaged people: out of work lone parents receiving Income Support (IS), low-paid lone parents working part time and receiving Working Tax Credit (WTC), and long-term unemployed people receiving Jobseeker’s Allowance (JSA). The ERA demonstration utilised a random assignment research design, assuring unbiased estimates of the programme’s impacts in the districts where the demonstration took place.⁴

The ERA final report (Hendra *et al.*, 2011) covers five years of programme impacts. Previous reports have examined impacts after one year (Dorsett *et al.*, 2006) two years (Riccio, *et al.*, 2008; Miller *et al.*, 2008) and four years (Dorsett and Robins, 2010, unpublished). The evaluation uses data from administrative records for the full sample and survey information collected for subsamples. In the ERA final report, administrative records are used to document impacts on several outcomes (mainly employment and benefit receipt) during the five years subsequent to random assignment. For two of the three target groups (out of work lone parents and WTC recipients), the impacts are generally quite modest and not statistically significant for most of the evaluation period. For the other target group (long-term JSA recipients), the impacts are statistically significant and sizeable, and persist into the post-programme period.

1.2 Local variation in impacts

Previous reports on the ERA demonstration have documented that the programme’s impacts vary across the six Jobcentre Plus districts participating in the demonstration, even when there are no statistically significant impacts averaged across all the districts. The purpose of this paper is to try to exploit such variation in order to determine whether certain features of the local programmes’ operations are systematically related to programme impacts. As with the ERA final report, five years of follow-up data are used.

² Jobcentre Plus is an agency of the UK Department for Work and Pensions (DWP) that provides help and advice on employment and training for people who can work and financial support for those of working age who cannot work.

³ These districts were located in the following regions: East Midlands, London, North East England, North West England, Scotland, and Wales.

⁴ In the evaluation literature, unbiased estimates in places where a random assignment demonstration takes place are commonly referred to as ‘internally valid’ estimates.

The results from this analysis can provide important guidance to policy-makers concerned with encouraging lone parents to move from welfare into sustained employment. The ERA evaluation was designed to comprehensively assess the combined effect of financial incentives and adviser support on subsequent labour market outcomes. In practice, this could be operationalised in a range of ways, with offices differing in the emphasis they attached to particular elements of this overall ‘package’. The analysis in this report explores the extent to which identifiable elements of this package appear to influence programme effectiveness. Its practical usefulness lies in showing which elements of ERA appear most effective and, conversely, which elements appear to work less well in helping lone parents achieve sustained employment. Such information could prove helpful for designing similar support programmes in the future.

Comparing the six districts alone would not provide statistically reliable results relating programme operations to programme impacts. However, the presence of data from over 50 individual offices within these six districts **can** be used to explain variation in programme impacts across the offices.⁵ Of course, there must be variation in programme operations among the various offices in order to identify which practices influence programme impacts. For example, offices that emphasise immediate job placement may experience larger impacts during the five-year follow-up period than offices that emphasise longer-term employment goals through increased education and training.

1.3 Approach to examining cross-office variation in impacts

The analysis uses a multi-level statistical model based on the methodology developed by Bryk and Raudenbush (1992) and first applied to the evaluation of social experiments by Bloom *et al.* (2005). Multi-level models use both individuals and institutions as the units of analysis and are quite appropriate for examining variation in programme impacts across offices. Other studies using a somewhat different methodology to exploit variation in office practices to estimate social programme impacts include Dehejia (2003) and Galdo (2008).

The analysis focuses on only one of the target groups participating in ERA, namely out of work lone parents receiving IS.⁶ This out of work lone parent group is of particular interest because over most of the five-year follow-up period no statistically significant average impacts were detected for this group on most of the outcomes studied (Hendra *et al.*, 2011). Hence, if we are able to identify programme features that explain inter-office variation in the impacts for this target group we will have added to the knowledge derived from the evaluation of the ERA programme.

1.4 Findings in brief

Our analysis identifies a few office practices that are systematically related to programme impacts for the out of work lone parent group. In particular, we find that an emphasis on in-work advancement, support while working and the employment retention bonus results in greater impacts on employment and benefit receipt. On the other hand, encouraging individuals to focus on long-term aims or encouraging education appears to do little to influence impacts over the period considered.

⁵ Actually, as will be described below, we combine certain offices based on information pertaining to operational procedures in the various offices, yielding 37 distinct ‘offices’ for the analysis.

⁶ Lack of office-level data, statistical problems associated with identifying key relationships, and generally uninformative results prevented us from conducting a credible analysis for the other two target groups.

Although not the primary objective of the analysis, we extend the Bloom *et al.* (2005) model to also examine how office characteristics influence control group outcomes, which reflect the status quo environment (the existing New Deal for Lone Parents (NDLP) programme). Allowing office characteristics to affect control group outcomes increases the explanatory power of the model and helps to make the key relationships more precisely estimated. We find that some of the same office characteristics are also systematically related to control group outcomes. In particular, we find that in offices where advisers had smaller caseloads there was greater employment and less benefit receipt among NDLP (control group) customers. Furthermore, in offices where a greater proportion of control group customers received help with in-work advancement there was greater employment and less benefit receipt among NDLP customers. Finally, in offices where there was a higher proportion of NDLP customers receiving help in finding education courses, employment was lower and benefit receipt was higher.

Finally, our approach enables us to examine variation in programme impacts with customer and environmental characteristics, in addition to office characteristics. Variation in impacts with customer characteristics is commonly referred to as ‘subgroup’ impacts. Among the large number of subgroups examined, we find that ERA increased employment and/or reduced IS receipt for those lone parents:

- with A-level⁷ qualifications (compared to those with no qualifications);
- 40 years of age and older (compared to those less than 30 years old); and
- living in more deprived areas (compared to those in less deprived areas).

1.5 Outline of this report

The remainder of this report proceeds as follows. In Chapter 2 we describe the ERA demonstration and what it was intended to accomplish. In Chapter 3 we present the hypotheses to be tested in examining cross-office variation in ERA impacts. In Chapter 4 we present the statistical model used to test these hypotheses. In Chapter 5 we discuss the data used to estimate the statistical model. In Chapter 6 we report our estimation results. Finally, in Chapter 7, we present our conclusions and policy recommendations.

⁷ ‘A-level’ is used throughout as a shorthand for ‘A-level or higher’ and therefore identifies those individuals whose qualification level is at least as high as that obtained on leaving school at age 18.

2 The policy setting

2.1 Going beyond the New Deal for Lone Parents

The Employment Retention and Advancement (ERA) demonstration builds on the New Deal for Lone Parents (NDLP) policy initiative introduced in the United Kingdom in 1998.⁸ NDLP's aim was to *'encourage lone parents to improve their prospects and living standards by taking up and increasing paid work, and to improve their job readiness to increase their employment opportunities'* (Department for Work and Pensions 2002, cited in Dolton *et al.*, 2006). NDLP customers were assigned a Personal Adviser (PA) through Jobcentre Plus to provide pre-employment job coaching services. PAs could also offer job search assistance and address any barriers customers might have had that challenged their search for work. They also had access to an Adviser Discretion Fund that provided money to help customers find employment. Finally, they advised customers on their likely in-work income at differing hours of work and helped them access education or training. All of the services provided under NDLP were voluntary.

The ERA demonstration project offered services beyond those available under NDLP, mainly in the form of in-work services and financial support. For the lone parent group analysed in this report, the additional services included coaching on advancement in a current job and/or finding a better job and rapid re-employment services when necessary. ERA services were provided by an Advancement Support Adviser, a specially trained person assigned to ERA customers. In addition, ERA provided a series of in-work retention bonuses to encourage work. Support for training was also available. ERA also covered tuition costs and offered financial incentives to train while in work. Lastly, it provided an in-work Emergency Discretion Fund designed to cover small financial emergencies that otherwise could threaten the individual's continued employment.⁹

2.2 What was expected from ERA

Because both NDLP and ERA services were voluntary, it was possible that in addition to providing in-work assistance, more ERA customers made use of pre-employment services than they would have in the absence of the ERA programme. Thus, in addition to receiving ERA services and financial support, customers might also have made greater use of traditional NDLP services. Because it was a demonstration, ERA services and financial assistance were available for only 33 months.

⁸ NDLP officially ended on 31 March 2011. All of the pre- and in-work support previously available under the NDLP programme (except for the Training Premium) remains available. This means that at the discretion of a Jobcentre Plus PA and as part of the Get Britain Working initiative, support is available to all lone parents who agree to a personalised return to work plan with their PA. Lone parents on Income Support in England will also be able to volunteer for the new Work Programme, where they will receive their return to work support from a provider instead of Jobcentre Plus.

⁹ The retention bonus consists of up to six payments of £400 for each period when customers work 30 or more hours per week for 13 out of 17 weeks. The training bonus consist of tuition payments up to £1,000 for in-work training plus £8 for every hour of training completed up to £1,000.

In order to evaluate the impacts of the multi-dimensional ERA programme, a random assignment research design was utilised. Customers who volunteered for ERA were randomly assigned to a programme group that was eligible for the full range of ERA services and financial assistance, and a control group that could only receive standard NDLP services. Because there were no systematic differences between the two groups prior to random assignment, average programme impacts (that is, differences in behaviour caused by the programme) could be identified by a simple comparison of mean outcomes of programme and control group members.

If the randomisation was implemented properly, these comparisons would be unbiased and internally valid. In other words, if ERA was implemented universally in the same districts, the expected impacts of the universal programme would be the same as the impacts derived from the demonstration. It is important to note that the impacts defined in this way are responses to the **offer** of ERA services and financial assistance, and not the actual receipt of such services or financial assistance, because the programme was voluntary and not all programme group members made use of what ERA offered.¹⁰ Furthermore, while there is a distinction between those using and not using ERA services and financial assistance, there are also considerable differences in the type and intensity of services and financial assistance received among those making use of ERA.

¹⁰ In the language of programme evaluation, we examine impacts on the **eligible** population (those offered the treatment) and not the **treated** population (those receiving the treatment).

3 Factors influencing variation in ERA's impacts

3.1 Measuring the impacts of ERA

The simplest measure of the **impact** of Employment Retention and Advancement (ERA) is the difference in mean outcomes between the programme and control groups over the follow-up period (five years in this report).¹¹ The two outcomes examined in this report are months receiving IS and months employed. The impact of ERA on months receiving Income Support (IS), for example, is the **difference** over the follow-up period between the programme and control groups in the average number of months receiving IS. The follow-up period for ERA is five years, roughly three of which are while the programme was operating and two are after the programme ended. Thus, the impact measured over the five-year period captures both short- and longer-run effects of the programme.

3.2 Reasons why the impacts of ERA might have varied

There are four main reasons why the impacts of ERA might have varied. Firstly, the impacts might have varied over time. For example, if it took time to fully implement ERA, programme group members may have received similar services to control group members in the early years and programme impacts would have been small, or possibly non-existent. Over time as ERA became fully implemented with programme group members receiving services not available to control group members, impacts might have become larger. Furthermore, individuals may have taken time to respond to ERA, particularly if the services offered by ERA were different from what they had been experiencing prior to ERA.

Secondly, individuals participating in ERA may have responded differently based on their characteristics. For example, those with longer histories of IS receipt or lower levels of education may have been harder to employ and less likely to have been able to use the ERA services effectively than persons with shorter histories of IS receipt or higher levels of education. On the other hand, those with older children may have been more willing to utilise the ERA services than persons with younger children.

Thirdly, environmental characteristics may have caused differences in the impacts of ERA. Persons living in areas with higher unemployment or, generally, in more deprived areas may have found it harder to have made effective use of ERA services.

Finally, the impacts of ERA may have varied because of differences in the way ERA was implemented in the various local offices. For example, offices with higher caseloads may have been less successful in providing meaningful help to ERA customers, thereby rendering ERA less effective.

