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# Human Well-being and In-Work Benefits: A Randomized Controlled Trial

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# Human Well-being and In-Work Benefits: A Randomized Controlled Trial

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# ABSTRACT

## Human Well-being and In-Work Benefits: A Randomized Controlled Trial*

Many politicians believe they can intervene in the economy to improve people's lives. But can they? In a social experiment carried out in the United Kingdom, extensive in-work support was randomly assigned among 16,000 disadvantaged people. We follow a sub-sample of 3,500 single parents for 5 ensuing years. The results reveal a remarkable, and troubling, finding. Long after eligibility had ceased, the treated individuals had substantially lower psychological well-being, worried more about money, and were increasingly prone to debt. Thus helping people apparently hurt them. We discuss a behavioral framework consistent with our findings and reflect on implications for policy.

JEL Classification: I31, D03, D60, H11, J38

Keywords: randomized controlled trials, government policy, in-work benefits, wage subsidies, well-being, happiness

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Human Well-being and In-Work Benefits: A Randomized Controlled Trial

*"Statistical offices [worldwide] should incorporate questions to capture people's life evaluations, hedonic experiences and priorities."* p.16. Executive Summary of the Stiglitz-Sen-Fitoussi Commission Report on the Measurement of Social and Economic Progress, 2009. www.stiglitz-sen-fitoussi.fr

*"There is ... a tendency to regard any existing government intervention as desirable."* Milton Friedman, Capitalism and Freedom, University of Chicago Press, 1962.

## 1. Introduction

Economic and social policies in western society are rarely based on the kinds of evidence required in fields such as medical science. How high to set the income-tax rate, whether to pay generous assistance to unemployed workers, what sorts of divorce laws to implement, how to regulate banks -- these types of decisions have been shaped historically by politicians' intuitions and the lobbying of advisors. By building upon new strands within the quantitative social-science literature, particularly research on well-being (Di Tella et al. 2001; Easterlin 2003; Stiglitz et al. 2009; Oishi et al. 2012; Adler and Posner 2008; Ifcher 2011; Ifcher and Zarghamee 2011; Dolan and Metcalfe 2012; Layard 2006; Helliwell and Huang 2008; Benjamin et al. 2012; Graham and Nikolova 2013; Stevenson and Wolfers 2013; Oswald et al. 2014), and upon insights from an important modern literature on randomized trials (Burtless 1995; Gintis 2000; Harrison and List 2005; List 2006; Dunn et al. 2008; Ludwig et al. 2011), this paper is an attempt to pursue an alternative approach of evaluation by randomized controlled trial. The analysis links also to issues of self-control (Thaler and Shefrin 1981). We study a major social-science experiment run by the government of the United Kingdom -- a randomized controlled trial that offered incentives to disadvantaged people to remain and advance in work and to become self-sufficient. The RCT provided in-work benefits to a treatment group (in-work policies are discussed in Pencavel 1986, Eissa and Hoynes 2004, Bargain and Orsini 2006, and Brewer et al. 2009).

Our main finding is that the intervention led to *significant falls -- when measured after 5 years -- in the reported well-being levels of those people in the treatment group* even though on average those individuals ended with higher earnings than the control group. People became less happy with their lives and worried more. Six well-being measures are available in our data set. Because of the multiple-comparisons problem of applied statistics, and to obviate the need for Bonferroni or equivalent corrections, results for all six measures are presented in the main body of the paper or in the Appendix (which is divided into three sections, A, B, and C).

Each of the six measures points to substantially lower well-being. In four of these the negative effects are individually significantly different from zero at the 1% or 5% significance levels. The randomized intervention had no discernible effects on hours worked (measured at Year 5). Hence no detailed tables are given later on that dimension of behavior. They are available upon request. Because earnings increased in the treatment group, the treated individuals could be said to be in higher-effort, or better, jobs. We return to this below.

As part of the study, we checked that the observable demographic characteristics of the treatment and control groups had not altered in Year 5. A later part of the Appendix also tests for the possibility of attrition bias caused by unobservables.

Why was the well-being of the treatment group reduced by the policy? That is a fundamental puzzle for social scientists and remains to be completely understood. One possibility, in the broad spirit of prospect theory (Kahneman and Tversky 1979), is that the removal of temporary state benefits hurts asymmetrically more than the initial gain from those benefits. There may also here be some conceptual connection with the negative findings, about criminality, in early research described in McCord (2007). One later section of our paper explores the structure of a formal account. A section of the Appendix also summarizes different reactions within the treatment group.

## 2. The Nature of the Randomized Trial

In the experiment (known as the Employment and Retention Advancement, or ERA, Demonstration), individuals were assigned in a randomized controlled trial to one of two groups -- either to a treatment group who were given additional incentives and support to take full-time work or to a control group who were not. In total, approximately 16,000 individuals were initially randomized, making this the largest social experiment undertaken in the UK (Hendra et al. 2011; Haynes et al. 2012). The results in this paper focus on a random sub-sample of 3,500 single mothers followed up in telephone and face-to-face interviews both at 2 years and at 5 years after the initial policy intervention. There were a small number (3%) of single fathers in the sample of single parents; for reasons of simplicity and homogeneity of sample these are omitted from the later calculations. If the single fathers are included in the later analysis, it makes no substantive difference to the study's conclusions. No survey data for Year 5 were collected on the remaining 12,500 people. That is why our study is of single mothers.

We draw in part upon a tradition of research -- across the fields of psychology, decision science, medical science, economics, and other behavioral sciences -- that uses questionnaire data on people's well-being. These usually take the form of numerical scores in response to survey questions such as: "how happy are you with your life overall" or "did you yesterday have moments of anxiety or of feeling depressed"? Sample sizes in published statistical analyses vary from a few dozen individuals in a laboratory to hundreds of thousands of people in a household survey. It is known that there are reasons to treat such data seriously and that there is evidence of a match between objective and subjective scores. This study focuses particularly upon life-satisfaction data. Other forms of well-being and positive-affect information can be used (Stone et al. 2010; Oswald and Wu 2010). Our later tables lay out results for a range of subjective scores such as the level of worry about debt.

For clarity, some of this paper's detailed tables are relegated to an Appendix. The key statistical results of the study are presented in Tables 1 and 2.

Outcomes in Year 5 are of special interest, so they are our focus in this paper. There are two reasons. One is that some payments were still being made to the experimental subjects at the end of the second year, which thus complicates inference in Year 2. By the fifth year, however, all payments and assistance to the treatment group had stopped. Hence Year 5 allows a clean comparison. A second reason is that a major question for the western governments is: can a policy of temporary in-work support help to foster long-run psychological and economic gains for their citizens?

In this paper the treated subjects are compared to equivalent people in the control group of that year. The conclusions are the following. First, the treatment increased Year-5 earnings and, for an initial period before Year 5, the chance of being in full-time work. Table 1 summarizes the economic outcomes. The ERA intervention raised people's earnings, five years afterwards, by approximately 10 pounds (about 15 US dollars) per week. In this sense, the results are more positive than some found earlier (Foley and Schwartz 2003; Card and Hyslop 2009). They are also slightly more positive than the key earnings impacts in the official evaluation report (Hendra et al. 2011) – see Section 1 of the Appendix for further details. However, the principal contribution here is to attempt to go beyond pecuniary consequences to try to understand broader effects of in-work benefits upon human well-being. In Year 5, the treatment had no statistically significant effects on people's hours worked. The point estimate, when comparing the treatment group with the control group, was 0.8 extra hours a week, with a large standard error (at Year 2, the effect was approximately one and half hours, significant at the 5% level). This finding continued to hold with tobit and other estimation methods. Hence, the later sections do not attempt to report detailed results for hours worked. Second, when compared to the control group, after five years the people who had been randomly assigned to the ERA treatment group had substantially lower satisfaction with their lives, perceived their financial situation as worse, ran out of money more often, worried about money to a greater extent, had more trouble with debts, and were less likely to have money left over at the end of the week. These were the individuals who were given extra public money and assistance. Helping them apparently hurt them.

Table 2 gives the randomized trial's key outcomes (other detailed findings are in the Appendix). The negative effect on life satisfaction in Year 5 is approximately -0.1 points. That drop relative to the control group is substantial. It is -- see Appendix A -- approximately half the size of the effect of having no educational qualifications compared to having passed advanced high-school exams. Life satisfaction in the econometric analysis is measured on a cardinal five-point scale. However, switching to ordinal estimators such as probit equations makes no substantive difference. The mean level of life satisfaction in Year 5 is 3.62 with a standard deviation of 1.07. It might be felt that a 0.1 effect is reasonably small. But such intuition would be misleading because the standard deviation here is driven by people's cross-sectional variation in answers. In fact, the in-work support made available under ERA apparently created substantial and long-term psychic costs. The effects were strongest outside London.