3.3 Office variation in impacts

Office variation in impacts according to the way ERA was implemented is the major focus of this paper, although we also examine how impacts vary over time with personal characteristics of the

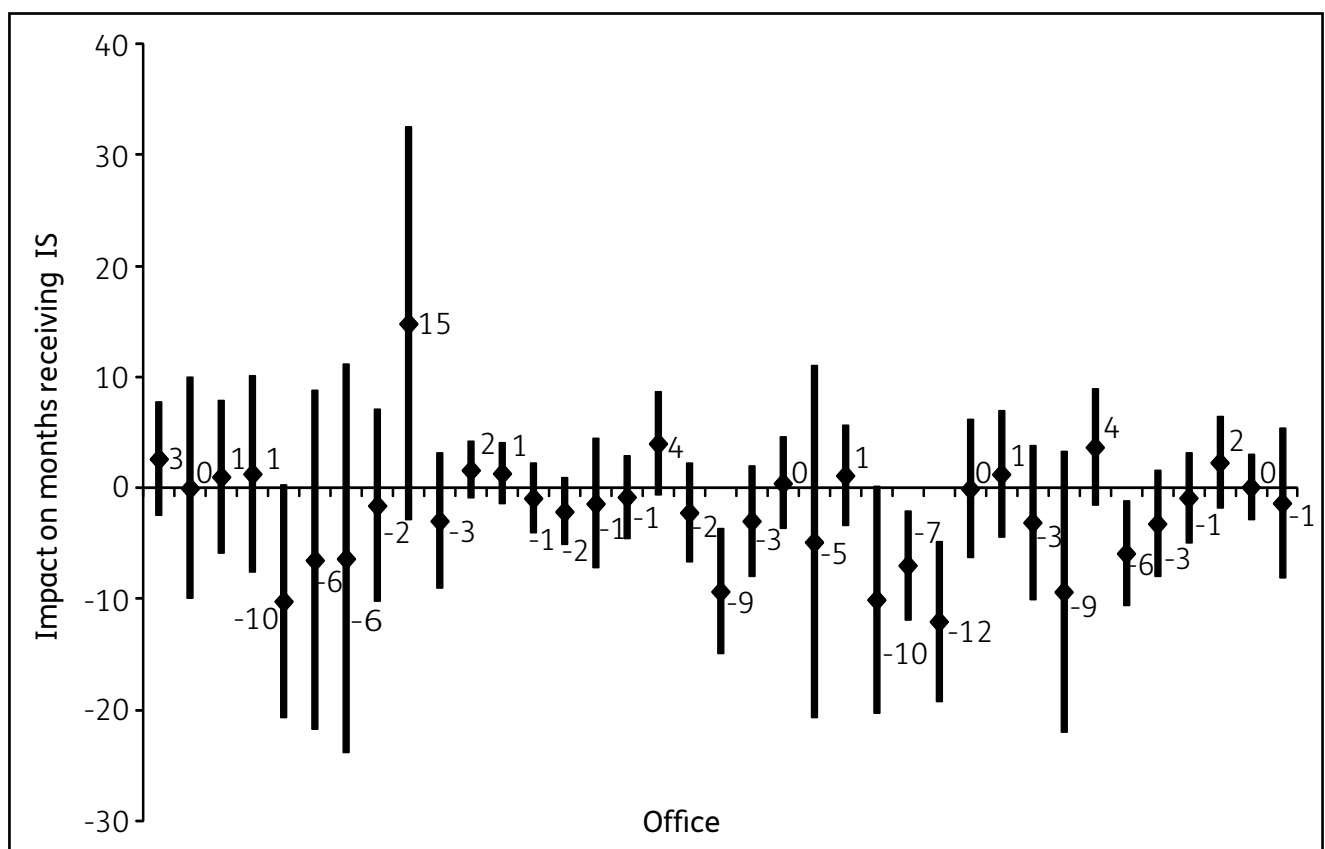
¹¹ In practice, this impact is usually 'regression adjusted', meaning that the influences of variations in customer characteristics at the time of random assignment are taken into account in deriving the impact. Regression adjustment makes the estimated impact more statistically precise.

customers and with environmental characteristics. Introducing office-level variation in impacts requires a more sophisticated statistical framework than is traditionally used in evaluation research. Specifically, the units of analysis are both the individual and the office, and the statistical framework must take this nesting into account. As will be described in greater detail below, multi-level modelling provides a natural framework for analysing variation in impacts across offices and across individuals within offices.

Before proceeding with the specification and estimation of a multi-level statistical model however, a fundamental question must be answered. Namely, is there enough variation in the impacts of ERA across offices so that implementation differences can possibly be explained by office-level characteristics? To determine this, we estimated a simple (traditional) ordinary least squares regression model in which we allowed the impacts of ERA on months receiving IS and months employed to be different in each of the 37 offices in our sample. We estimated models for the impacts over all five follow-up years as well as models for the impacts in each of the five follow-up years. We conducted formal statistical tests to determine whether the individual impacts were significantly different from the average impact estimated over all offices.

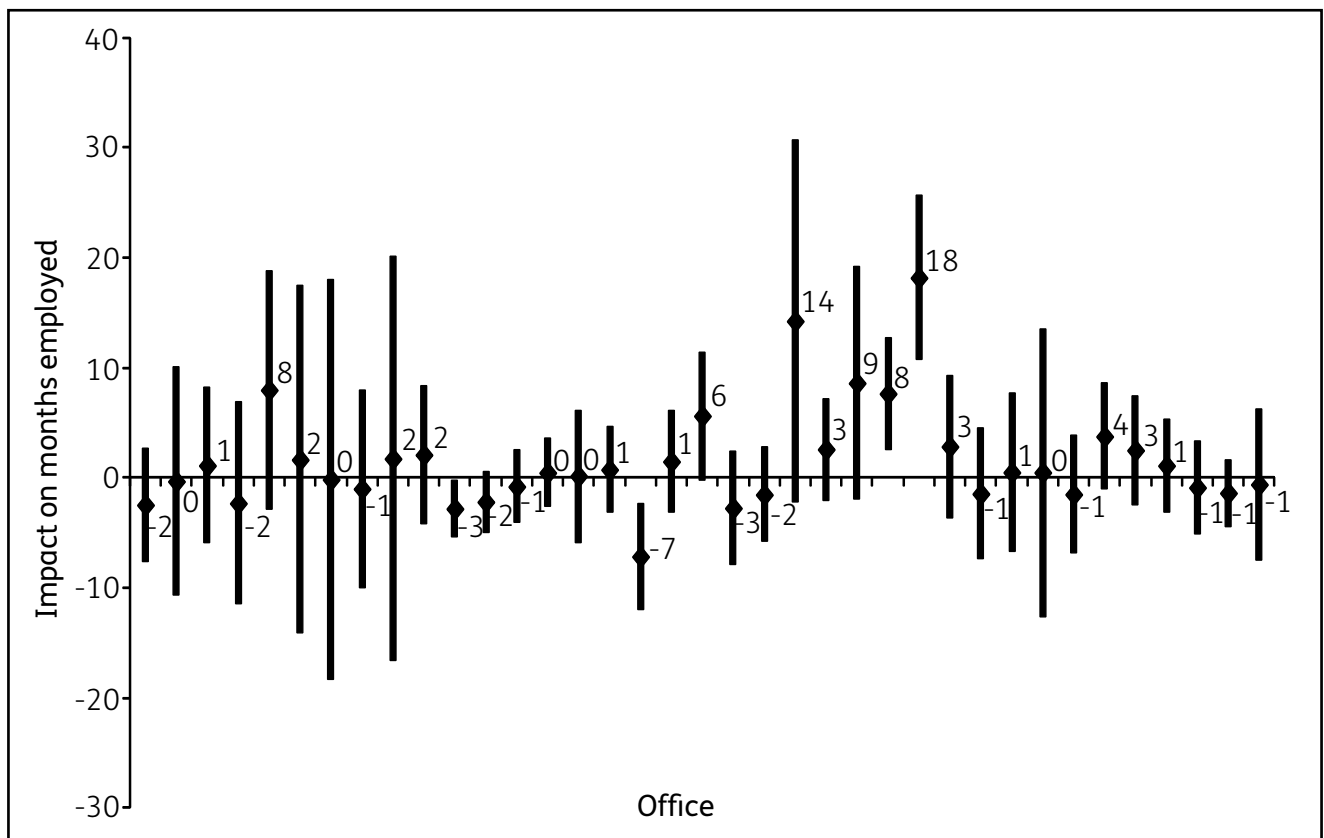
Figures 3.1 and 3.2 present the estimated office impacts during the five-year follow-up period along with 90 per cent confidence intervals for these impacts.¹² If the confidence interval doesn't cross the zero impact line, then the estimated impact is statistically significant at the ten per cent level.

Figure 3.1 Office-level impacts on months receiving IS, with ten per cent confidence intervals



¹² Results for each of the individual follow-up years are available from the authors on request. A 90 per cent confidence interval is a statistical calculation that says there is a 90 per cent probability that the 'true' impact of ERA in the office falls within that interval.

Figure 3.2 Office-level impacts on months employed, with ten per cent confidence intervals



As indicated in the figure, the IS impacts (Figure 3.1) range from -12 months to +15 months, but only four of the 37 estimates are significantly different from zero. The employment impacts (Figure 3.2) range from -7 months to +18 months, with again only four of the estimate being significantly different from zero. The formal test of statistical significance for the variation in impacts across offices is statistically significant for employment($p=.07$), but is not statistically significant for IS ($p=.14$). However, the variation in IS impacts is statistically significant in year two of the five follow-up years (not shown in the figure). Based on these results, we conclude that there is sufficient variation in the impacts across offices to warrant a further, more sophisticated, analysis to determine whether part of the variation can be explained by office characteristics.

4 Methodological framework for explaining cross-office variation

4.1 General approach

Our fundamental approach for examining variation in impacts across offices is based on the simple notion that the implementation (or production) of Employment Retention and Advancement (ERA) services within a particular office was related to a set of customer, environmental and office factors (or inputs). These factors are based on customer needs and experiences and characteristics of the office managers and the Advancement Support Advisers (ASAs) administering ERA in each office. Characteristics of the office managers and ASAs include their technical qualifications (education and experience) as well as their philosophical approaches to providing services to disadvantaged persons. Also important is how these factors were used for programme group members (ERA customers) relative to control group members (New Deal for Lone Parents (NDLP) customers).

4.2 Hypotheses tested

In examining variation in ERA impacts across offices, we focus on ERA services that are consistent with the primary objectives of the demonstration, namely retention and advancement services. Two basic hypotheses will be tested (the specific variables related to each of these hypotheses are described in detail below). First, we hypothesise that the strength (or effectiveness) of ERA's impacts (as opposed to the direction of impacts) will be systematically related to the **intensity** of ERA services (reflected, perhaps, by the amount of time advisers spend with each ERA customer). Second, we hypothesise that the strength of ERA's impacts will be related to the **types** of ERA services provided (such as help with advancement or help with finding education and training opportunities). Both of these hypotheses are relevant for policy-makers. For example, if it is found that the intensity of services matters, then hiring additional caseworkers may represent an effective use of public funds. Or, if it is found that particular types of services lead to greater impacts, then programme operators who are not currently emphasising such services might find it worthwhile to redirect their programme delivery activities towards favouring such services.

For both the above hypotheses, the direction of impacts (as opposed to the strength or effectiveness) will depend on the nature of the ERA service. If, for example, the service emphasises longer-term outcomes beyond the follow-up period (such as encouraging investment in human capital through additional take-up of education and/or training), the impact on months of employment during the follow-up period may be negative and the impact on months receiving Income Support (IS) may be positive. On the other hand, if the ERA service emphasises shorter-term outcomes during the follow-up period (such as in-work advice or information about monetary benefits available from ERA), the impact on months of employment during the follow-up period may be positive and the impact on months receiving IS may be negative. From the policy-maker's perspective, negative impacts on employment and positive impacts on IS receipt during the follow-up period may be viewed as somewhat disappointing; however from the customer's perspective these may lead to better long-term outcomes, beyond the follow-up period.

It is important to keep in mind that when testing hypotheses about how the intensity and type of ERA services influences the impacts of ERA, the control group plays an important role. Many control group members received services under the NDLP programme that were similar to the services received by programme group members under ERA. The impact of ERA will be influenced by the **differential** receipt of services between programme and control group members. If control group members receive the same advancement services as programme group members, then both might potentially benefit, but the **impact** of ERA would be zero. Thus, when we measure services received by ERA programme group members in a particular office, we will always construct them as the **difference** in the receipt of those services between programme and control group members. The actual level of service receipt of control group members will influence control group (NDLP) outcomes, but not the impacts of ERA.¹³

4.3 A multi-level model

The simple ordinary least squares (OLS) regression model used to test for variation in ERA impacts across offices only requires individual-level (customer) data. The OLS model regresses the individual's outcome (months on IS or months employed) on a set of explanatory variables (customer characteristics, an indicator of whether the individual was in the programme or control group, an indicator of the office serving the individual, and a set of interactions of the programme-control indicator with the office indicator). Because we now wish to extend the analysis to determine whether the inter-office differences in impacts can be explained by particular characteristics of the offices, we add an additional level to the data, namely office-level variables.

Hence, our extended analysis uses both individual-level data and office-level data and, therefore, a multi-level statistical framework is required (see Bryk and Raudenbush, 1992 and Bloom *et al.*, 2005 for details). The basic structure of the multi-level model has the following interpretation. First, the individual-level outcomes (months on IS and months employed) are allowed to vary across individuals according to programme-control group status and individual characteristics. Programme impacts are also allowed to vary with individual characteristics, yielding what are commonly known as subgroup impacts. In addition, control group outcomes and programme impacts are allowed to vary with office characteristics indicating the intensity and type of services received. The framework allows both the control group outcomes and the programme impacts to be explained by observable and unobservable characteristics. The observable characteristics are those we can measure and the unobservable characteristics are represented by random error terms. There are three random error terms in the model in addition to the observable individual- and office-level characteristics. The first random error term captures random variation in the individual-level outcomes. The second random error term captures random variation in the average office-level outcome for control group (NDLP) customers. The third random error term captures random variation in the average office-level impact for programme group (ERA) customers. It is the separate specification of the three error terms and the inclusion of office-level characteristics as explanatory variables that distinguish the multi-level model from the more traditional regression models used in the programme evaluation literature.