Table 2 suggests that the negative consequences work principally through greater financial worries. One potential interpretation is that giving people temporary subsidies in Year 1 and Year 2 created aspirations and a lifestyle that were impossible to sustain.

## 3. The Intervention in Greater Detail

Individuals in the treatment group were given help in three broad ways. First, participants in ERA had access to special 'post-employment' job coaching. Second, they were given strong financial incentives to work. Third, they were given training opportunities. All these were added, in effect, to the standard benefits available to anyone in the UK and to the job placement services ordinarily available through unemployment offices. The intervention was designed to add to the understanding achieved from experimental research carried out in the US and Canada (Foley and Schwartz 2003; Card and Hyslop 2009; Rangarajan and Novak 1999; Gennetian et al. 2005; Huston et al. 2003; Michalopoulos et al. 2002; Hendra et al. 2010).

The job coaching available under the ERA experiment took the form of advice and assistance from an 'Advancement Support Adviser', specially trained to help individuals remain and advance in work. Those who did so could receive substantial cash rewards, called 'retention bonuses'. These formed a key element of the ERA support (Dorsett and Robins 2014). They were based on a 17-week accounting period. Individuals working 30 hours or more per week for 13 out of 17 weeks received a tax-free payment of £400. This works out at about £1 per hour for an individual working 30 hours a week for just 13 weeks. It can be compared to an average hourly wage of about £8 for those in work at the time of the year-5 survey interview (Hendra et al. 2011). This is approximately 12.5%. Each individual could receive a maximum of six bonuses over a period of up to 33 months after randomization. ERA eligibility ended at this point for everyone in the treatment group, regardless of what use they had made of the support on offer. Lastly, ERA encouraged training by providing help with tuition costs and offering cash rewards for completing training courses while employed.

A number of steps were taken to ensure a high response rate and to keep track of respondents. Respondents were given a £20 voucher in return for their cooperation. Individuals in the survey sample were sent pre-contact letters (first done 6 months after the randomization) setting out the purpose of the study and the survey, explaining about the £20, giving a confidentiality assurance, and enclosing a postcard to inform of changes in contact details. Another letter was sent 8 days before the start of fieldwork. Interviewing was managed by the Office of National Statistics and was carried out by telephone, with non-contacts and refusals re-issued to face-to-face interviewers. Details were recorded of 3 other people who could be approached in case there were difficulties contacting the person.

Prior to the randomization, information was collected on individuals' baseline characteristics. People's subsequent employment, earnings and welfare outcomes were tracked by using a mixture of administrative records and surveys. Three surveys were carried out -- approximately one, two and five years after randomization. The timing of these surveys was such that the first two fell within the period of ERA eligibility, while the last survey, held in year 5, was a substantial period after all ERA participation and payments had ended. This timing allows the effects of ERA -- both during and beyond the period of eligibility -- to be examined.

Appendix Table A1 describes basic background information about the sample used in the calculations. The characteristics of the participants in (both of) the Year 2 and Year 5 surveys are shown. In total, the sample consists of 3,335 respondents. At the start of the study these individuals were all disadvantaged single mothers, either out of work or working part-time. Approximately 40% of participants had worked for fewer than 12 months out of the previous 36 months. Importantly, it can be seen from Table A1 that the mean characteristics of the treatment group and the control group are almost identical. Some differences are to be expected as a result of random variation, particularly as smaller subsamples are considered. For example, the third and fourth columns of Table A1, which give data for the area of London on its own, show weekly earnings prior to randomization to differ between the control and treatment groups (the means are 52.94 and 63.91 pounds). However, the difference falls short of statistical significance at conventional levels (a two-tailed t-test gives a p-value of 0.175).

Some of the paper's calculations are carried out for the regions of the UK excluding London. The reason for wishing to do this is that market wages are typically considerably higher in London than elsewhere, so the retention bonuses paid to the treatment group as part of the ERA experiment represent a considerably greater proportionate increase outside the capital (Table A1 suggests lower mean pay in London than elsewhere in the year before randomization, but that is an illusion caused by a lower employment rate in London).[1]

Table A3 gives in more detail the positive effect on earnings; Tables A4-A9 show the negative effects on life satisfaction, perceived financial situation, running out of money, worry, trouble with debts, and cash left at the end of the month. As would be expected -- assuming the randomization had been done effectively -- these tables suggest that there is almost no difference between the raw estimate of the treatment effect and the estimate after adjustment for people's observed characteristics.

One issue raised in seminar presentations of this work was how different kinds of individuals within the treatment group fared over the ensuing 5 years. Table B1 reports data on this. The dependent variable is the change in life satisfaction. Gainers tend to be those initially

[1] Of course, since costs are also higher in London than elsewhere, it may be that, as a proportion of net income, the regional variations are less marked.

in part-time work (see the first column of Table B1). Those with higher satisfaction at year 2 saw the greatest fall (column 2 of Table B1), although some of this may be explicable as simple mean-reversion.

Some other information is relevant. First, with regard to take-up of the financial bonuses, these are for those in the treatment group who responded to the 5-year survey. Only those individuals who worked full-time for sufficiently long could receive the bonus, so non-receipt cannot be regarded as these individuals ignoring the policy. Approximately 36% received a bonus. Of those who did, 90% continued after the bonus payments ended to work the same hours per week as when they were collecting bonuses. Of those who changed their hours after the bonus payments ended, 19% worked more hours, 44% worked fewer hours, and 37% stopped working altogether. These percentages are based on just 57 individuals (the 10% who did not continue to work the same hours). However, among who changed their hours following the end of the bonus, only 9% reported that this was a direct result of the bonuses ending. Far more commonly it was due to some other reason.

It is also interesting to consider what might have happened to fertility. The survey does not capture precise ages of children post-randomization. However, it does ask about the number of children under 5. Since this is a Year-5 survey (approximately), this could be expected to capture any effect on additional children as a result of the ERA treatment. In fact, there does not appear to be any effect. In both the treatment and control groups, 16% of women have a child under the age of 5. Nor was there any apparent effect on partnering: 22% of women reported living with someone and this was the same for the treatment and control groups.

## 4. Conceptual Issues

One issue for economists and behavioral scientists is how conceptually to make sense of the main empirical finding of the study. Some possible analytics are set out below. The results of the randomized trial hold independently of a model, of course, so deductive reasoning is able only to offer ex post theoretical ideas that will have to be scrutinized in detail in future research inquiries. Nevertheless, it is perhaps worth speculating on theoretical structures.

Assume that individuals have a utility function which depends on income and the effort the person puts in at work. Effort is not hours; it is intensity. Assume that the cost of effort, $e$, can be summarized by a convex and increasing function $c(e)$. Define net utility, $V$, as the difference between the utility from income and the cost of effort. For simplicity, let earned income be thought of as the product of effort, $e$, times an earnings piece-rate, $p$. This is more general than the traditional assumption of an income-hours trade-off; it allows for the possibility that people choose high-intensity jobs in return for greater wages.

*Non-workers*

Assume that some individuals find it optimal not to work. They receive non-labor income $Y$. Their effort level, $e$, is effectively zero. Assume their total income is uncertain, but that there is a certain (that is, riskless) unemployment-benefit payment, $b$. Assume that with probability $\alpha$ they also receive -- perhaps in gifts from family or friends or in payment for black market work they do not declare -- an extra amount of income, $y$. But assume that with probability $1-\alpha$ they receive nothing from this source.

Individuals must decide on their consumption spending while bearing in mind their likely income flows and the uncertainty surrounding those. In total, the expected income of non-workers is particularly simple:

$$EY = b + y\alpha + (1-\alpha)0 = b + y\alpha$$

$= c$ *Consumption of non-workers*

where $c$ in this equation is defined to be consumption, which is thus assumed set equal to expected income. This formulation implies that on occasions the individual will run out of money. More precisely, for those individuals who do not take a paid job, the probability of running short of cash is $1 - \alpha$.

*Workers*

Consider those who take a job. Think of them as earning an amount given by their effort times the piece rate for that particular job. Like non-workers, assume they get some random unearned income amount, $y$. Workers get employment income of $pe$. Assume also that there is a government subsidy, $s$, that is payable to those workers who hold a job and not to those out of work. This subsidy is temporary. It is positive in the first period and becomes zero in the second period. Workers get utility from these income flows.

Consider workers as potentially caring not just about their own absolute income but also as having a reference level of income, $r$. Assume that -- consciously or subconsciously -- people compare their earnings to that level. Assume that, in part, workers get utility, in an increasing and diminishing way, from the gap between what they achieve and this benchmark amount against which they compare their earnings.

Let utility depend potentially on a convex combination of absolute earnings, $pe$, and of earnings relative to the reference level, $pe - r$. Let the weights on these two be $z$ and $1-z$. Therefore, in the classical textbook case, $z$ would be unity, and people would not compare at all to a reference level. By contrast, in a world of extreme reference comparisons, $z$ is zero.

Here 'net' utility can be thought of as being given by the difference between the utility from money and the costs of effort. Write it

$V = v(y + zpe + (1\text{-}z)(pe - r) + s) - c(e).$

This can be simplified in the following way. Define the weighted reference term $(1\text{-}z)r$ as what might be termed the worker's 'aspired' or comparison income level and denote it $a$. Then net utility from the above equation can be rewritten as:

$V = v(y + pe - a + s) - c(e)$

= *utility from earned income - aspiration level + subsidy – costs of effort.*

where $v(.)$ is a concave, increasing function that is defined on total income, given by non-labor income $y$ plus earnings and subsidy, $pe + s$, less the aspiration level, $a$. In this case, a utility-maximizing employee chooses his or her effort level to balance the marginal utility gains from extra income against the marginal cost of extra effort.

Two forces act to push up the overall aspiration level, $a$, of earnings. One is if the worker has a lower $z$ (namely, a lower utility weight on pure absolute earnings, and thus a higher one on relative pay). The second is if the worker has an intrinsically higher $r$ (namely, a higher base reference level of income).

Assume utility is defined over two periods (the present and the future) and workers behave in an optimizing way. Let their effort levels in each period be respectively $e$ and $\zeta$. Assume that the piece-rate is defined on the unit interval, so the highest rate that can be earned is 1 and the lowest is zero. It is uncertain ex ante, so $p$ is characterized by a probability density function $f(p)$. Loosely, high values of the piece-rate $p$ might be thought of as corresponding to a boom in the economy. Assume the worker's utility over the two periods is given by

$$\int_0^1 \{Eu(y + pe + s) - c(e) + Ev(y + p\zeta - a(s)) - c(\zeta)\} f(p)dp.$$

This form of maximand captures the two periods with two utility functional forms, *u(.)* and *v(.)*. Without loss of generality, it normalizes aspiration levels by setting them equal to zero in the first period. In this specification, there are two random variables because *y* is random and *p* is random. The aspiration level is written *a(s)* by the assumption that high income subsidies today could lead to higher aspired income levels in the future.

In this notation, the people who find it desirable not to work are those with low marginal utility from income. They rely only on non-labor income, so over the two periods their utility is given by

*EU = Eu(y) + Ev(y)*    *A non-worker's utility*

Such individuals face no effort costs.

For those who find it optimal to work, the subsidy *s* is a non-differentiable function where above some effort level the amount of *s* is fixed. This means that workers may be at a corner where they are minimizing their effort subject to (just) being able to collect the subsidy s. Initially, however, consider an interior maximum. Then workers choose their effort, in each of the two periods, to ensure that the first-order conditions for an optimum are

$$\text{e}: \quad \int_0^1 \{Eu'(y+pe+s)p - c'(e)\} f(p)dp = 0$$

$$\zeta: \quad \int_0^1 \{Ev'(y+p\zeta - a(s))p - c'(\zeta)\} f(p)dp = 0 \,.$$

The more unusual equation is the second. By period 2, in this framework, workers have developed greater aspirations -- brought on by the higher income that was itself brought on by the government subsidy, *s*. This has, in a sense, altered their utility function. Intrinsically, now, people have a higher marginal utility from earned income, because they evaluate their income flow with respect to the new, and more stringent, aspiration level, *a*.

We then have the following results.

Proposition 1. Assume *a* is positive and sufficiently large. Assume that the structure of the person's first-period utility function *u(.)* is exactly, or sufficiently, similar to that of their second-period utility function *v(.)*. Then:

(i) Workers' effort levels in the second period $\zeta$ are as high as, or higher than, in the first period $e$.

(ii) Their utility is lower in the second period than in the first period (and also lower than the utility of the marginal non-worker).

(iii) Earnings remain high in the second period even though the subsidy has been removed.

(iv) In a significant class of cases, workers run out of money more in the second period than do non-workers.

The proof of (i), which helps establishes the key element of the other parts of the proposition, is by contradiction. Assume the reverse, namely, that $e > \zeta$. Then, by the convexity of the cost function,

$$c'(e) > c'(\zeta).$$

Therefore, rearranging the first-order conditions,

$$\int_0^1 \{Eu'(y + pe + s)p\} f(p)dp > \int_0^1 \{Ev'(y + p\zeta - a(s))p\} f(p)dp.$$

However, at any given p and y, it must be the case, by concavity of utility, and the fact that *s* is positive and *a(s)* is non-negative, that

$$u'(y + pe + s) < v'(y + p\zeta - a(s)).$$

Then, by the monotonicity of the E operator over uncertain *y*, we can take expectations of both sides and preserve the inequality sign:

$Eu'(y + pe + s) < Ev'(y + p\zeta - a(s))$.

Repeating the same step for the piece-rate distribution, by the monotonicity of the expectations operator it must be that

$$\int_0^1 \{Eu'(y + pe + s)p\} f(p)dp < \int_0^1 \{Ev'(y + p\zeta - a(s))p\} f(p)dp .$$

But by the first-order conditions this condition can only hold if $c'(e) < c'(\zeta)$ which in turn establishes the necessary contradiction.

It might be thought that, as a matter of accounting, all workers would set their consumption after the actual price *p* is known, but this framework allows for forward-looking consumption choices. What happens instead in this framework, therefore, is that those who work in the second period set their overall consumption level, *c**, equal to the expected income level, so

$$\textit{Consumption of a worker} = c^* = \alpha y + \zeta[\int_0^{p^*} pf(p)dp + \int_{p^*}^1 pf(p)dp]$$

where *p** plays a particular role explained below.

This formulation does not mean that individuals will never run out of income. They often will. To see this, it is helpful to define *p** as the piece-rate level at which workers just break even. Below *p**, they run short of cash. How often these workers run out of money will depend upon the covariance between the shocks to non-labor income *y* and the shocks to piece-rate *p*. But it is straightforward to see that there will be a class of cases where the probability of running out of money exceeds the rate among non-workers, which is rate $1- \alpha$. A trivial example of this is where *y* is arbitrarily small. Then any $p<p^*$ will result in the worker consuming more than is being earned at that point and hence being short of money.

Loosely, the more right-skewed is the *f(.)* distribution, the more often will a worker tend to run out of money. Using Markov's inequality, the expected piece-rate can be written as

$$\int_0^1 pf(p)dp \geq \int_0^{p^*} pf(p)dp + p^* \int_{p^*}^1 f(p)dp$$

and the greater is *f(.)* at the upper end of the unit interval the lower must be the value of $p^*$.

Point (ii) in the list earlier implies that $v(y + p\zeta - a(s)) - c(\zeta) < v(y + pe) - c(e)$. The change in period 2 utility resulting from an ERA-like intervention is then $[v(y + pe + p\Delta - a(s)) - v(y + pe)] - [c(e + \Delta) - c(e)]$, where $\Delta = \zeta - e$. Since utility and cost-functions are increasing, the first bracketed term is positive if $p\Delta > a(s)$ and the second bracketed term is positive. So, ERA will reduce period 2 utility if individuals do not increase their effort such that earnings go up at least as much as aspiration income (i.e. $p\Delta \leq a(s)$). However, we know that, without ERA, optimizing individuals choose their level of *e* such that any effort in excess of that level increases costs more than the positive utility element. It follows then that this also holds with ERA. So, under ERA, *e* in that period is set at the rational choice of effort but this is associated with lower utility than would be the case without ERA.