¹³ Technically, control group members were not supposed to receive ERA services, but in practice control group members may have received similar type services. In fact, as will be seen, many control group members did receive ERA-like services.

4.4 Formal description of the multi-level model

The multi-level model described above has the following formal statistical structure:¹⁴

$$(1) \quad \text{Level 1: } Y_{ji} = \alpha_j + \beta_j P_{ji} + \sum_k \delta_k CC_{kji} + \sum_k \gamma_k CC_{kji} P_{ji} + \varepsilon_{ji},$$

$$(2) \quad \text{Level 2: } \alpha_j = \alpha_0 + \sum_m \zeta_m SI_{mj} + \sum_n \eta_n ST_{nj} + u_j,$$

$$\beta_j = \beta_0 + \sum_m \pi_m DSI_{mj} + \sum_n \phi_n DST_{nj} + \mu_j,$$

or, combining the equations for levels 1 and 2,

$$(3) \quad Y_{ji} = \alpha_0 + \sum_m \zeta_m SI_{mj} + \sum_n \eta_n ST_{nj} + \beta_0 P_{ji} + \sum_m \pi_m DSI_{mj} P_{ji} + \sum_n \phi_n DST_{nj} P_{ji} + \sum_k \delta_k CC_{kji} + \sum_k \gamma_k CC_{kji} P_{ji} + [u_j + \mu_j P_{ji} + \varepsilon_{ji}],$$

where:

- Y_{ji} = outcome for sample member i in office j (number of months receiving IS or number of months employed);
- P_{ji} = a binary indicator of programme status for sample member i in office j (1 for programme group members, 0 for control group members);
- CC_{kji} = the value of customer characteristic k for sample member i in office j (grand-mean-centred);¹⁵
- SI_{mj} = the value of service intensity feature m for control group members in office j (grand mean-centred);
- ST_{nj} = the value of service type feature n for control group members in office j (grand mean-centred);
- DSI_{mj} = programme-control group difference in the value of service intensity feature m for office j (grand mean-centred);
- DST_{nj} = programme-control group difference in the value of service type feature n for office j (grand mean-centred);
- α_j = control-group outcome for the average sample member in office j ;
- β_j = programme-group impact for the average sample member in office j ;
- δ_k = the effect of customer characteristic k on the control group's outcome;
- γ_k = the effect of customer characteristic k on the programme impact ($\delta_k + \gamma_k$ is the effect of customer characteristic k on the programme group's average outcome);
- α_0 = the grand mean control group outcome;
- ζ_m = the effect of the service intensity feature m on the control group's outcome;
- η_n = the effect of the service type n on the control group's outcome;

¹⁴ This model is an extension of the multi-level model developed by Bloom, Hill and Riccio (2005). For details on the statistical properties of multi-level models, see Bryk and Raudenbush (1992).

¹⁵ As will be described below, included in the set of client characteristics is an environmental variable (index of local deprivation), which is defined at a finer level than the local offices. Specifically, there are 1,676 distinct values of this variable for the 6,754 individuals in our sample.

- β_0 = the grand mean impact;
- π_m = the effect of the service intensity difference feature m on the programme impact;
- ϕ_n = the effect of the service type difference n on the programme impact;
- u_j = a random error term for the control group's outcome in office j ;
- μ_j = a random error term for the programme impact in office j ;
- ε_{ji} = a random error term for the outcome of sample member i in office j .

The basic structure of this model has the following interpretation. First, the outcome y_{ji} is allowed to vary across individuals according to programme-control group status (P) and individual characteristics (CC). Programme impacts are also allowed to vary with individual characteristics, yielding subgroup impacts (represented by the γ_k). In addition, average office control group outcomes (the α_j) and the average office programme impacts (the β_j) are allowed to vary with office characteristics (SI and ST for control group members and DSI and DST for programme group members).¹⁶ Because customer and office characteristics are grand-mean centred, α_j represents the outcome an average control group individual would experience in local office j and α_0 represents the overall value of the outcome for the average control group member of the full sample. Similarly, β_j represents the impact ERA would have on the average programme group member in office j , while β_0 represents the average impact of ERA in the sample as a whole.

In statistically estimating the parameters of this model, we assume that the office error terms μ_j and u_j are uncorrelated with each other and with the individual random error term ε_{ji} . Although there is no theoretical justification for imposing these restrictions, the assumptions were made out of necessity because statistical convergence was not obtained or the model produced unstable results when correlated error terms were allowed.¹⁷

¹⁶ Allowing control group outcomes to vary with office characteristics represents an extension of the model presented in Bloom *et al.* (2005) which only allows office characteristics to affect programme impacts, whereas we allow office characteristics to affect both control group outcomes **and** programme impacts.

¹⁷ Because we include a substantial number of office variables in our model relative to the number of offices in the sample, there simply are not enough degrees of freedom to estimate a fully unrestricted error structure. However, given the large number of observable characteristics in the model, the assumption of uncorrelated error terms is reasonable.

5 Data

To estimate the parameters of equation (3), two kinds of data are required. First, there are the variables measured at the individual level (the outcomes, Y, and the customer characteristics, CC). Second, there are variables measured at the office level (service intensity, SI, and service type, ST). In the Employment Retention and Advancement (ERA) demonstration, there were 57 offices in the six participating Jobcentre districts. For purposes of analysis, we have combined some of the offices, particularly those in rural areas, because fieldwork and data from the office staffing forms suggested that operations among these offices were shared.¹⁸ After combining the offices with shared operations, we were left with 37 distinct ‘offices’.¹⁹ Office variables used in the analysis were derived from staffing forms and the customer surveys conducted during the follow-up period.

5.1 Outcomes

The outcomes examined in this report are the number of months on Income Support (IS) and number of months employed during the five-year follow-up period (roughly 2005 to 2009).²⁰ Both outcomes were taken from administrative records – the Department for Work and Pensions’ Work and Pensions Longitudinal Study database. Information on IS receipt and employment status is available on a monthly basis.

5.2 Customer characteristics

Individual-level background characteristics were collected as part of the randomisation process. Because they were recorded prior to the onset of ERA, these background characteristics are ‘exogenous’. By exogenous, it is meant that they are not influenced by the ERA programme in any way and hence, can be included as explanatory variables in the multi-level model without biasing the estimated programme impacts. Although, strictly speaking, it is not necessary to include exogenous individual background characteristics in the model (a successful randomisation process

¹⁸ In Northwest England/Manchester, offices worked independently and thus office units were retained. The situation was similar in Scotland with the exception of the rural Rothesay and Dunoon offices which sometimes shared advisers and were geographically proximate. As such, the two offices were combined for analysis. The Wales district was very rural and offices were in relatively close geographic proximity. Staff worked across all offices, and as such, the district was treated as one office. London was left as eight distinct offices although there was some staff sharing, particularly between Barking and Romford, Ilford and Seven Kings, Dagenham and Hornchurch, and Leytonstone and Walthamstow. Gateshead was also left with five offices: Blaydon, Jarrow, Gateshead, South Shields, and Felling. There was minimal sharing of staff across offices, primarily in South Shields. Derbyshire had a hybrid system with offices grouped by geography into subregions. As such, the district was recoded into four larger districts: Derby (including Derby Normanton, Derby St Peters, Derby Wardwick), Derby periphery (including Alfreton, Belper, Heanor, Ilkeston, Long Eaton, Matlock, Swandlincote), Chesterfield (including Bolsover, Chesterfield, Clay Cross, Shirebrook, Staveley) and Peaks (Buxton, Glossop).

¹⁹ We performed some analyses using the full 57 office sample, but the results were not as informative as the analyses performed on the combined offices sample.

²⁰ We considered examining earnings, but a formal statistical test revealed that earnings impacts did not vary significantly across offices. Furthermore, examining inter-office variation in earnings impacts raised issues of comparability in cost of living standards across offices and over time.

provides unbiased estimates of programme impacts without the need to control for background characteristics), they are included for two reasons. First, their inclusion increases the precision of the estimated programme impacts (i.e. it reduces the outcome error variance). Second, their inclusion allows us to examine whether the estimated programme impact varies across individuals with different characteristics (that is, whether there are subgroup impacts). This is accomplished by interacting the background characteristics with the programme status indicator, P .

The following customer characteristics (CC) are included in our model:²¹

- sex
- partnership status
- highest level of qualification
- number of children
- number of weeks worked in the three years prior to random assignment
- whether the individual worked in the year prior to random assignment
- weekly earnings for the most recent job in the year prior to random assignment
- quarter of random assignment
- number of months receiving IS in the two years prior to random assignment
- age of the youngest child
- age
- ethnicity.

These individual characteristics were augmented with data on local²² deprivation taken from the ERA non-participation study (Goodman and Sianesi, 2007).²³ There are 1,676 distinct values of this variable for the 6,784 individuals in our sample.

As will be discussed below, to facilitate interpretation of the estimated coefficients, all customer characteristics were grand-mean-centred (expressed as deviations from the overall mean).

²¹ A full definition of the variables (including an indication of the reference groups for comparison) is presented in Table 5.1.

²² More precisely, Super Output Areas. For details, see <http://www.neighbourhood.statistics.gov.uk/dissemination/Info.do?page=aboutneighbourhood/geography/superoutputareas/soa-intro.htm>

²³ Based on a methodology developed by the Social Disadvantage Research Centre at the University of Oxford, separate indices have been constructed for England, Northern Ireland, Scotland and Wales. Though not directly comparable, each index is based on the concept that distinct dimensions of deprivation such as income, employment, education and health can be identified and measured separately. These dimensions, sometimes referred to as 'domains' are then aggregated to provide an overall measure of multiple deprivation and each individual area is allocated a deprivation rank and score. The indices are used to help target policies and funding, and reinforce a common goal to improve the quality of life in disadvantaged communities. However, the indices may not be used together to create a single United Kingdom index.

Furthermore, each of these grand-mean-centred variables was interacted with a treatment group dummy so that subgroup effects could be estimated.²⁴

Table 5.1 presents means of the customer characteristics and outcomes used in our analysis, along with their cross-office range. As this table indicates, the sample overwhelmingly comprises female lone parents with generally low levels of qualifications. About one-half of these mothers have only one child and in about half of all cases the child is under the age of six years. More than 70 per cent of the sample did not work in the year prior to random assignment and they received IS for an average of 17 of the 24 months preceding random assignment.

Table 5.1 Customer characteristics (measured at random assignment unless otherwise noted)

	Cross-office range		
	Mean	Minimum	Maximum
Customer characteristics			
1 = Female (reference category is 'male')	0.95	0.86	0.98
1 = Never partnered (reference category is 'previously partnered')	0.72	0.37	0.86
Qualification level (reference category is 'no qualifications')			
1 = O-level qualification	0.48	0.24	0.63
1 = A-level qualification	0.22	0.09	0.64
1 = Other educational qualification	0.07	0.00	0.16
1 = More than one child in family (reference category is 'one child')	0.44	0.30	0.68
Weeks worked in past three years (reference category is 25-36)			
1 = Worked for 12 or fewer months in the past three years	0.73	0.50	0.93
1 = Worked for 13 to 24 months in the past three years	0.13	0.00	0.32
1 = Worked in the past year (reference category is 'no work in year prior to RA')	0.29	0.10	0.64
Weekly earnings in most recent job in the year before random assignment	27.17	12.65	61.09
Quarter of random assignment (reference category is July-September 2004)			
1 = Randomly assigned between October 2003-December 2003	0.10	0.00	0.29
1 = Randomly assigned between January 2004-March 2004	0.30	0.10	0.63
1 = Randomly assigned between April 2004-June 2004	0.21	0.07	0.52
Number of months receiving IS in the two years prior to random assignment	17.31	13.95	19.42
1 = Youngest child in household less than six at random assignment (reference category is 'youngest child older than six')	0.49	0.14	0.69
Customer age (reference category is 'less than 30')			
1 = Aged 30-39 years old	0.40	0.26	0.59
1 = Aged 40 plus years old	0.19	0.00	0.53
1 = Non-white (reference category is 'white')	0.15	0.00	0.63
Index of multiple deprivation	33.87	15.43	62.28
Outcome (measured over the five-year follow up)			
Months receiving IS	26.19	14.43	35.28
Months of employment	25.29	20.34	33.40
<i>Sample size (N = 6,754)</i>			

²⁴ Some of the characteristics are missing for a few individuals. We include a dummy variable in our estimated models for cases with missing values of the characteristics.

The median deprivation index in our sample is 27.4. Of the 32,482 Lower Super Output Areas in England, an index of 27.4 is for an area in approximately the 71st percentile of deprivation.²⁵ Thus, our sample is somewhat overrepresented by individuals living in relatively disadvantaged areas.

There was considerable inter-office variation in many of the characteristics, including marital status, qualifications, number and ages of children, prior work status, age and ethnicity of the customer, and the level of multiple deprivation in the community served by the office.

The average customer in our sample spent about 26 months on IS during the follow-up period (about 43 per cent of the time) and was employed for roughly the same amount of time. Of the two outcomes, average months on IS showed the greatest inter-office variation, ranging from 14.4 months (in Rothesay/Dunoon) to 35.3 months (in Ilford). Average months employed ranged from 20.3 months (in Longsight) to 33.4 (in Johnstone).