These results can be put in a more intuitive way. If the government offers a temporary subsidy to people who take a job, some individuals will respond to that incentive. They will choose to exert effort and earn more in the first period (that is, the period in which the subsidy applies) than the individuals who continue not to work. However, in the framework described here, for the workers who are persuaded into the workforce there is a kind of sting in the tail. If the first period of earning leads workers to revise up their aspirations, then in the second period they may make a different choice than they would have if they had never accepted the subsidy money. There are three consequences. The first is that the employees work hard in the second period and thus earn income. Their underlying motivation, it might be argued, has been altered:

their greater income aspirations mean that at any income level the marginal utility of earning is larger than it was before the subsidy scheme. The workers now feel they need extra money. Because they are less satisfied at each level of income than they were originally, it will be optimal to work intensively even after the subsidy has been withdrawn. However, all this comes at the expense of net utility. Workers are eventually less happy, even if earning the same as they were. In comparison to an individual who was indifferent between taking the subsidized job and not working, and who ultimately chose the latter, the workers who take the subsidy have lower utility in the second period than do the non-workers.

This analytical result has the flavor, although not the detail, of prospect theory. Losses loom larger than gains. Despite the period-1 advantage from taking the government subsidy, those workers -- who by then are no longer able to draw the subsidy -- have in period 2 to live with the curse of raised aspirations. Workers thus become richer but not happier.

In the interest of balance, it should be emphasized that the conceptual framework we develop, while potentially helpful in interpreting the findings of this study, has its own limitations. A truly general model would allow for the possibility that individuals' cost functions might also change over time. One of the hopes behind interventions such as ERA is that the support made available might help people overcome psychological and other barriers to employment. These barriers contribute to the 'costs' of employment initially but may feature less prominently once a fuller adjustment to working life has been made. In this scenario, the implications for utility and other outcomes become more complex. However, the pattern of results in the current study suggests that any such changes to subjective costs are likely to be of insufficient size to alter the insights from our simpler model.

## 5. Checks

Two empirical concerns deserve final consideration.

First, although this study's RCT is of a well-defined kind, a potentially interesting further question -- as a matter of statistical description rather than causal inference -- can be asked. Within the treatment group, who is particularly strongly affected by the intervention? We examine this in Appendix B.

Second, although in this study we are obliged to deal with the available data, in which people must voluntarily agree to fill up survey forms (we cannot compel them to do so), a potential weakness is that there might be selective attrition from the experiment over the ensuing five years in which we are especially interested. Attrition itself is not, in principle, a problem. However, differential rates of attrition from the treatment and control groups could be, because that might lead to biased estimates. On this, Appendix C provides some evidence that -- for our key finding that the quality of individuals' lives apparently worsens -- attrition bias is unlikely to be severe.

## 6. Conclusions

This paper has examined the outcome of a social-science experiment funded by the government of the United Kingdom. The study explores the hypothesis that the provision of temporary in-work benefits will produce an improvement in the quality of people's lives. Our results do not support that hypothesis. Methodologically, this inquiry rests on the use of randomized trials as one standard for reliable empirical knowledge, and it considers as a criterion for policy evaluation not just whether people become richer but whether they enjoy their lives more after a policy intervention. With honorable exceptions (such as Ludwig et al. 2012), there have been almost no large RCTs of economic policies in which well-being variables are taken as the primary outcome criteria.

The purpose of the randomized controlled trial was to discover how labor-market interventions to help disadvantaged workers might best be designed. The results reveal -- at least for an important illustrative case -- that traditional ways of making and evaluating labor market policy can produce striking, and potentially concerning, results. Years after the ERA randomized intervention finished, those in the treated group were less satisfied with life and worried more. From a well-being perspective, randomly assigning in-work benefits appears to have hurt rather than helped. This finding is not what most scholars would have predicted. It is, however, somewhat reminiscent of the negative results in the Cambridge-Somerville experiment in the late 1930s, in which disadvantaged boys assigned a mentor, and given other help, went on to have worse (criminal) outcomes than those in a control group. McCord (1978) suggested that the intervention in that sample may have created unrealistic expectations in the males in the treatment group, and that the ultimate effect was thus worse than no intervention. It is possible that an equivalent mechanism is at work here.

Although it cost multiple millions to fund, the inquiry described here is methodologically a simple one. One vision of the future of economics is that it develops into an experimental discipline in which RCTs of this kind become commonplace. In substantive terms, this study demonstrates the possibility of well-being effects that can run fundamentally counter to intuitive expectations. Future studies will have to aim to allow society to reach an informed view on whether, and if so how widely, such findings generalize.

## Table 1: Evidence of Higher Earnings in the Treatment Group by Year 5 of the Study (with Standard Errors in Parentheses)

*Notes: To interpret this table, a number like* 10.941 *in the top left-hand corner indicates that in Year 2 the treatment group earned 10.9 pounds a week more than those in the control group.*

- * Is significant at 90% confidence on a two-tailed test
- ** Is significant at 95%
- *** Is significant at 99%

| | **Great Britain, excl. London** | | | **Great Britain** | | |
|---|---|---|---|---|---|---|
| | **(1)** | **(2)** | **(3)** | **(1)** | **(2)** | **(3)** |
| *Weekly earnings* | | | | | | |
| Year 2 | 10.941 ** | 11.864 *** | 8.990 ** | 10.228 ** | 11.432 *** | 9.001 ** |
| | (4.38) | (4.23) | (3.93) | (4.29) | (4.15) | (3.83) |
| Year 5 | 9.903 * | 10.749 ** | 11.216 ** | 9.651 * | 10.876 ** | 10.354 ** |
| | (5.30) | (5.11) | (4.84) | (5.21) | (5.05) | (4.76) |
| Year 5-Year 2 | -1.030 | -1.029 | 2.119 | -0.575 | -0.492 | 1.253 |
| | (4.51) | (4.52) | (4.62) | (4.46) | (4.47) | (4.53) |

Note: The sample size -- see Table 2 -- varies very slightly across outcomes, reflecting a small number of missing values. The sample size given is the smallest across all outcomes considered.

Three sets of results are shown each time -- without covariates (column 1), with age and ethnicity (2), and with a full set of covariate regressors (3).

## Table 2: Evidence of Negative Well-being Effects of the Treatment by Year 5 of the Study (with Standard Errors in Parentheses)

Notes: To interpret this table, a number like 0.04 in the top left-hand corner indicates that in Year 2 of the study the treatment group had 0.04 extra points (with a standard error of 0.04, so not significantly different from zero at the 95% confidence level) of life satisfaction when compared to the control group.

- * Is significant at 90% confidence on a two-tailed test
- ** Is significant at 95%
- *** Is significant at 99%

| | Great Britain, excluding London | | | Great Britain | | |
|---|---|---|---|---|---|---|
| | **(1)** | **(2)** | **(3)** | **(1)** | **(2)** | **(3)** |
| *Life satisfaction* (1=very dissatisfied, 5=very satisfied): | | | | | | |
| Year 2 | 0.040 | 0.044 | 0.035 | 0.036 | 0.042 | 0.034 |
| | (0.04) | (0.04) | (0.04) | (0.04) | (0.04) | (0.04) |
| Year 5 | -0.086** | -0.080** | -0.084** | -0.071* | -0.065* | -0.071* |
| | (0.04) | (0.04) | (0.04) | (0.04) | (0.04) | (0.04) |
| Year 5-Year 2 | -0.125*** | -0.124*** | -0.119*** | -0.107*** | -0.106** | -0.106** |
| | (0.04) | (0.04) | (0.04) | (0.04) | (0.04) | (0.04) |
| *Financial situation* (1=very difficult, 5=very easy): | | | | | | |
| Year 2 | 0.030 | 0.032 | 0.029 | 0.025 | 0.029 | 0.026 |
| | (0.03) | (0.03) | (0.03) | (0.03) | (0.03) | (0.03) |
| Year 5 | -0.086** | -0.079** | -0.081** | -0.083** | -0.076** | -0.079** |
| | (0.03) | (0.03) | (0.03) | (0.03) | (0.03) | (0.03) |
| Year 5-Year 2 | -0.116*** | -0.112*** | -0.112*** | -0.109*** | -0.106*** | -0.107*** |
| | (0.04) | (0.04) | (0.04) | (0.04) | (0.04) | (0.04) |
| Other Year 5 outcomes: | | | | | | |
| *Run out of money* | -0.223*** | -0.219*** | -0.211*** | -0.213*** | -0.205*** | -0.205*** |
| (1=always, 6=never) | (0.06) | (0.06) | (0.06) | (0.06) | (0.06) | (0.06) |
| *Worry about money* | -0.070* | -0.064* | -0.068* | -0.064* | -0.056 | -0.062* |
| (1=almost always, 4=never) | (0.04) | (0.04) | (0.04) | (0.04) | (0.03) | (0.03) |
| *Trouble with debts* | -0.089** | -0.085** | -0.094** | -0.066* | -0.060* | -0.069** |
| (1=almost always, 4=never) | (0.04) | (0.04) | (0.04) | (0.04) | (0.03) | (0.03) |
| *Money left at end of week* | -0.090* | -0.087 | -0.097* | -0.078 | -0.076 | -0.091* |
| (1=never, 6=always) | (0.05) | (0.05) | (0.05) | (0.05) | (0.05) | (0.05) |
| N | 2830 | 2830 | 2830 | 3197 | 3197 | 3197 |

Note: The sample size varies very slightly across outcomes, reflecting a small number of missing values. The sample size given is the smallest across all outcomes considered.

Three sets of results are shown each time -- without covariates (column 1), with age and ethnicity (2), and with a full set of covariate regressors (3).

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## APPENDIX A

### Design of the Project and Detailed Checks

Details of the evaluation design

The experiment was conducted in 6 regions within the UK. Intake to the experiment began in October 2003 and continued until April 2005. Three groups of individuals were targeted:

- out of work single parents on welfare ("Income Support" – IS);
- single parents working part-time in low-paid jobs that qualified them for in-work financial support known as the "Working Tax Credit" (WTC – comparable to the Earned Income Tax Credit in the US)
- long-term unemployed people on welfare ("Jobseeker's Allowance" - JSA. A key difference between IS and JSA is that the former placed no requirement on recipients to look for work).

Welfare, employment and earnings outcomes for five years after randomization were taken from administrative records. For the two single parent groups, additional outcomes were available from surveys carried out approximately two years after random assignment and again approximately five years after random assignment.

Notes on the sample

The analysis in this paper used data collected from surveys and so is restricted to the two single parent groups. Furthermore, attention is restricted to mothers (who account for 96 per cent of single parents in the experiment). The analysis presented in the paper is based on single mothers responding to both year 2 and year 5 survey interviews. Interviews were attempted with 5,444 single mothers. Of these, 3,212 responded at both year 2 and year 5, an overall response rate of 59 per cent. The characteristics of the resulting sample are shown in Table A1.

The official report maintained a distinction at all times between the three target groups listed above and produced results for men and women combined. The main earnings impacts in that report were based on outcomes observed in administrative data. This was in order to allow the full sample of experimental participants to be used rather than the sub-sample of survey respondents and also to avoid issues of survey attrition. There are definitional differences between the earnings measures available from administrative sources and those collected through surveys that reduce comparability across the two sources.

Measures of life satisfaction and perceived financial security

The survey collected information on life satisfaction and a range of aspects of individuals' perceived financial circumstances. The wording of the questions asked is given below. The first two questions were asked in both the year 2 and year 5 surveys:

*Thinking about all aspects of your life at the moment, how satisfied are you with your life as a whole. Are you ...*

*(1) very satisfied,*
*(2) satisfied,*
*(3) neither satisfied nor dissatisfied*
*(4) dissatisfied, or*
*(5) very dissatisfied?*

*How difficult would you say your financial situation is at the moment.*
*Is it...*

*(1) very difficult,*
*(2) quite difficult,*
*(3) neither easy nor difficult,*
*(4) quite easy,*
*(5) or very easy?*

The other questions were asked only at the year 5 interview:

*How often would you say, do you run out of money before the end of the week or the month?*

*(1) always*
*(2) most weeks/months*
*(3) more often than not*
*(4) sometimes*
*(5) hardly ever*
*(6) or never?*
*(7) spontaneous: don't know/too hard to say/varied too much to say*

*How often would you say you have been worried about money during the last few weeks?...*

*(1) almost all the time*
*(2) quite often*
*(3) only sometimes*
*(4) never?*

*Thinking back over the past 12 months, how often would you say you have had trouble with debts that you found hard to repay?*

*(1) almost all the time*
*(2) quite often*
*(3) only sometimes*
*(4) never?*

*How often, would you say, do you have money left over at the end of the week, or if you budget by the month, at the end of the month?*

*(1) always*
*(2) most weeks/months*
*(3) more often than not*
*(4) sometimes*
*(5) hardly ever*
*(6) or never?*
*(7) spontaneous: don't know/too hard to say/varied too much to say*

Basic descriptives for the outcome variables considered

Where necessary, responses to questions were recoded so that for all variables a higher value corresponded to a more desirable outcome. For instance, in the life satisfaction question, 5 was recoded to 1, 4 was recoded to 2, 2 was recoded to 4 and 1 was recoded to 5. With this in mind, some basic descriptives of the transformed variables are provided in Table A2.

Detailed estimation results

Tables A3-A9 provide the full detail of the key estimation results in the paper. These are the results relating to the UK excluding London. For each outcome measure three sets of results are shown. These differ in the covariates included in the regressions: the first specification has no covariates; the second specification includes only age and ethnicity (as firmly exogenous variables); the third specification includes a full set of baseline information (still exogenous to treatment status since collected prior to randomization).

**Table A1.**
**The Characteristics of the Sample Who were Randomized (percentages)**

| | Great Britain | | London | | GB, excl. London | |
|---|---|---|---|---|---|---|
| | Control | Treat | Control | Treat | Control | Treat |
| District (col. %) | | | | | | |
| Scotland | 0.13 | 0.13 | | | 0.15 | 0.15 |
| North East | 0.15 | 0.15 | | | 0.17 | 0.17 |
| North West | 0.11 | 0.12 | | | 0.13 | 0.13 |
| Wales | 0.10 | 0.11 | | | 0.12 | 0.12 |
| East Midlands | 0.38 | 0.37 | | | 0.44 | 0.42 |
| London | 0.12 | 0.12 | | | | |
| Highest qualification (col. %) | | | | | | |
| A level or higher | 0.29 | 0.28 | 0.27 | 0.31 | 0.30 | 0.27 |
| O level | 0.45 | 0.47 | 0.49 | 0.50 | 0.44 | 0.46 |
| Other education qualification | 0.10 | 0.10 | 0.11 | 0.07 | 0.10 | 0.10 |
| None | 0.16 | 0.16 | 0.13 | 0.11 | 0.17 | 0.17 |
| Months worked in past 3 years (col. %) | | | | | | |
| 12 or fewer months | 0.42 | 0.41 | 0.52 | 0.58 | 0.41 | 0.39 |
| 13-24 months | 0.13 | 0.11 | 0.13 | 0.08 | 0.13 | 0.12 |
| 25-36 months | 0.45 | 0.47 | 0.35 | 0.34 | 0.46 | 0.49 |
| Last weekly earnings in year pre-RA (£) | 69.73 | 71.50 | 52.94 | 63.91 | 71.95 | 72.50 |
| | (69.08) | (70.80) | (73.11) | (84.86) | (68.25) | (68.70) |
| Months on welfare in two years pre-RA | 10.14 | 9.95 | 12.79 | 13.04 | 9.79 | 9.54 |
| | (10.30) | (10.34) | (10.67) | (10.55) | (10.20) | (10.24) |
| Quarter of RA (col. %) | | | | | | |
| Oct 03-Dec 03 | 0.02 | 0.02 | 0.04 | 0.04 | 0.02 | 0.02 |
| Jan 04-Mar 04 | 0.24 | 0.22 | 0.25 | 0.24 | 0.24 | 0.22 |
| Apr 04-Jun 04 | 0.18 | 0.18 | 0.18 | 0.19 | 0.18 | 0.18 |
| Jul 04-Sep 04 | 0.21 | 0.22 | 0.27 | 0.27 | 0.21 | 0.21 |
| Oct 04-Dec 04 | 0.26 | 0.26 | 0.21 | 0.22 | 0.27 | 0.26 |
| Jan 05-Apr 05 | 0.09 | 0.09 | 0.05 | 0.03 | 0.09 | 0.10 |
| Age | 34.77 | 34.97 | 35.78 | 34.87 | 34.63 | 34.99 |
| | (8.05) | (8.11) | (7.97) | (8.30) | (8.06) | (8.09) |
| Age of youngest child (col. %) | | | | | | |
| less than 6 | 0.43 | 0.43 | 0.40 | 0.49 | 0.44 | 0.42 |
| 6-10 | 0.30 | 0.29 | 0.30 | 0.31 | 0.30 | 0.28 |
| 11-18 | 0.27 | 0.29 | 0.30 | 0.19 | 0.27 | 0.30 |
| Receiving IS or WTC (col. %) | | | | | | |
| IS | 0.49 | 0.50 | 0.69 | 0.71 | 0.47 | 0.47 |

| | | | | | | |
|---|---|---|---|---|---|---|
| WTC | 0.51 | 0.50 | 0.31 | 0.29 | 0.53 | 0.53 |
| Ethnic minority status (col. %) | | | | | | |
| White | 0.91 | 0.90 | 0.68 | 0.66 | 0.93 | 0.93 |
| Non-white | 0.09 | 0.10 | 0.32 | 0.34 | 0.07 | 0.07 |
| N | 1,603 | 1,723 | 187 | 201 | 1,416 | 1,522 |