5.3 Office characteristics

As indicated above, we classify the office variables into service intensity (customer caseload measures) and service type (type of help and advice provided). For the service-type variables, ERA-control differentials are used to explain variation in programme impacts. To explain variation in control group outcomes, control-group values of the service-type variables are used.

The caseload measures were constructed from monthly monitoring forms for the first 17 months of the experiment. All other office-level variables were constructed from customers' responses to survey interviews carried out 12 and 24 months after random assignment.²⁶ It is likely that the advice and support offered to individual customers were influenced by their own characteristics. However, more relevant to the analysis is a measure of the extent to which the office emphasised particular elements of ERA (i.e. their philosophical approach to helping IS customers achieve self-sufficiency), controlling for differences in the caseload composition. Although office implementation philosophy cannot be observed directly from any of the available data sources, we form proxies for them by adjusting the customer survey measures to control for observable customer characteristics across offices that may influence the type of service received using the following regression model:²⁷

$$(4) \quad F_i = \lambda_0 + \sum_k \lambda_{1k} O_{ki} + \sum_k \lambda_{2k} O_{ki} P_i + \sum_l \lambda_{3l} CC_{li} + e_{ij}$$

where F_i is the measure of interest for individual i , O_{ki} is a dummy variable indicating whether individual i is in office k , P_i is a dummy variable indicating whether individual i is in the programme group or the control group, and CC_{li} is the value of background characteristic l for individual i . The background characteristics include: gender, marital status, the number of children, education, work history and earnings history. Because all variables other than the treatment dummy are mean-centred, the coefficient λ_{1k} is taken to summarise the mean value of F for control group members in office k , while the corresponding measure for the treatment group is $\lambda_{1k} + \lambda_{2k}$. The programme-control differential is λ_{2k} .

²⁵ The median deprivation index for England in 2007 was 17.1 (see http://data.gov.uk/dataset/index_of_multiple_deprivation_imd_2007). The 25th percentile in our sample was 16.4, compared to 9.6 for England and the 75th percentile in our sample was 40.3 compared to 30.2 for England.

²⁶ For details on the customer surveys, see Dorsett *et al.*, 2007 and Riccio *et al.*, 2008).

²⁷ Overall, the adjusted office implementation measures are correlated to some extent with each other (meaning that offices that rank high on one measure have some tendency to rank high on the other), but the correlations are modest at best. Thus, we are able to treat these office implementation measures as separate variables in the statistical analysis.

The specific office variables used in this study and their expected effects on control group outcomes and ERA programme impacts are as follows:

5.3.1 Service intensity

To capture service intensity, we construct two measures, ‘Caseload per Adviser’ (assumed to affect control group outcomes, α_j) and ‘Proportion of Caseload Working with ERA Customers’ (assumed to affect programme impacts, β_j). These variables were constructed from information provided in staff surveys conducted during the course of ERA.

Adviser caseload

Measuring the caseload separately for Personal Advisers (PAs) and Advancement Support Advisers (ASAs) is complicated by the fact that some caseworkers acted purely as ASAs, some acted purely as PAs and some acted as both. For those who acted as both, there is no way of knowing the proportion of time spent as ASA compared to the proportion of time spent as PA. Furthermore, whether someone acted purely as ASA or as ASA/PA depended partly on their ERA caseload. There is evidence (Greenberg *et al.*, 2009) that ASAs whose low ERA caseload left them with excess capacity took on non-ERA cases as well.

The reason for being interested in caseload is that it provides an insight into the degree of support provided to both ERA and traditional New Deal for Lone Parents (NDLP) individuals. ‘Caseload per Adviser’ is a general caseload measure that we allow to affect control group outcomes [α_j in equation (2)] and is defined as:

$$(5) \quad \text{Adviser caseload} = \text{NDLP caseload} / \text{number of advisers},$$

where the number of advisers consists of ASAs, PAs and ASA/PAs, expressed as full-time equivalents.

Proportion of advisers working with ERA customers

As noted above, accurately measuring the caseload of ASAs compared to PAs is complicated. We focus instead on a related measure which is the proportion of advisers working on ERA to at least some extent: This is allowed to affect programme impacts [β_j in equation (2)] and is defined as:

$$(6) \quad \text{Proportion of advisers working with ERA customers} = (\text{number of ASAs and ASA/PAs}) / \text{number of advisers},$$

where, again, the number of advisers consists of ASAs, PAs and ASA/PAs, expressed as full-time equivalents.

This nature of the available data means that this variable is not directly interpretable as capturing the variation across offices in the extent to which they concentrated resources on ERA rather than non-ERA cases. To see this, note that offices where all advisers act as both ASAs and PAs will score highly on this measure yet it is unknown what proportion of their time is spent with ERA customers. Instead, it simply captures the extent to which offices vary in the proportion of advisers who work, at least some of the time, with ERA customers.

The first caseload measure is expected to be positively related to control group months on IS and negatively related to months of employment. In other words, the greater the size of the caseload, the less intensive, on average, will be the services that were provided to each customer. Less intensive services mean each customer was receiving less attention and hence was more likely to remain on IS and less likely to increase employment.

The expected sign of the second caseload measure is ambiguous. In one sense, the higher proportion of advisers working with ERA customers could reflect a greater focus on ERA customers relative to control group customers in which case we would expect a higher score on this measure to lead to a larger negative impact of ERA on months receiving IS and a larger positive impact of ERA on employment. However, a high score may also result from an office practice under which it is the norm for advisers to work as both ASAs and PAs. Where this is the case, one might expect impacts to be reduced if one feels that specialisation as an ASA is likely to result in an improved service to ERA customers.

5.3.2 Service type

We hypothesise that the type of service received by control and programme group members will influence the outcome level for control group members as well as the impact of ERA. As indicated earlier, implicit in this hypothesis is that the type of service received by customers reflects to some extent the emphasis placed by office staff on achieving certain goals for their customers, although clearly the specific needs of the customers were also taken into account in implementing staff goals. Of course, it would have been desirable to also have had data on the actual philosophy of the staff in each office, but such data are not available.²⁸

We measure type of service received by the percentage of customers receiving a particular type of service during the follow-up period. These are calculated for both programme and control group members and are derived from responses to the customer surveys conducted as a part of the ERA evaluation. The types of service received by control group members will influence the average office control-group outcome α_j and the **difference** in the types of service received between programme and control group members is hypothesised to influence the average office **impact** β_j .²⁹ Depending on the nature of the service, the influence of these variables on the outcomes and programme impacts can be either positive or negative. For example, services that emphasise human capital investment may actually have led to an increase in months receiving IS and a decrease in employment during the follow-up period because these services may have induced customers to remain out of work and continue to receive IS while they pursued education or training activities. On the other hand, services that emphasised activities like support while working may be expected to have reduced months receiving IS and to have increased employment during the follow-up period. The service type measures we include in our model are as follows.

Proportion of customers advised to think long term

The survey asked customers: 'When working did you get any advice or encouragement on working out long-term career goals? When not working did you get any advice or help from Jobcentre staff with working out long-term career goals?' Their responses were used to indicate whether customers had received any advice on long-term goals, either when working or when out of work. We hypothesise that customers advised to think-long term were more likely to pursue human capital goals than customers not advised in these ways and, hence, may have prolonged their stay on IS and have had fewer months of employment during the follow-up period.³⁰

²⁸ In their cross-office study, Bloom *et al.* (2005) use variables for both programme type (termed programme activities) and staff philosophy (termed programme implementation). As part of the ERA data collection process, information on staff attitudes was collected, but the samples were too small to be useful at the office level.

²⁹ Use of the differential to influence programme impacts follows Bloom *et al.* (2005).

³⁰ Because we only have five years of follow-up data, we are unable to examine the longer-term job retention and progression consequences of pursuing human capital goals.

Proportion of customers helped finding an education or training course

The survey asked customers: ‘When working did you get any advice or encouragement finding an education or training course and when not working did you get any advice or help from Jobcentre staff with looking for education or training courses?’ Responses to these questions were used to indicate whether customers had received any help finding an education or training course. We hypothesise that customers advised to seek education or training were more likely to have stayed on IS and have had fewer months of employment for at least part of the follow-up period.³¹

Proportion of customers whose advisers discussed in-work advancement

The survey asked customers: ‘When working did you get any advice or encouragement on how to negotiate a pay raise, how to negotiate better terms (for example, more convenient hours) or how to get a promotion in your present work? When not working did you get any advice or help from Jobcentre staff with helping you to stay in work or advance in future jobs?’ Their responses were used to indicate whether customers had discussed in-work advancement, either when working or when out of work. We hypothesise that customers receiving this encouragement to advance were less likely to have returned to IS and more likely to have increased their employment.

Proportion of customers given a lot of support while working

The survey asked customers: ‘And how much support has this person [the adviser] given you when working?’ Respondents could choose the response ‘a lot’ and this was used to define the variable. The office mean was calculated on the basis of those in work at some point. We hypothesise that customers given a lot of support while working would have been less likely to have returned to IS and more likely to have increased their employment.

Proportion of ERA customers aware of the work retention bonus

The survey asked customers in the programme group: ‘Under the ERA programme, some people who work for at least 30 hours a week for about 3 months out of 4 can claim a bonus of £400. Have you heard of this bonus?’ The responses to this question were used to indicate whether customers were aware of the work retention bonus, either when working or when out of work. We hypothesise that knowledge of the bonus would have led to more customers leaving IS and increasing their employment. A possible concern with the bonus variable is that awareness of the retention bonus may have been affected by individuals’ employment status. For example, if awareness was higher **because of** being in work, attempts to examine the relationship between employment and awareness will encounter problems of endogeneity. There are a number of reasons why this problem may be mitigated, at least in part. First, as shown below, awareness of the retention bonus was very high. Even in the office with the lowest level of awareness, three-quarters of the customers still knew about the bonus. Other offices showed universal awareness. These levels are much higher than the levels of employment (on average, only 25 months of the 60 months post-random assignment were spent in employment) and this reflects a key aim of ERA to make everybody who might at some stage be eligible aware of the retention bonus. Second, as explained above, the office-level measure of awareness was regression-adjusted to control for customer characteristics and, in so doing, will control for the effect of some of the characteristics that will influence employment. Third,

³¹ Customers not employed may delay work entry while they complete their course. Employed customers may be able to participate in training while remaining in work, but some may be induced to temporarily reduce their employment in order to participate in education or training.

the measure of awareness relates to the first two years post-random assignment whereas the main outcomes considered in the analysis cover the first five years post-random assignment.³²

All of the office-level variables (both control group measures and programme group measures) were grand-mean-centred. The control values of the grand-mean-centred variables were included in the level 2 equation determining α_j (the control mean outcome) while the difference in the values of the grand-mean-centred variables between the programme and the control groups were interacted with the treatment dummy variable (P_{ji}) and included in the level 2 equation determining β_j (the programme impact).

5.3.3 Summary statistics for the office variables

Table 5.2 present the means and the cross-office range of the (regression adjusted) office variables used in the multi-level analysis. The caseload averages about 29 customers per adviser and about 42 per cent of these advisers, on average, work with ERA customers. There is significant variation in the caseload across offices (from about three customers per adviser to 110 customers per adviser) and in the proportion of advisers working with ERA customers (from about 20 per cent to 94 per cent).

For each of the service type measures, Table 5.2 presents the mean proportion for the control (NDLP) group, the mean proportion for the programme (ERA) group, and the mean ERA-control group difference in the proportion. The first and third of these (control group value and ERA-control group difference) are used as variables in the multi-level model. The second (ERA value) is not directly included in the multi-level model (except for the retention bonus awareness variable) and is shown for informational purposes only.

On average, for every service type, the ERA group had a higher proportion receiving that service than the control group. This is as would be expected; however the differential is not always that great. In some offices, a greater proportion of the control group received the services, as reflected in the negative minimum values of the differential in the cross-office ranges.³³ In no office were less than three-quarters of the ERA customers aware of the retention bonuses and in some offices all of the ERA customers surveyed were aware of the bonuses.

³² Note that the other office-level variables are not subject to this complication since they are defined conditional on employment status.

³³ Specifically, there were four offices in which the proportion of customers advised to think long term was higher among the control group than the programme group; seven offices where the proportion of customers receiving help finding an education or training course was higher; six offices where the proportion receiving help with in-work advancement was higher; and seven offices where the proportion receiving support while working was higher.

Table 5.2 Office characteristics (measured during the follow-up period)

Office variable	Cross-office range		
	Mean	Minimum	Maximum
Caseload (per adviser)	29.47	3.27	109.99
Proportion of advisers working with ERA customers	0.42	0.20	0.94
Proportion of customers advised to think long term			
Control group	0.30	0.00	0.74
ERA group	0.49	0.02	1.00
ERA-control differential	0.19	-0.49	0.93
Proportion of customers receiving help finding education course			
Control group	0.43	0.00	1.00
ERA group	0.64	0.14	0.99
ERA-control differential	0.21	-0.17	0.54
Proportion of customers receiving help with in-work advancement			
Control group	0.20	0.00	0.67
ERA group	0.40	0.00	1.00
ERA-control differential	0.20	-0.69	1.00
Proportion of customers receiving support while working			
Control group	0.45	0.00	1.00
ERA group	0.65	0.21	1.00
ERA-control differential	0.21	-0.49	1.00
Proportion of ERA customers aware of retention bonuses	0.87	0.75	1.00
<i>Sample size</i>	37	37	37

Note: Caseload variables taken from staffing form. All other variables taken from customer surveys. The office variables have been regression adjusted to control for differences in customer characteristics across offices.

Table 5.3 presents a correlation matrix of the office variables for the control group and the ERA programme group. For both groups, the correlations between the non-caseload variables are all positive, suggesting that retention and advancement services were being delivered together, although not perfectly. For the ERA group these positive correlations are consistent with the goals of the demonstration. From a statistical standpoint, the fact that the correlations are modest implies that it is theoretically possible to estimate the contribution of each element separately.

Table 5.3 Correlation matrix of office characteristics

Office variable							
Control group		1	2	3	4	5	
Caseload (per adviser)	1	1					
Proportion of customers advised to think long term	2	-0.0617	1				
Proportion of customers receiving help finding education course	3	0.0047	0.3383	1			
Proportion of customers receiving help with in-work advancement	4	-0.1565	0.4858	0.2967	1		
Proportion of customers receiving support while working	5	0.0142	0.4318	0.2504	0.1095	1	
ERA group		1	2	3	4	5	6
Proportion of advisers working with ERA customers	1	1					
Proportion of customers advised to think long term	2	-0.0060	1				
Proportion of customers receiving help finding education course	3	0.0930	0.6456	1			
Proportion of customers receiving help with in-work advancement	4	-0.1728	0.6717	0.3664	1		
Proportion of customers receiving support while working	5	-0.1617	0.4295	0.4475	0.2758	1	
Proportion of ERA customers aware of retention bonuses	6	-0.2520	0.4375	0.2953	0.3784	0.2527	1

Note: Caseload variables taken from staffing form. All other variables taken from customer surveys. The office variables have been regression adjusted to control for differences in customer characteristics across offices.

6 Results

We present the results of estimating the multi-level model in Tables 6.1-6.5. Table 6.1 presents summary statistics from various versions of the model to indicate how much accounting for office-characteristics matters. Table 6.2 shows the effects of the customer characteristics on the five-year outcomes. Table 6.3 shows how these customer characteristics affect the programme impact (subgroup impacts). Table 6.4 shows how the office characteristics affect the control group outcomes and Table 6.5 shows how the office characteristics affect the programme impacts. Two tables in the appendix present the year-by-year estimates of the model. When informative, the year-by-year results will be discussed.

6.1 Ability of the office variables to explain office variation in outcomes and impacts

6.1.1 Extending the standard impact model

Earlier (in Section 3.3), we presented the results of tests of the statistical significance of the cross-office variation in programme impacts. We concluded that there was enough variation to warrant specifying and estimating a more comprehensive multi-level model. Table 6.1 presents summary statistics for three versions of the multi-level model. The first set of summary statistics is for a multi-level model with only random error terms for the office control outcome and the office programme impacts. In other words, the office control outcomes and the office programme impacts are assumed to be randomly distributed across offices without any systematic variation due to office characteristics. This is equivalent to saying that the distributions of office impacts in Figures 3.1 and 3.2 are purely random and arise from sampling error. For this specification of the multi-level model, the level 2 equations are simply:

$$(7) \quad \begin{aligned} \alpha_j &= \alpha_0 + u_j, \\ \beta_j &= \beta_0 + \mu_j. \end{aligned}$$

The statistics reported for this model are estimates of the standard deviations of the three error terms in the model [$sd(u_j)$, $sd(\mu_j)$, and $sd(\varepsilon_{ji})$]. Standard errors of the estimated standard deviations are also presented.

The second set of summary statistics in Table 6.1 is for a multi-level model where the office control outcomes are allowed to vary systematically across offices, but the office programme impacts are assumed to be randomly distributed across offices. For this specification, the level 2 equations are

$$(8) \quad \begin{aligned} \alpha_j &= \alpha_0 + \sum_m \zeta_m SI_{mj} + \sum_n \eta_n ST_{nj} + u_j, \\ \beta_j &= \beta_0 + \mu_j. \end{aligned}$$

Finally, the third set of summary statistics is for a multi-level model where both the office control outcomes and the office programme impacts are allowed to vary systematically across offices; that is, the fully specified model given by equations (1) and (2).

Table 6.1 Summary statistics for estimated multi-level models with and without office variables (standard errors of standard deviation estimates are in parenthesis)

Model	Months on IS	Months employed
(1) No office variables – equation 7 in text		
Standard deviation of control group outcome error – $sd(u_j)$	3.26 (0.23)	2.07 (0.46)
Standard deviation of programme impact error – $sd(\mu_j)$	0.33 (.23)	0.68 (1.48)
Standard deviation of individual random error – $sd(\varepsilon_{ji})$	19.84 (0.17)	20.54 (0.18)
χ^2 for test versus linear regression	83.58***	26.27***
(2) Control office variables only – equation 8 in text		
Standard deviation of control group outcome error – $sd(u_j)$	2.63 (0.54)	1.32 (0.55)
Standard deviation of programme impact error – $sd(\mu_j)$	0.77 (1.22)	1.04 (1.06)
Standard deviation of individual random error – $sd(\varepsilon_{ji})$	19.84 (0.17)	20.54 (0.18)
χ^2 for test versus linear regression	34.88***	4.99*
χ^2 for test versus model (1)	15.28***	17.01***
(3) Control and impact office variables only – equation 3 in text		
Standard deviation of control group outcome error – $sd(u_j)$	2.02 (0.52)	1.32 (0.57)
Standard deviation of programme impact error – $sd(\mu_j)$	0 (0.00)	0 (0.00)
Standard deviation of individual random error – $sd(\varepsilon_{ji})$	19.84 (0.17)	20.54 (0.18)
χ^2 for test versus linear regression	12.11**	2.59
χ^2 for test versus model (2)	18.38***	13.83***
Number of individuals	6,754	6,754
Number of offices	37	37
Individuals per office		
Minimum	14	14
Maximum	661	661
Average	182.5	182.5

Notes: Even though $sd(\varepsilon_{ji})$ estimates are the same in each model, they are not required to be the same. Even though $sd(\mu_j)$ estimates for model (3) are zero, they are not required to be zero.

* Significant at the 10 per cent level.

** Significant at the 5 per cent level.

*** Significant at the 1 per cent level.

For each of the three models, a chi-square statistic is reported for a test of the multi-level model versus a linear regression. In other words, it indicates whether or not the office control outcome (α_j) and the office programme impact (β_j) can be assumed to have random components (the u_j and μ_j). If the test is rejected, then we accept the hypothesis that the office control outcome and the office programme impact vary significantly across offices and that part of the variation is random. In addition to the test for a linear regression, models (2) and (3) present chi-square statistics for the significance of the variables in the model compared to the previous model. Thus, for example, the chi-square statistic for model (2) versus model (1) is a test for the significance of including the office variables for the control group. The chi-square statistic for model (3) versus model (2) is a test for the significance of adding the office variables for the ERA group.

6.1.2 Are the random components justified?

For the first model in Table 6.1 in which it is assumed that the only variation across offices in the control outcome and the programme impact is random, the linear regression specification is rejected for both months on Income Support (IS) and months employed. In other words, we reject the hypothesis that there are no random components to the office control outcome and the office programme impact. Or, to put it another way, the multi-level model with three error terms is preferred to the standard linear specification with only one error term.

6.1.3 Can inclusion of the office variables be justified?

When the office characteristics are allowed to affect the office control outcome only (model (2)), the results for the existence of random error terms do not change and the linear regression specification is again rejected. Furthermore, the second chi-square statistic indicates model (2) represents an improvement over model (1). When the office characteristics are allowed to affect the programme impacts (model (3)), the linear regression specification is rejected for months on IS, but is no longer rejected for months employed. However, the second chi-square statistic indicates that model (3) is a significant improvement over model (2). Given the conservative nature of the linear regression specification test, it is not unreasonable to conclude that the multi-level model given by model (3) (equations (1) and (2) previously) is an appropriate specification for examining systematic variation in office control outcomes and office programme impacts across both individuals and across offices.³⁴

6.2 Effects of customer characteristics on outcomes

Table 6.2 presents the effects of the customer characteristics on months receiving IS and months employed during the five-year follow-up period. For comparison purposes, the grand mean control group outcome (α_0) is also presented in the table. The coefficients of the customer characteristic variables in Table 6.2 represent the **deviation** from the mean outcome of the omitted reference group (see Table 5.1). Thus, for example, the coefficient of 3.63 for individuals with A-level qualification on months employed is their additional months employed compared to customers with no qualifications (outcome for customers with no qualifications is not shown in Table 6.2).

The average control group member spent 26.7 months on IS and 25.2 months employed during the five-year follow-up period (first row of Table 6.2). As would be expected, many of the customer characteristics are significantly related to the outcomes. Individuals who are younger (below age 30), less educated (qualifications below General Certificate of Secondary Education (GCSE) level),

³⁴ As indicated in the Stata manual (p. 339), the linear regression test is conservative so that it is possible there is significant variation in the office random error terms even if the chi-square test suggests there is not.

have less previous work experience (worked 12 or fewer months in the past three years), are non-white, and live in more deprived areas spend longer periods of time on IS and have less time employed than their counterparts (who are aged at least 30, qualified above GCSE level, worked more than 12 months in three years before random assignment, white and living in less deprived areas). Customers who were not previously partnered also spend more time on IS than those who were previously partnered, but did not spend less time employed during the follow-up period.

Table 6.2 Effects of customer characteristics on control group outcomes over five years

Characteristic	Months on IS		Months employed	
	Coefficient	Standard error	Coefficient	Standard error
Control group grand mean outcome (α_0)	26.66***	0.52	25.22***	0.44
Effect on control group outcome (deviation from reference group)				
1 = Female	1.12	1.66	6.07***	1.71
1 = Never partnered	2.12**	0.85	-0.44	0.88
1 = O-level qualification	-3.48***	0.88	2.66***	0.92
1 = A-level qualification	-6.21***	1.06	3.63***	1.09
1 = Other educational qualification	-1.70	1.44	-0.38	1.48
1 = More than one child in family	-0.46	0.73	0.99	0.76
1 = Worked for 12 or fewer months in the past three years	4.46***	1.20	-6.17***	1.24
1 = Worked for 13 to 24 months in the past three years	0.84	1.35	-1.84	1.39
1 = Worked in the past year	-4.04***	1.25	6.19***	1.29
Weekly earnings in most recent job in the year before random assignment	0.02	0.01	-0.02**	0.01
1 = Randomly assigned between October 2003 to December 2003	5.58***	1.24	-3.35***	1.28
1 = Randomly assigned between January 2004 to March 2004	2.94***	0.84	-0.31	0.86
1 = Randomly assigned between April 2004 to June 2004	1.85**	0.93	-1.08	0.96
Number of months receiving IS in the two years prior to random assignment	0.44***	0.05	-0.12**	0.05
1 = Youngest child in household less than six at random assignment	3.80***	0.84	-1.65*	0.87
1 = Aged 30-39 years old	-2.62***	0.91	1.49	0.94
1 = Aged 40 plus years old	-4.16***	1.20	3.18**	1.24
1 = Non white	4.69***	1.10	-3.39***	1.12
Index of multiple deprivation	0.09***	0.02	-0.05***	0.02

Notes: Coefficient on characteristic represents *deviation* from mean outcome of omitted reference group (see Table 5.1). Thus, the coefficient of 3.63 for individuals with A-level qualification on months employed is their additional months employed compared to customers with no qualifications (not shown in table).

* Significant at the 10 per cent level.

** Significant at the 5 per cent level.

*** Significant at the 1 per cent level.

Interestingly, time spent on IS declines systematically according to the calendar time of random assignment (the later the time of random assignment, the fewer the months spent on IS during the follow-up period). At first sight, this seems somewhat surprising given the onset of recession in the second quarter of 2008 will have affected labour market outcomes of those randomised earlier less than those randomised later. However, there are two countervailing factors. First, a feature of the recent recession is that, up until the second quarter of 2010 (the latest period for which outcomes are considered in this analysis), the reduction in the overall employment rate was driven almost entirely by the fall in the proportion of men in work. As we have already seen, the New Deal for Lone Parents (NDLP) group is predominantly female and women's employment remained comparatively stable. Second, policy developments in the United Kingdom (UK) have increased the conditions placed on lone parents. For example, the frequency of mandatory Work Focused Interviews (WFIs) has increased and, since 2005, most of those who attend a WFI have been required to agree an action plan with their Personal Adviser to prepare themselves for work (Finn and Gloster, 2010). As another example, since 2008, lone parents with a youngest child aged 12 or over are no longer entitled to IS solely on the grounds of being a lone parent (Department for Work and Pensions, 2007). Those randomly assigned more recently will have been subject to the new regulations for a greater proportion of their follow-up period than those randomly assigned earlier.³⁵

6.3 Effects of customer characteristics on programme impacts

Table 6.3 presents the effects of the customer characteristics on programme impacts over the five-year follow-up period. As indicated earlier, these are commonly referred to as 'subgroup' impacts. For comparison purposes, the grand mean impact of ERA (β_0) is also presented in the table. The coefficients on the subgroup impacts in this table represent deviations from the impacts of the omitted reference groups (see Table 5.1). Thus, for example, the coefficient of 4.84 for individuals with A-level qualification on months employed is their **additional** impact compared to customers with no qualifications. Note that the impacts for individuals in the reference groups (those with no qualifications in this example) are not shown in Table 6.3. All that the table shows are **deviations** in impacts from the reference group, and not the impacts themselves for either group.