Standard deviations in parentheses

**Table A2.**
**Mean, standard deviation and distribution of the well-being measures**

| | | Year 2 | Year 5 |
|---|---|---|---|
| Life satisfaction | mean | 3.59 | 3.62 |
| | standard deviation | 1.04 | 1.07 |
| | Distribution: | | |
| | - very dissatisfied | 4.61% | 5.21% |
| | - dissatisfied | 12.14% | 11.02% |
| | - neither satisfied nor dissatisfied | 19.10% | 19.33% |
| | - satisfied | 47.80% | 45.09% |
| | - very satisfied | 16.36% | 19.36% |
| | Number of observations | 3,320 | 3,322 |
| | | | |
| Financial situation | mean | 2.18 | 2.19 |
| | standard deviation | 0.92 | 0.92 |
| | Distribution: | | |
| | - very difficult | 26.85% | 25.78% |
| | - quite difficult | 36.22% | 36.90% |
| | - neither easy nor difficult | 29.92% | 30.57% |
| | - quite easy | 6.54% | 5.87% |
| | - very easy | 0.48% | 0.87% |
| | Number of observations | 3,319 | 3,320 |
| | | | |
| Run out of money | mean | | 3.76 |
| | standard deviation | | 1.67 |
| | Distribution: | | |
| | - always | | 16.07% |
| | - most weeks/months | | 10.66% |
| | - more often than not | | 9.64% |
| | - sometimes | | 25.89% |
| | - hardly ever | | 20.57% |
| | - never | | 17.16% |
| | Number of observations | | 3,310 |

| | | |
|---|---|---|
| Worry about money | mean | 2.19 |
| | standard deviation | 1.00 |
| | Distribution: | |
| | - almost all the time | 32.65% |
| | - quite often | 25.46% |
| | - only sometimes | 32.56% |
| | - never | 9.33% |
| | Number of observations | 3,323 |
| | | |
| Trouble with debts | mean | 2.19 |
| | standard deviation | 1.00 |
| | Distribution: | |
| | - almost all the time | 11.36% |
| | - quite often | 14.46% |
| | - only sometimes | 33.78% |
| | - never | 40.40% |
| | Number of observations | 3,319 |
| | | |
| Money left over | mean | 3.62 |
| | standard deviation | 1.07 |
| | Distribution: | |
| | - never | 29.91% |
| | - hardly ever | 26.98% |
| | - sometimes | 24.32% |
| | - more often than not | 7.01% |
| | - most weeks/months | 5.50% |
| | - always | 6.28% |
| | Number of observations | 3,310 |

**Table A3.**
**Effect of the ERA treatment on weekly earnings (GB excluding London, years 2 and 5)**

| | Year 2 | | | Year 5 | | |
|---|---|---|---|---|---|---|
| ERA | 10.94** | 11.86*** | 8.99** | 9.90* | 10.75** | 11.22** |
| | (4.38) | (4.23) | (3.93) | (5.30) | (5.11) | (4.84) |
| Age | | 11.52*** | 8.36*** | | 18.68*** | 15.27*** |
| | | (1.70) | (1.73) | | (2.08) | (2.08) |
| Age squared (/100) | | -11.95*** | -10.85*** | | -21.52*** | -19.67*** |
| | | (2.50) | (2.45) | | (3.02) | (2.94) |
| Age missing | | 71.84*** | 9.17 | | 73.31*** | 21.00* |
| | | (9.88) | (9.28) | | (12.72) | (12.63) |
| Non-white | | 0.60 | -2.64 | | 22.15 | 10.73 |
| | | (9.52) | (8.85) | | (13.93) | (13.52) |
| Ethnicity missing | | 5.09 | -29.77 | | -3.60 | -35.80 |
| | | (29.30) | (32.12) | | (141.27) | (137.93) |
| Youngest child aged 6-10 | | | 3.37 | | | 2.80 |
| | | | (5.01) | | | (6.36) |
| Youngest child aged 11-18 | | | 21.16*** | | | 16.89** |
| | | | (5.65) | | | (7.51) |
| Highest qual: a-level | | | 48.71*** | | | 74.88*** |
| | | | (6.13) | | | (7.50) |
| Highest qual: o-level | | | 22.06*** | | | 33.64*** |
| | | | (4.91) | | | (5.92) |
| Highest qual: other | | | 31.45*** | | | 40.29*** |
| | | | (7.78) | | | (8.81) |
| Worked <=12 months in past 3 years | | | -11.84* | | | -14.37* |
| | | | (6.18) | | | (8.61) |
| Worked 13-24 months in past 3 years | | | -23.03*** | | | -21.14** |
| | | | (6.35) | | | (8.21) |
| Weekly earnings in past year | | | 0.42*** | | | 0.37*** |
| | | | (0.05) | | | (0.06) |
| Earnings in past year missing | | | 220.31*** | | | 217.89*** |
| | | | (35.86) | | | (33.88) |
| Months on welfare in past 2 years | | | -1.01*** | | | -1.39*** |

| | | |
|---|---|---|
| | (0.31) | (0.43) |
| District: Scotland | -6.38 | -8.28 |
| | (5.93) | (7.63) |
| District: North East | -4.40 | -0.20 |
| | (5.51) | (6.64) |
| District: North West | 8.60 | 22.94*** |
| | (6.78) | (8.60) |
| District: Wales | 3.62 | -5.02 |
| | (7.31) | (8.26) |
| RA in Oct 03-Dec 03 | 3.25 | 11.96 |
| | (13.28) | (18.11) |
| RA in Jan 04-Mar 04 | 1.02 | 40.25*** |
| | (10.43) | (13.12) |
| RA in Apr 04-Jun 04 | -6.01 | 45.18*** |
| | (12.98) | (16.46) |
| RA in Jul 04-Sep 04 | -5.43 | 22.35 |
| | (10.70) | (14.06) |
| RA in Oct 04-Dec 04 | 0.29 | 30.80*** |
| | (9.14) | (11.90) |
| Month: February | 3.44 | -23.30** |
| | (8.78) | (10.11) |
| Month: March | -12.50 | -14.12 |
| | (13.89) | (17.33) |
| Month: April | 5.93 | -17.87 |
| | (12.54) | (15.34) |
| Month: May | 6.22 | -27.27* |
| | (12.56) | (15.73) |
| Month: June | 2.92 | -32.29*** |
| | (10.77) | (12.52) |
| Month: July | 1.66 | -9.21 |
| | (7.53) | (9.96) |
| Month: August | 1.67 | 3.70 |
| | (9.05) | (13.28) |
| Month: September | -2.24 | -13.20 |
| | (10.71) | (13.64) |
| Month: October | -8.43 | -26.79** |
| | (9.18) | (11.12) |
| Month: November | -2.89 | -9.27 |
| | (9.97) | (12.57) |
| Month: December | -11.02 | -2.55 |
| | (9.95) | (13.48) |

| | | | | | | |
|---|---|---|---|---|---|---|
| Receiving WTC | | | -0.06 | | | -2.90 |
| | | | (6.72) | | | (8.95) |
| _cons | 128.29*** | -123.16*** | -69.90** | 151.22*** | -228.79*** | -201.72*** |
| | (3.15) | (28.18) | (31.50) | (3.73) | (34.51) | (39.08) |
| N | 2833 | 2833 | 2833 | 2842 | 2842 | 2842 |