The average response to ERA (the first row of Table 6.3) is not statistically significant for either outcome and, for the most part, it does not appear that the impacts vary significantly across subgroups. One result that stands out across all the outcomes has to do with qualifications. It appears that individuals with A-level qualifications had larger responses over the five-year follow-up period to ERA than individuals with no qualifications. They had larger reductions in the number of months on IS, and larger increases in the number of months employed than individuals with no qualifications. Another notable result is that the impacts on months receiving IS and months employed seem to have varied with the degree of local area deprivation. Specifically, ERA customers living in more deprived areas had larger reductions in months on IS and larger increases in months employed than ERA customers living in less deprived areas. Thus, ERA appears to have been more effective in more deprived areas. Finally, ERA seems to have caused larger reductions in months on IS for customers 40 years of age and older, compared to customers younger than 30 years of age, but did not lead to larger increases in their months employed.³⁶

³⁵ A fuller discussion of policy developments in the UK during the years ERA was conducted is presented in Hendra *et al.*, 2011.

³⁶ Though not statistically significant, ethnic minorities had a more positive impact on months employed and a more negative impact on months receiving IS than whites. Within the first two years of ERA, the larger ethnic minority impact on employment was statistically significant (see Riccio *et al.*, 2008, Table 6.5).

Table 6.3 Effects of customer characteristics on programme impacts over five years

Characteristic	Months on IS		Months employed	
	Coefficient	Standard error	Coefficient	Standard error
Grand mean impact of ERA (β_0)	-0.79	0.48	0.11	0.50
Subgroup impact (deviation from reference group)				
1 = Female	-4.00*	2.30	2.82	2.38
1 = Never partnered	-0.02	1.21	-0.84	1.25
1 = O-level qualification	-1.86	1.26	1.36	1.30
1 = A-level qualification	-3.17**	1.51	4.84***	1.56
1 = Other educational qualification	-3.50*	2.06	4.42**	2.13
1 = More than one child in family	0.86	1.04	-0.97	1.08
1 = Worked for 12 or fewer months in the past three years	-0.72	1.67	0.54	1.73
1 = Worked for 13 to 24 months in the past three years	0.96	1.90	-0.81	1.96
1 = Worked in the past year	0.48	1.71	-3.68**	1.77
Weekly earnings in most recent job in the year before random assignment	-0.01	0.01	0.02	0.01
1 = Randomly assigned between October 2003 to December 2003	-2.52	1.75	0.14	1.81
1 = Randomly assigned between January 2004 to March 2004	0.61	1.18	-1.67	1.22
1 = Randomly assigned between April 2004 to June 2004	-0.56	1.31	0.35	1.36
Number of months receiving IS in the two years prior to random assignment	-0.02	0.07	0.02	0.07
1 = Youngest child in household less than six at random assignment	-0.82	1.19	-0.13	1.23
1 = Aged 30-39 years old	-2.06	1.29	1.52	1.34
1 = Aged 40 plus years old	-3.74**	1.70	2.15	1.76
1 = Non white	-2.06	1.50	2.25	1.54
Index of multiple deprivation	-0.06**	0.03	0.06**	0.03

Notes: All characteristics are interacted with a dummy variable for being an ERA customer. Coefficients on subgroup impacts represent *deviation* from mean impacts of omitted reference group (see Table 5.1). Thus, the coefficient of 4.84 for individuals with A-level qualification on months employed is their additional impact compared to customers with no qualifications (impact for reference group is not shown in table).

* Significant at the 10 per cent level.

** Significant at the 5 per cent level.

*** Significant at the 1 per cent level.

6.4 Effects of office characteristics on office control group outcomes

Table 6.4 shows how the office characteristics affect office control group outcomes. In other words, the results in Table 6.4 provide an indication of whether office characteristics are systematically related to office outcomes for traditional NDLP customers. For comparison purposes, the grand mean control group outcome (α_0) is also presented in the table. In addition to presenting the coefficient estimates, we also present the interquartile range of the outcome across offices. The interquartile range is the predicted outcome from the 25th percentile of the office characteristic to the 75th percentile. The interquartile range provides an indication of how the control outcome varies across offices possessing the middle 50 per cent range of values of a particular characteristic.

Table 6.4 Effects of office characteristics on office control group outcomes over five years

	Coefficient	Standard error	Interquartile range across offices
Months on IS			
Control group grand mean (α_0)	26.66***	0.52	24.43 to 28.11
Caseload (per adviser)	0.06***	0.02	25.36 to 26.76
Proportion of customers advised to think long term	3.53	5.44	26.29 to 26.88
Proportion of customers receiving help finding education course	11.05***	3.83	25.64 to 27.78
Proportion of customers receiving help with in-work advancement	-12.01**	5.30	27.82 to 25.78
Proportion of customers receiving support while working	-3.41	2.16	27.06 to 26.22
Months employed			
Control group grand mean (α_0)	25.22***	0.44	23.89 to 26.53
Caseload (per adviser)	-0.04**	0.02	26.11 to 25.16
Proportion of customers advised to think long term	-4.60	4.85	25.71 to 24.94
Proportion of customers receiving help finding education course	-9.04***	3.50	26.06 to 24.31
Proportion of customers receiving help with in-work advancement	9.01**	4.60	24.36 to 25.89
Proportion of customers receiving support while working	3.00	1.91	24.87 to 25.61

Notes: Interquartile range is the predicted outcome from the 25th percentile of the office characteristic.

** Significant at the 5 per cent level.

*** Significant at the 1 per cent level.

As Table 6.4 indicates (and as already shown in Table 6.2), the average control group member spent about 27 months (45 per cent of the time) on IS over the five-year follow-up period and about 25 months (41 per cent of the time) employed. The interquartile range across offices is modest, from 24 to 28 months on IS and 24 to 27 months employed. The full range of the average control outcomes across offices (not presented in the table) is 21 to 34 months for IS and 21 to 30 months for employment.³⁷

³⁷ These ranges differ from those presented earlier in Table 5.1. The earlier ranges are simply the raw average outcomes by office while the ranges presented here are the ranges derived directly from the estimates of the multi-level model.

As Table 6.4 further indicates, both the intensity and type of service appears to matter for the control group. Offices with larger adviser caseloads have control customers that spent more months on IS and fewer months employed during the follow-up period. The results imply that for the average office with almost 30 customers per adviser (see Table 5.2), a doubling of the caseload would result in almost two more months on IS ($.06 \times 30$) and almost 1.2 fewer months of employment ($-.04 \times 30$) for control (NDLP) customers. The interquartile range across offices of the effects of caseload size ranges from 25 to 27 months for IS and 25 to 26 months for employment. While these are not large differences across offices, they do illustrate that the size of adviser caseloads matter for effectively administering services under the traditional New Deal programme.

The results also suggest that in offices where all NDLP customers receive help in finding education courses, the amount of time spent on IS is increased by 11 months and the amount of time spent in work is reduced by nine months relative to offices where no customers receive such help. These are sizeable effects. While they imply greater dependence on IS during the five-year follow-up period, they may imply greater self-sufficiency in the long run if the education eventually leads to upgraded skills and higher employment and earnings. In Tables A.1 to A.2 in the appendix, we present the year-by-year results for the multi-level model. The year-by-year results suggest that the effects of prolonging IS and reducing employment are stronger in the first three years of follow-up and gradually weaken after that, losing statistical significance in years four and five.³⁸ As was the case for the effects of caseload size, the variation across offices in the proportion of customers receiving help in finding education courses is not great, leading to a small interquartile range of just over two months in the actual effect of this service on months spent on IS and just under two months in the effect of this service on months employed.

In contrast to education services, in offices where all customers receive help with in-work advancement, 12 months fewer are spent on IS, on average, and nine months more are spent employed than in offices that offer no such help. These are also sizeable effects and the cross-office variation in the proportion of customers receiving this service is somewhat less than the cross office variation in the proportion of customers receiving education help, leading to a smaller interquartile range of just over two months on IS and just over 1.5 months of employment.

6.5 Effects of office characteristics on ERA programme impacts

Table 6.5 shows how the office characteristics affect ERA programme impacts. Recall that for the ERA input types available to control group members (advice for thinking long term, help in finding education courses, help with in-work advancement, and support while working), the office characteristics included in the multi-level model are measured as **differences** in the proportions receiving such services between the ERA programme group and the control group (see Table 5.2). The other two office characteristics included in the multi-level model (the proportion of advisers working with ERA customers and the proportion of ERA customers aware of the employment retention bonus) apply only to ERA programme group members and, hence, are simply measured as the proportion for ERA programme group members. Also note that none of the average five-year ERA programme impacts across offices is significantly different from zero over the five-year follow-up period (the first row of results under each outcome in Table 6.5 (and shown previously in Table

³⁸ Data beyond the five-year follow-up would be required to determine whether the short-run IS and employment effects of providing education advice eventually reverse in sign and become statistically significant.

6.3)).³⁹ Thus, office characteristics that we identify as being significantly related to five-year ERA programme impacts may imply that the impacts for some offices are significantly positive and for other offices significantly negative. In fact, the interquartile range of impacts across offices is -2.2 months to .4 months for IS and -.7 months to 1.0 months for employment. The full range of impacts across offices (not reported in the table) is -9.3 months to 3.7 months for IS and -4.9 months to 9.4 months for employment.

Table 6.5 Effects of office characteristics on office programme impacts over five years

	Coefficient	Standard error	Interquartile range across offices
Months on IS			
Grand mean impact of ERA (β_0)	-0.79	0.48	-2.18 to 0.41
Proportion of advisers working with ERA customers	-4.78	3.41	-0.30 to -1.19
Proportion of customers advised to think long term	3.79	4.09	-1.23 to -0.36
Proportion of customers receiving help finding education course	4.61	3.34	-1.65 to -0.25
Proportion of customers receiving help with in-work advancement	-7.74*	4.38	-0.18 to -1.42
Proportion of customers receiving support while working	-3.77	2.35	-0.14 to -1.33
Proportion of ERA customers aware of retention bonuses	-26.27***	8.42	0.75 to -2.19
Months employed			
Grand mean impact of ERA	0.11	0.50	-0.69 to 1.04
Proportion of advisers working with ERA customers	3.16	3.39	-0.21 to 0.37
Proportion of customers advised to think long term	-2.57	4.03	0.41 to -0.18
Proportion of customers receiving help finding education course	-5.35	3.31	1.11 to -0.51
Proportion of customers receiving help with in-work advancement	9.32**	4.39	-0.62 to 0.87
Proportion of customers receiving support while working	4.32*	2.37	-0.64 to 0.73
Proportion of ERA customers aware of retention bonuses	15.86*	8.24	-0.82 to 0.95

Notes: Interquartile range is the predicted outcome from the 25th percentile of the office characteristic.

* Significant at the 10 per cent level.

** Significant at the 5 per cent level.

*** Significant at the 1 per cent level.

As indicated in Table 6.5, three of the six office characteristics are estimated to be significantly related to ERA programme impacts, although in one case significance does not exist for both outcomes. First, in offices where ERA customers were given help with in-work advancement, the average programme group member spent eight fewer months on IS and nine more months in employment over the five-year follow-up period than in offices where ERA customers were not given such help. More precisely, if 100 per cent of ERA customers in these offices received help with in-work advancement and no control group members received this help, ERA would have reduced the

³⁹ However, as shown in the tables in the appendix, some of the ERA programme impacts are statistically significant in certain years. Specifically, months spent on IS are reduced significantly (but quite modestly – by less than one month) in years one and two.

amount of time spent on IS by a further eight months relative to the impact in an office where there is no difference in the proportion of ERA versus control group members receiving this kind of help. Or, to put it another way, an individual in an office with a ten percentage point higher proportion of ERA customers versus control group members receiving help with in-work advancement will have .8 fewer months on IS than an individual in an office where there is no such difference in the proportion receiving this help. The information on interquartile ranges is very important because, in practice, few of the differences in receiving this kind of help were very large, so the effect translates to only about a 1.2 month interquartile range across offices in the impact on IS receipt and about a 1.5 month interquartile range across offices in the impact on months employed.

Second, in offices where all ERA customers received support while working during the follow-up period, an average of about four months fewer were spent on IS and about four months more employed than in offices where no ERA customers received such help (although the effect on IS is not statistically significant). As was the case for in-work advancement, the cross-office variation in the programme-control group differential in the proportion getting this help is not very great, so the effect translates to only about a 1.2 month interquartile range across offices in the impact on IS receipt and a 1.4 month interquartile range across offices in the impact on months employed.

Finally, the strongest estimated effect on programme impacts is given by the ERA customers' awareness of the employment retention bonus. In offices where all ERA customers were aware of the bonus, the coefficient implies that they would have worked 16 more months and would have spent 26 fewer months on IS than in offices where no ERA customers were aware of the bonus. This effect illustrates the integral part played by the financial bonus as part of the overall ERA programme design. Fortunately, almost all ERA customers were aware of the bonus (no office had fewer than 75 per cent aware), so while the bonus was apparently an important part of the ERA programme design, it translated into a moderately small interquartile range of ERA programme impacts across offices (about three months for the interquartile range of impacts on IS and about two months for the interquartile range of impacts on employment).

Though not statistically significant, two additional results in Table 6.5 are worth mentioning. It appears that in offices where ERA customers received help in finding education courses and thinking long term, they are estimated to have spent, if anything, more months on IS and fewer months in employment than in offices where ERA customers did not receive such help.⁴⁰ As was the case for the control group under the traditional NDLP programme, such a result may have more favourable long-term benefits if the human capital investment stimulated by the adviser intervention eventually leads to more stable employment and higher earnings beyond the follow-up period. Additional data beyond the current follow-up period would be needed to shed light on this possibility.

⁴⁰ As indicated from the year-by-year results in the appendix, the effect of thinking long term on months of employment is statistically significant and negative in the first year after random assignment, but not thereafter.

7 Conclusions and policy implications

For out of work lone parents, the Employment Retention and Advancement (ERA) demonstration did not have statistically significant impacts over the five-year follow-up period on the two outcomes examined in this report (months receiving Income Support (IS) and months employed). However, there is some evidence that the impacts of ERA on these outcomes varied across the offices participating in the demonstration. The main purpose of this study has been to try to determine whether this cross-office variation in ERA programme impacts for lone parents can be explained by differences in the way the ERA programme was implemented across the offices that participated in the demonstration. Secondary objectives of this study have been to determine whether office characteristics can help explain cross-office variation in the control environment (under the traditional New Deal for Lone Parents (NDLP) programme) and whether the impacts of ERA vary with certain personal characteristics of the ERA customers (subgroup impacts).

To accomplish these objectives, a multi-level statistical model capable of explaining variation across both individuals and offices in behavioural outcomes and ERA programme impacts was specified and estimated. The model represents an extension of the pioneering framework developed by Bloom *et al.* (2005).

In interpreting the results of this study, it is important to understand that while certain office characteristics may be quite important in explaining outcomes and impacts, lack of variation in these characteristics across offices may lead to only a small estimated variation in these outcomes and impacts across offices. Thus, for example, while our results indicate the importance of conveying information about the financial rewards available to lone parent ERA customers who maintain employment, there was not much variation in the actual conveying of this information across offices, so it only led to modest variation in programme impacts across offices.

Our results indicate that lone parent ERA customers with A-level qualifications (approximately one-fifth of the ERA customers) responded differently to the ERA programme than customers with no qualifications. In particular, they reduced their months on IS and increased their months employed by more than lone parent ERA customers with no qualifications. There is also some evidence that lone parent ERA customers living in the most deprived areas had larger impacts than lone parent ERA customers living in less deprived areas. Specifically, lone parent customers living in more deprived areas reduced their months on IS and increased their months employed in response to ERA by more than lone parent ERA customers living in less deprived areas. Finally, older lone parent ERA customers (40 years of age and above) also tended to respond more to ERA, reducing their months on IS by more than lone parent ERA customers under the age of 30. Subgroup variation was not, though, the primary focus of this analysis (it is considered further in the ERA final report). Our main results concern the effect of office characteristics. Several such characteristics were found to affect the control environment (outcomes of control customers under the traditional NDLP programme). Offices with higher adviser caseloads had lone parent customers that spent more months on IS and fewer months employed over the five-year follow-up period. Where offices prioritised help in finding education courses, customers had more months on IS and fewer months employed. Finally, where offices emphasised in-work advancement, customers had fewer months on IS and more months employed.

Most importantly, we find that several office characteristics affected the **impacts** of ERA on lone parents. ERA's main design feature was to extend the New Deal programme by providing help **after** employment was obtained. We have estimated that such retention services can lead to additional impacts beyond those obtained under the New Deal programme and can help customers achieve economic self-sufficiency (by spending fewer months on IS and more months employed). Offices that emphasised in-work advancement and in-work support more generally tended to deliver stronger effects of ERA, as did those offices where awareness levels of the employment retention bonus were higher. On the other hand, some of the services examined (particularly those that emphasise human capital investment rather than in-work advancement) were not found to affect the impact of ERA.

Such findings provide previously unavailable evidence to policy-makers concerned with encouraging employment among lone parents on welfare. Most obviously, they point to the ingredients likely to be important when designing future in-work support programmes. They also suggest which elements may be less effective. In line with the finding in the ERA final report that ERA increased training participation without leading to an overall effect on employment, the results of this analysis suggest that the impact of ERA does not vary according to the extent to which offices emphasised training and planning for the long term. It is, of course, possible that the additional human capital resulting from training will prove to be beneficial in the long run. Definitive evidence on whether it eventually translates into more favourable long-run outcomes requires additional data beyond the five years examined in this report.

In conclusion, it is relevant to mention that, as with any long-term study, the economic and policy environment changes. Most obviously, the results relate to a period marked by severe recession and associated increases in unemployment. Equally relevant though is the fact that the last few years have seen a number of policies introduced that directly affect lone parents in the United Kingdom. Lone parents have been increasingly required to attend Work-Focused Interviews and those with a youngest child aged seven or over now have to actively seek work. Furthermore, In-Work Credit was introduced in 2008, providing weekly subsidies to lone parents entering work of 16 or more hours per week. The effect of such policy developments is to reduce the contrast between the service available to the ERA group and that available to the control group and has an important bearing on how to view the overall effect of ERA. However, despite this, the analysis has still been successful in identifying those particular implementation features that tended to strengthen the impact of ERA.

Appendix

Year-by-year estimates

Table A.1 Results by year for number of months receiving Income Support

Variable	Year 1	Year 2	Year 3	Year 4	Year 5
1 = ERA	-0.311*** (0.10)	-0.319** (0.13)	-0.158 (0.13)	-0.074 (0.12)	0.072 (0.12)
1 = Female	-0.682* (0.35)	0.372 (0.43)	1.031** (0.43)	0.319 (0.42)	0.074 (0.41)
1 = Never partnered	0.367** (0.18)	0.428* (0.22)	0.284 (0.22)	0.494** (0.22)	0.549*** (0.21)
Highest qualification: O-level	-0.505*** (0.19)	-0.734*** (0.23)	-0.81*** (0.23)	-0.741*** (0.23)	-0.676*** (0.22)
Highest qualification: A-level	-1.077*** (0.23)	-1.461*** (0.27)	-1.192*** (0.27)	-1.16*** (0.27)	-1.282*** (0.27)
Highest qualification: Other	-0.845*** (0.31)	-0.57 (0.37)	-0.071 (0.37)	-0.053 (0.37)	-0.14 (0.36)
Number of children: Two +	0.009 (0.16)	-0.052 (0.19)	-0.034 (0.19)	-0.131 (0.19)	-0.245 (0.18)
Worked for 12 or fewer months in the past three years	1.132*** (0.26)	1.061*** (0.31)	0.906*** (0.31)	0.674** (0.31)	0.703** (0.30)
Worked 13-24 months in the past three years	0.967*** (0.29)	0.244 (0.35)	0.013 (0.35)	-0.26 (0.34)	-0.103 (0.34)
Worked in the past year	-1.201*** (0.27)	-0.873*** (0.32)	-0.729** (0.32)	-0.729** (0.32)	-0.545* (0.31)
Weekly earnings in the past year, most recent job	0.003 (0.00)	0.004 (0.00)	0.003 (0.00)	0.003 (0.00)	0.002 (0.00)
RA in October 2003-December 2003	1.603*** (0.26)	1.062*** (0.32)	1.182*** (0.32)	1.003*** (0.32)	0.78** (0.31)
RA in January 2004-March 2004	1.074*** (0.18)	0.462** (0.22)	0.583*** (0.22)	0.503** (0.21)	0.327 (0.21)
RA in April 2004-June 2004	0.589*** (0.20)	0.194 (0.24)	0.294 (0.24)	0.492** (0.24)	0.289 (0.23)
Number of months on IS in two years pre-RA	0.09*** (0.01)	0.083*** (0.01)	0.096*** (0.01)	0.093*** (0.01)	0.079*** (0.01)
Youngest child less than six at RA	0.339* (0.18)	0.578*** (0.22)	0.774*** (0.22)	0.871*** (0.21)	1.239*** (0.21)
Aged 30-39 years old at RA	-0.442** (0.19)	-0.429* (0.23)	-0.689*** (0.24)	-0.656*** (0.23)	-0.427* (0.23)

Continued

Table A.1 Continued

Variable	Year 1	Year 2	Year 3	Year 4	Year 5
Aged 40 + at RA	-0.683*** (0.26)	-0.548* (0.31)	-0.948*** (0.31)	-1.15*** (0.31)	-0.858*** (0.30)
Non-white	1.142*** (0.24)	1.483*** (0.28)	0.878*** (0.28)	0.707** (0.28)	0.635** (0.27)
Index of multiple deprivation	0.014*** (0.00)	0.021*** (0.01)	0.021*** (0.01)	0.014*** (0.01)	0.013*** (0.00)
ERA*female	-0.495 (0.49)	-0.951 (0.59)	-1.432** (0.59)	-0.757 (0.59)	-0.374 (0.58)
ERA*single	-0.216 (0.26)	0.195 (0.31)	0.388 (0.31)	-0.12 (0.31)	-0.267 (0.30)
ERA*O-level	-0.218 (0.27)	-0.463 (0.32)	-0.352 (0.33)	-0.442 (0.32)	-0.393 (0.32)
ERA*A-level	-0.39 (0.32)	-0.61 (0.39)	-0.882** (0.39)	-0.916** (0.38)	-0.387 (0.38)
ERA*other educational qualification	0.589 (0.44)	-0.699 (0.53)	-1.028* (0.53)	-1.334** (0.53)	-1.034** (0.52)
ERA*more than one child	0.087 (0.22)	0.207 (0.27)	0.13 (0.27)	0.054 (0.27)	0.367 (0.26)
ERA*worked for 12 or fewer months in the past three years	-0.949*** (0.36)	-0.332 (0.43)	0.089 (0.43)	0.264 (0.43)	0.206 (0.42)
ERA*worked for 13-24 months in the past three years	-0.624 (0.40)	0.221 (0.49)	0.028 (0.49)	0.65 (0.48)	0.656 (0.48)
ERA*worked in the past year	0.383 (0.36)	-0.017 (0.44)	0.208 (0.44)	0.005 (0.44)	-0.044 (0.43)
ERA*weekly earnings in the past year for most recent job	-0.005* (0.00)	-0.002 (0.00)	-0.004 (0.00)	-0.002 (0.00)	0.00 (0.00)
ERA*RA in October 2003-December 2003	-0.385 (0.37)	-0.648 (0.45)	-0.719 (0.45)	-0.374 (0.45)	-0.419 (0.44)
ERA*RA in January 2004-March 2004	-0.047 (0.25)	0.379 (0.30)	0.021 (0.31)	0.134 (0.30)	0.102 (0.30)
ERA*RA in April 2004-June 2004	-0.07 (0.28)	0.257 (0.34)	-0.031 (0.34)	-0.329 (0.34)	-0.388 (0.33)
ERA*number of months on IS in two years pre-RA	0.02 (0.01)	0.012 (0.02)	-0.025 (0.02)	-0.018 (0.02)	-0.009 (0.02)
ERA*youngest child in household less than six at RA	0.250 (0.25)	-0.001 (0.31)	-0.323 (0.31)	-0.201 (0.30)	-0.5* (0.30)
ERA*aged 30-39 years old at RA	-0.312 (0.28)	-0.52 (0.33)	-0.273 (0.33)	-0.184 (0.33)	-0.749** (0.32)