Standard errors in parentheses

**Table A4.**

**Effect of the ERA treatment on life satisfaction (GB excluding London, years 2 and 5 – higher score indicates higher life satisfaction)**

| | | Year 2 | | | Year 5 | |
|---|---|---|---|---|---|---|
| ERA | 0.04 | 0.04 | 0.04 | -0.09** | -0.08** | -0.08** |
| | (0.04) | (0.04) | (0.04) | (0.04) | (0.04) | (0.04) |
| Age | | 0.02 | 0.02 | | 0.04** | 0.02 |
| | | (0.02) | (0.02) | | (0.02) | (0.02) |
| age squared (/100) | | -0.04 | -0.04 | | -0.07** | -0.05* |
| | | (0.02) | (0.02) | | (0.03) | (0.03) |
| Age missing | | 0.12 | -0.05 | | 0.13 | -0.07 |
| | | (0.09) | (0.10) | | (0.09) | (0.10) |
| Non-white | | -0.25*** | -0.23** | | -0.28*** | -0.29*** |
| | | (0.08) | (0.09) | | (0.09) | (0.09) |
| Ethnicity missing | | -1.26*** | -1.37*** | | -0.96*** | -1.13*** |
| | | (0.26) | (0.29) | | (0.28) | (0.33) |
| Youngest child aged 6-10 | | | -0.05 | | | -0.07 |
| | | | (0.05) | | | (0.05) |
| Youngest child aged 11-18 | | | -0.05 | | | -0.13** |
| | | | (0.06) | | | (0.06) |
| Highest qual: a-level | | | 0.17** | | | 0.21*** |
| | | | (0.07) | | | (0.07) |
| Highest qual: o-level | | | 0.14** | | | 0.10* |
| | | | (0.06) | | | (0.06) |
| Highest qual: other | | | 0.08 | | | 0.10 |
| | | | (0.08) | | | (0.08) |
| Worked <=12 months in past 3 years | | | -0.08 | | | -0.13** |
| | | | (0.06) | | | (0.07) |
| Worked 13-24 months in past 3 years | | | -0.08 | | | -0.16** |
| | | | (0.06) | | | (0.07) |
| Weekly earnings in past year | | | 0.00 | | | 0.00** |
| | | | (0.00) | | | (0.00) |
| Earnings in past year missing | | | 0.33** | | | 0.22 |
| | | | (0.14) | | | (0.20) |
| Months on welfare in past 2 years | | | 0.00 | | | 0.00 |
| | | | (0.00) | | | (0.00) |
| District: Scotland | | | -0.08 | | | -0.08 |
| | | | (0.06) | | | (0.06) |
| District: North East | | | -0.07 | | | -0.06 |
| | | | (0.06) | | | (0.06) |
| District: North West | | | -0.13* | | | -0.01 |

| | | | | | | |
|---|---|---|---|---|---|---|
| | | | (0.07) | | | (0.07) |
| District: Wales | | | -0.03 | | | -0.19*** |
| | | | (0.07) | | | (0.07) |
| RA in Oct 03-Dec 03 | | | -0.22 | | | 0.21 |
| | | | (0.16) | | | (0.16) |
| RA in Jan 04-Mar 04 | | | -0.11 | | | 0.13 |
| | | | (0.11) | | | (0.11) |
| RA in Apr 04-Jun 04 | | | -0.19 | | | -0.10 |
| | | | (0.13) | | | (0.14) |
| RA in Jul 04-Sep 04 | | | -0.20* | | | 0.11 |
| | | | (0.12) | | | (0.12) |
| RA in Oct 04-Dec 04 | | | -0.12 | | | 0.02 |
| | | | (0.10) | | | (0.10) |
| Month: February | | | -0.07 | | | -0.12 |
| | | | (0.09) | | | (0.09) |
| Month: March | | | -0.19 | | | -0.31* |
| | | | (0.14) | | | (0.18) |
| Month: April | | | 0.02 | | | 0.17 |
| | | | (0.12) | | | (0.13) |
| Month: May | | | 0.02 | | | 0.30** |
| | | | (0.13) | | | (0.14) |
| Month: June | | | 0.07 | | | 0.18* |
| | | | (0.09) | | | (0.10) |
| Month: July | | | 0.00 | | | 0.09 |
| | | | (0.08) | | | (0.08) |
| Month: August | | | 0.11 | | | 0.05 |
| | | | (0.10) | | | (0.10) |
| Month: September | | | -0.04 | | | 0.02 |
| | | | (0.11) | | | (0.12) |
| Month: October | | | -0.06 | | | 0.02 |
| | | | (0.10) | | | (0.10) |
| Month: November | | | -0.07 | | | 0.02 |
| | | | (0.10) | | | (0.11) |
| Month: December | | | -0.16 | | | 0.06 |
| | | | (0.11) | | | (0.11) |
| Receiving WTC | | | 0.04 | | | 0.16** |
| | | | (0.07) | | | (0.07) |
| _cons | 3.62*** | 3.36*** | 3.56*** | 3.71*** | 3.16*** | 3.32*** |
| | (0.03) | (0.29) | (0.34) | (0.03) | (0.31) | (0.35) |
| N | 2837 | 2837 | 2837 | 2841 | 2841 | 2841 |

Standard errors in parentheses

**Table A5.**
**Effect of the ERA treatment on difficulty of financial situation (GB excluding London, years 2 and 5 – higher score indicates better financial situation)**

| | | Year 2 | | | Year 5 | |
|---|---|---|---|---|---|---|
| ERA | 0.03 | 0.03 | 0.03 | -0.09** | -0.08** | -0.08** |
| | (0.03) | (0.03) | (0.03) | (0.03) | (0.03) | (0.03) |
| Age | | 0.04** | 0.02 | | 0.01 | -0.01 |
| | | (0.02) | (0.02) | | (0.01) | (0.02) |
| age squared (/100) | | -0.05** | -0.04* | | -0.01 | 0.00 |
| | | (0.02) | (0.02) | | (0.02) | (0.02) |
| Age missing | | 0.14* | 0.00 | | 0.25*** | 0.06 |
| | | (0.08) | (0.09) | | (0.08) | (0.09) |
| Non-white | | -0.12* | -0.08 | | -<br>0.23*** | -<br>0.20*** |
| | | (0.07) | (0.08) | | (0.07) | (0.07) |
| Ethnicity missing | | -0.56 | -0.66 | | 0.42 | 0.23 |
| | | (0.54) | (0.58) | | (0.29) | (0.29) |
| Youngest child aged 6-10 | | | 0.06 | | | 0.01 |
| | | | (0.04) | | | (0.04) |
| Youngest child aged 11-18 | | | 0.10** | | | -0.10* |
| | | | (0.05) | | | (0.05) |
| Highest qual: a-level | | | 0.14** | | | 0.21*** |
| | | | (0.06) | | | (0.06) |
| Highest qual: o-level | | | 0.14*** | | | 0.12** |
| | | | (0.05) | | | (0.05) |
| Highest qual: other | | | 0.09 | | | 0.15** |
| | | | (0.07) | | | (0.07) |
| Worked <=12 months in past 3 years | | | -0.04 | | | -0.11* |
| | | | (0.06) | | | (0.06) |
| Worked 13-24 months in past 3 years | | | -0.04 | | | -0.14** |
| | | | (0.06) | | | (0.06) |
| Weekly earnings in past year | | | 0.00 | | | 0.00 |
| | | | (0.00) | | | (0.00) |
| Earnings in past year missing | | | 0.46*** | | | 0.40** |
| | | | (0.16) | | | (0.16) |
| Months on welfare in past 2 years | | | 0.00 | | | -0.01* |
| | | | (0.00) | | | (0.00) |
| District: Scotland | | | -0.12** | | | -0.02 |
| | | | (0.05) | | | (0.06) |
| District: North East | | | 0.00 | | | -0.02 |
| | | | (0.05) | | | (0.05) |