Continued

Table A.1 Continued

Variable	Year 1	Year 2	Year 3	Year 4	Year 5
ERA*aged 40+ at RA	-0.626*	-1.009**	-0.772*	-0.362	-0.94**
	(0.36)	(0.44)	(0.44)	(0.44)	(0.43)
ERA*non-white	-0.602	-0.658*	-0.175	-0.301	-0.44
	(0.32)	(0.39)	(0.39)	(0.38)	(0.37)
ERA*index of multiple deprivation	0.007	-0.013*	-0.017**	-0.012*	-0.007
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Adviser caseload	0.014**	0.014***	0.015***	0.009*	0.006
	(0.01)	(0.01)	(0.00)	(0.01)	(0.00)
% think long term – control	2.37*	1.442	0.107	0.233	0.293
	(1.35)	(1.27)	(1.22)	(1.25)	(1.12)
% education help – control	2.896***	2.796***	2.246**	1.719*	1.299
	(0.93)	(0.91)	(0.88)	(0.89)	(0.82)
% think advancement – control	-1.337	-2.03*	-3.213***	-3.258***	-2.279**
	(1.35)	(1.22)	(1.16)	(1.19)	(1.06)
% much support while working – control	-0.919*	-1.324***	-0.823*	-0.281	-0.016
	(0.55)	(0.50)	(0.48)	(0.49)	(0.44)
IMP: proportion of advisers working on ERA	-0.332	-0.273	-1.635*	-1.561*	-1.179
	(0.75)	(0.88)	(0.85)	(0.85)	(0.81)
IMP: % think long term – diff	1.176	0.731	0.842	1.164	0.554
	(0.90)	(1.05)	(1.01)	(1.01)	(0.96)
IMP: % education help – diff	0.85	1.397	1.219	0.493	0.306
	(0.73)	(0.87)	(0.83)	(0.83)	(0.79)
IMP: % think advancement – diff	-1.406	-0.964	-2.066*	-2.196**	-1.143
	(0.96)	(1.12)	(1.10)	(1.09)	(1.05)
IMP: % much support (work) – diff	-0.517	-0.981	-1.036*	-0.76	-0.33
	(0.51)	(0.61)	(0.59)	(0.59)	(0.57)
IMP: % aware retention bonus – prog	-5.48***	-5.019**	-6.729***	-5.735***	-5.785***
	(1.88)	(2.15)	(2.07)	(2.07)	(1.96)
_cons	7.776***	5.778***	4.896***	4.342***	3.865***
	(0.14)	(0.12)	(0.11)	(0.12)	(0.10)
Ins1_1_1					
sd(T)	0.00**	0.168	0.003	0.002*	0.00
	(0.00)	(0.39)	(0.01)	(0.01)	(0.00)
Ins1_1_2					
sd(_cons)	0.61**	0.386***	0.342***	0.377***	0.264**
	(0.12)	(0.13)	(0.13)	(0.13)	(0.15)
Insig_e					
_cons	4.23***	5.118***	5.133***	5.069***	4.976***
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
log likelihood	-19,372	-20,639.7	-20,656.6	-20,573.2	-20,446.2

Continued

Table A.1 Continued

Variable	Year 1	Year 2	Year 3	Year 4	Year 5
χ_2 for test versus linear regression	35.34***	6.33**	3.70	4.19	1.24
p-level for test versus linear regression	0.00	0.042	0.157	0.123	0.539
N	6,754	6,754	6,754	6,754	6,754

Table A.2 Results by year for number of months employed

Variable	Year 1	Year 2	Year 3	Year 4	Year 5
1 = ERA	0.091 (0.11)	0.092 (0.13)	0.009 (0.13)	-0.052 (0.13)	-0.006 (0.15)
1 = Female	1.496*** (0.39)	1.501*** (0.43)	1.183*** (0.44)	1.05** (0.44)	0.879** (0.44)
1 = Never partnered	-0.1 (0.20)	0.08 (0.22)	0.165 (0.23)	-0.25 (0.23)	-0.35 (0.23)
Highest qualification: O-level	0.348* (0.21)	0.453** (0.23)	0.634*** (0.24)	0.611*** (0.24)	0.605** (0.24)
Highest qualification: A-level	0.768*** (0.25)	0.979*** (0.27)	0.719** (0.28)	0.623** (0.28)	0.546* (0.28)
Highest qualification: Other	0.328 (0.34)	-0.001 (0.37)	-0.121 (0.38)	-0.247 (0.38)	-0.358 (0.39)
Number of children: Two +	0.126 (0.17)	0.241 (0.19)	0.168 (0.19)	0.087 (0.20)	0.342* (0.20)
Worked for 12 or fewer months in the past three years	-1.699*** (0.28)	-1.424*** (0.31)	-1.203*** (0.32)	-0.922*** (0.32)	-0.923*** (0.32)
Worked 13-24 months in the past three years	-1.045*** (0.32)	-0.859** (0.35)	-0.358 (0.36)	0.185 (0.36)	0.249 (0.36)
Worked in the past year	1.642*** (0.29)	1.391*** (0.32)	1.247*** (0.33)	1.082*** (0.33)	0.859** (0.33)
Weekly earnings in the past year, most recent job	-0.004* (0.00)	-0.004 (0.00)	-0.004 (0.00)	-0.005* (0.00)	-0.005 (0.00)
RA in October 2003-December 2003	-1.006*** (0.29)	-0.371 (0.32)	-0.64* (0.33)	-0.688** (0.33)	-0.654** (0.33)
RA in January 2004-March 2004	-0.565*** (0.20)	0.246 (0.22)	0.106 (0.22)	-0.096 (0.22)	-0.001 (0.22)
RA in April 2004-June 2004	-0.603*** (0.22)	0.186 (0.24)	-0.128 (0.25)	-0.399 (0.25)	-0.136 (0.25)
Number of months on IS in two years pre-RA	0.009 (0.01)	-0.02 (0.01)	-0.036*** (0.01)	-0.036*** (0.01)	-0.038*** (0.01)
Youngest child less than six at RA	-0.249 (0.20)	-0.202 (0.22)	-0.218 (0.22)	-0.376* (0.22)	-0.621*** (0.23)
Aged 30-39 years old at RA	0.059 (0.21)	0.298 (0.23)	0.544** (0.24)	0.345 (0.24)	0.244 (0.24)

Continued

Table A.2 Continued

Variable	Year 1	Year 2	Year 3	Year 4	Year 5
Aged 40 + at RA	0.385 (0.28)	0.744** (0.31)	1.08*** (0.32)	0.698** (0.32)	0.269 (0.32)
Non-white	-0.953*** (0.25)	-0.946*** (0.27)	-0.643** (0.28)	-0.576** (0.29)	-0.452 (0.29)
Index of multiple deprivation	-0.007 (0.00)	-0.012** (0.01)	-0.013** (0.01)	-0.008 (0.01)	-0.012** (0.01)
ERA*female	0.139 (0.54)	0.425 (0.59)	0.575 (0.61)	0.684 (0.61)	0.978 (0.62)
ERA*single	-0.033 (0.28)	-0.563* (0.31)	-0.509 (0.32)	0.13 (0.32)	0.151 (0.33)
ERA*O-level	0.311 (0.30)	0.461 (0.32)	0.278 (0.33)	0.121 (0.34)	0.197 (0.34)
ERA*A-level	0.578 (0.35)	0.978** (0.39)	1.27*** (0.40)	1.23*** (0.40)	0.765* (0.40)
ERA*other educational qualification	-0.46 (0.48)	0.798 (0.53)	0.984* (0.55)	1.554*** (0.55)	1.552*** (0.55)
ERA*more than one child	-0.297 (0.25)	-0.277 (0.27)	-0.035 (0.28)	0.059 (0.28)	-0.386 (0.28)
ERA*worked for 12 or fewer months in the past three years	0.462 (0.39)	0.276 (0.43)	0.032 (0.44)	-0.372 (0.45)	0.151 (0.45)
ERA*worked for 13-24 months in the past three years	0.476 (0.45)	0.441 (0.49)	-0.262 (0.51)	-0.721 (0.51)	-0.741 (0.51)
ERA*worked in the past year	-0.608 (0.40)	-0.783* (0.44)	-0.755* (0.45)	-0.903** (0.46)	-0.638 (0.46)
ERA*weekly earnings in the past year for most recent job	0.005 (0.00)	0.002 (0.00)	0.005 (0.00)	0.007** (0.00)	0.005 (0.00)
ERA*RA in October 2003-December 2003	-0.223 (0.41)	0.104 (0.45)	0.093 (0.46)	0.083 (0.47)	0.117 (0.47)
ERA*RA in January 2004-March 2004	-0.337 (0.28)	-0.448 (0.31)	-0.247 (0.31)	-0.184 (0.31)	-0.423 (0.32)
ERA*RA in April 2004-June 2004	0.228 (0.31)	-0.141 (0.34)	-0.076 (0.35)	0.27 (0.35)	0.084 (0.35)
ERA*number of months on IS in two years pre-RA	0.002 (0.02)	-0.001 (0.02)	0.01 (0.02)	0.003 (0.02)	0.004 (0.02)
ERA*youngest child in household less than six at RA	0.023 (0.28)	-0.182 (0.31)	-0.172 (0.32)	-0.023 (0.32)	0.238 (0.32)
ERA*aged 30-39 years old at RA	0.394 (0.30)	0.322 (0.33)	0.257 (0.34)	0.142 (0.34)	0.389 (0.35)

Continued

Table A.2 Continued

Variable	Year 1	Year 2	Year 3	Year 4	Year 5
ERA*aged 40+ at RA	0.528 (0.40)	0.249 (0.44)	0.205 (0.45)	0.317 (0.45)	0.84* (0.46)
ERA*non-white	0.763** (0.35)	0.587 (0.39)	0.398 (0.40)	0.127 (0.40)	0.455 (0.40)
ERA*index of multiple deprivation	0.012* (0.01)	0.02*** (0.01)	0.019*** (0.01)	0.007 (0.01)	0.004 (0.01)
Adviser caseload	-0.014*** (0.00)	-0.005 (0.00)	-0.011** (0.00)	-0.008* (0.00)	-0.004 (0.00)
% think long term – control	-2.747*** (1.00)	-1.056 (1.10)	-1.219 (1.14)	-0.772 (1.17)	0.23 (1.25)
% education help – control	-2.749*** (0.74)	-2.199*** (0.81)	-1.354 (0.84)	-0.737 (0.86)	-1.492 (0.91)
% think advancement – control	1.887** (0.94)	2.469** (1.03)	2.41** (1.07)	1.211 (1.10)	0.739 (1.18)
% much support while working – control	0.692* (0.40)	0.84* (0.43)	0.717 (0.45)	0.429 (0.46)	0.316 (0.49)
IMP: proportion of advisers working on ERA	0.026 (0.75)	-0.163 (0.86)	1.472* (0.85)	0.903 (0.86)	1.089 (0.96)
IMP: % think long term – diff	-2.335*** (0.88)	-0.229 (1.01)	-0.779 (1.00)	-0.631 (1.01)	0.192 (1.14)
IMP: % education help – diff	-0.743 (0.73)	-1.161 (0.84)	-0.915 (0.82)	-0.702 (0.83)	-0.77 (0.96)
IMP: % think advancement – diff	2.105** (0.98)	1.568 (1.10)	2.926*** (1.10)	2.449** (1.12)	0.014 (1.20)
IMP: % much support (work) – diff	0.444 (0.53)	0.919 (0.60)	0.792 (0.60)	0.806 (0.60)	1.427** (0.67)
IMP: % aware retention bonus – prog	5.107*** (1.80)	2.31 (2.06)	4.48** (2.03)	2.719 (2.07)	2.852 (2.33)
_cons	4.49*** (0.09)	5.062*** (0.10)	5.076*** (0.10)	5.169*** (0.11)	5.425*** (0.11)
Ins1_1_1 sd(T)	0.00** (0.00)	0.217 (0.28)	0.00*** (0.00)	0.00 (0.05)	0.338 (0.00)
Ins1_1_2 sd(_cons)	0.19** (0.16)	0.185* (0.16)	0.217** (0.16)	0.257** (0.16)	0.306 (0.00)
Insig_e _cons	4.68*** (0.04)	5.124*** (0.04)	5.282*** (0.05)	5.3*** (0.05)	5.331 –
log likelihood	-20,032.7	-20,641.5	-20,844.6	-20,868.9	-20,912.7

Continued

Table A.2 Continued

Variable	Year 1	Year 2	Year 3	Year 4	Year 5
χ_2 for test versus linear regression	0.51	1.32	0.75	0.96	1.49
p-level for test versus linear regression	0.775	0.518	0.687	0.619	0.802
<i>N</i>	6,754	6,754	6,754	6,754	6,754

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The UK Employment Retention and Advancement (ERA) demonstration examined the extent to which a combination of post-employment advisory support and financial incentives could help low-income individuals to find sustained employment with prospects for advancement. ERA was experimentally tested across more than 50 Jobcentre Plus offices and, within each office, individuals were randomly assigned to either a 'treatment' group (eligible for ERA) or a control group (not eligible). Such a design enables internally valid estimates of the impact of ERA for individual offices.

This report presents the results of an analysis that examines the variation in these office-level impacts and attempts to understand what factors tend to be associated with positive impacts. The focus in particular is on the extent to which estimated impacts vary according to which elements of the overall ERA package individual offices appeared to emphasise the most. In this way, some insight into which elements of ERA are most effective becomes possible. The analysis is carried out for lone parents receiving Income Support and volunteering for the New Deal for Lone Parents.

The analysis suggests that an emphasis on in-work advancement, support while working and the employment retention bonus results in greater impacts on employment and benefit receipt. On the other hand, encouraging individuals to focus on long-term aims or encouraging education appears to do little to influence impacts over the period considered.

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