| | | | | | | |
|---|---|---|---|---|---|---|
| District: North West | | | -0.08 | | | -0.09 |
| | | | (0.06) | | | (0.06) |
| District: Wales | | | 0.02 | | | -0.10* |
| | | | (0.06) | | | (0.06) |
| RA in Oct 03-Dec 03 | | | 0.09 | | | -0.04 |
| | | | (0.13) | | | (0.13) |
| RA in Jan 04-Mar 04 | | | -0.05 | | | -0.07 |
| | | | (0.10) | | | (0.10) |
| RA in Apr 04-Jun 04 | | | 0.01 | | | 0.00 |
| | | | (0.12) | | | (0.12) |
| RA in Jul 04-Sep 04 | | | 0.03 | | | -0.05 |
| | | | (0.10) | | | (0.11) |
| RA in Oct 04-Dec 04 | | | -0.06 | | | -0.14 |
| | | | (0.09) | | | (0.09) |
| Month: February | | | -0.10 | | | -0.13 |
| | | | (0.08) | | | (0.08) |
| Month: March | | | 0.20 | | | 0.04 |
| | | | (0.14) | | | (0.14) |
| Month: April | | | 0.01 | | | -0.11 |
| | | | (0.12) | | | (0.11) |
| Month: May | | | -0.06 | | | -0.05 |
| | | | (0.11) | | | (0.11) |
| Month: June | | | 0.00 | | | 0.06 |
| | | | (0.09) | | | (0.09) |
| Month: July | | | -0.02 | | | 0.06 |
| | | | (0.07) | | | (0.07) |
| Month: August | | | -0.15 | | | 0.02 |
| | | | (0.09) | | | (0.09) |
| Month: September | | | -0.11 | | | -0.01 |
| | | | (0.10) | | | (0.10) |
| Month: October | | | 0.00 | | | -0.01 |
| | | | (0.09) | | | (0.08) |
| Month: November | | | -0.03 | | | 0.04 |
| | | | (0.09) | | | (0.09) |
| Month: December | | | -0.12 | | | -0.13 |
| | | | (0.10) | | | (0.10) |
| Receiving WTC | | | 0.16*** | | | 0.00 |
| | | | (0.06) | | | (0.06) |
| _cons | 2.19*** | 1.52*** | 1.68*** | 2.26*** | 2.10*** | 2.48*** |
| | (0.02) | (0.28) | (0.32) | (0.02) | (0.26) | (0.29) |
| N | 2837 | 2837 | 2837 | 2837 | 2838 | 2838 |

Standard errors in parentheses

**Table A6.**

**Effect of the ERA treatment on how often money runs out (GB excluding London, year 5 – higher score indicates run out of money less often)**

| | | | |
|---|---|---|---|
| ERA | -0.22*** | -0.22*** | -0.21*** |
| | (0.06) | (0.06) | (0.06) |
| Age | | 0.11*** | 0.08*** |
| | | (0.03) | (0.03) |
| age squared (/100) | | -0.13*** | -0.10*** |
| | | (0.04) | (0.04) |
| Age missing | | 0.32** | 0.00 |
| | | (0.15) | (0.16) |
| Non-white | | -0.41*** | -0.27** |
| | | (0.13) | (0.14) |
| Ethnicity missing | | 0.35 | -0.04 |
| | | (0.26) | (0.25) |
| Youngest child aged 6-10 | | | -0.03 |
| | | | (0.08) |
| Youngest child aged 11-18 | | | -0.25*** |
| | | | (0.09) |
| Highest qual: a-level | | | 0.57*** |
| | | | (0.11) |
| Highest qual: o-level | | | 0.46*** |
| | | | (0.09) |
| Highest qual: other | | | 0.40*** |
| | | | (0.13) |
| Worked <=12 months in past 3 years | | | -0.35*** |
| | | | (0.11) |
| Worked 13-24 months in past 3 years | | | -0.31*** |
| | | | (0.10) |
| Weekly earnings in past year | | | 0.00 |
| | | | (0.00) |
| Earnings in past year missing | | | 0.50* |
| | | | (0.28) |
| Months on welfare in past 2 years | | | 0.00 |
| | | | (0.01) |
| District: Scotland | | | 0.01 |
| | | | (0.10) |
| District: North East | | | -0.06 |
| | | | (0.10) |
| District: North West | | | -0.34*** |

| | | | |
|---|---|---|---|
| | | | (0.11) |
| District: Wales | | | -0.14 |
| | | | (0.11) |
| RA in Oct 03-Dec 03 | | | -0.29 |
| | | | (0.24) |
| RA in Jan 04-Mar 04 | | | 0.08 |
| | | | (0.18) |
| RA in Apr 04-Jun 04 | | | -0.15 |
| | | | (0.21) |
| RA in Jul 04-Sep 04 | | | 0.04 |
| | | | (0.19) |
| RA in Oct 04-Dec 04 | | | -0.02 |
| | | | (0.16) |
| Month: February | | | -0.16 |
| | | | (0.14) |
| Month: March | | | 0.29 |
| | | | (0.22) |
| Month: April | | | 0.24 |
| | | | (0.20) |
| Month: May | | | 0.34* |
| | | | (0.20) |
| Month: June | | | 0.05 |
| | | | (0.16) |
| Month: July | | | 0.14 |
| | | | (0.12) |
| Month: August | | | 0.13 |
| | | | (0.15) |
| Month: September | | | -0.15 |
| | | | (0.17) |
| Month: October | | | -0.05 |
| | | | (0.15) |
| Month: November | | | -0.04 |
| | | | (0.16) |
| Month: December | | | -0.08 |
| | | | (0.18) |
| Receiving WTC | | | 0.01 |
| | | | (0.11) |
| _cons | 3.92*** | 1.84*** | 2.18*** |
| | (0.04) | (0.47) | (0.52) |
| N | 2831 | 2831 | 2831 |

Standard errors in parentheses

**Table A7.**

**Effect of the ERA treatment on how often individuals have money worries (GB excluding London, year 5 – higher score indicates worry about money less often)**

| | | | |
|---|---|---|---|
| ERA | -0.07* | -0.06* | -0.07* |
| | (0.04) | (0.04) | (0.04) |
| Age | | 0.04** | 0.02 |
| | | (0.02) | (0.02) |
| age squared (/100) | | -0.05** | -0.03 |
| | | (0.02) | (0.02) |
| Age missing | | 0.33*** | 0.12 |
| | | (0.08) | (0.09) |
| Non-white | | -0.07 | -0.04 |
| | | (0.07) | (0.08) |
| Ethnicity missing | | -0.61 | -0.85 |
| | | (0.54) | (0.54) |
| Youngest child aged 6-10 | | | -0.02 |
| | | | (0.05) |
| Youngest child aged 11-18 | | | -0.13** |
| | | | (0.05) |
| Highest qual: a-level | | | 0.21*** |
| | | | (0.06) |
| Highest qual: o-level | | | 0.12** |
| | | | (0.05) |
| Highest qual: other | | | 0.22*** |
| | | | (0.07) |
| Worked <=12 months in past 3 years | | | -0.17*** |
| | | | (0.06) |
| Worked 13-24 months in past 3 years | | | -0.15** |
| | | | (0.06) |
| Weekly earnings in past year | | | 0.00*** |
| | | | (0.00) |
| Earnings in past year missing | | | 0.46*** |
| | | | (0.16) |
| Months on welfare in past 2 years | | | 0.00 |
| | | | (0.00) |
| District: Scotland | | | -0.08 |
| | | | (0.06) |
| District: North East | | | -0.04 |
| | | | (0.06) |
| District: North West | | | -0.12* |

| | | | |
|---|---|---|---|
| | | | (0.06) |
| District: Wales | | | -0.17*** |
| | | | (0.06) |
| RA in Oct 03-Dec 03 | | | 0.09 |
| | | | (0.15) |
| RA in Jan 04-Mar 04 | | | 0.06 |
| | | | (0.11) |
| RA in Apr 04-Jun 04 | | | 0.02 |
| | | | (0.12) |
| RA in Jul 04-Sep 04 | | | 0.04 |
| | | | (0.11) |
| RA in Oct 04-Dec 04 | | | -0.09 |
| | | | (0.09) |
| Month: February | | | 0.00 |
| | | | (0.08) |
| Month: March | | | 0.18 |
| | | | (0.15) |
| Month: April | | | 0.18 |
| | | | (0.12) |
| Month: May | | | 0.18 |
| | | | (0.12) |
| Month: June | | | 0.21** |
| | | | (0.09) |
| Month: July | | | 0.17** |
| | | | (0.08) |
| Month: August | | | 0.10 |
| | | | (0.10) |
| Month: September | | | 0.11 |
| | | | (0.10) |
| Month: October | | | 0.13 |
| | | | (0.09) |
| Month: November | | | 0.13 |
| | | | (0.09) |
| Month: December | | | 0.02 |
| | | | (0.11) |
| Receiving WTC | | | 0.02 |
| | | | (0.06) |
| _cons | 2.24*** | 1.43*** | 1.67*** |
| | (0.03) | (0.27) | (0.31) |
| N | 2841 | 2841 | 2841 |

Standard errors in parentheses

**Table A8.**

**Effect of the ERA treatment on how often individuals have trouble with debts (GB excluding London, year 5 – higher score indicates trouble with debts less common)**

| | | | |
|---|---|---|---|
| ERA | -0.09** | -0.09** | -0.09** |
| | (0.04) | (0.04) | (0.04) |
| Age | | 0.06*** | 0.04** |
| | | (0.02) | (0.02) |
| age squared (/100) | | -0.06*** | -0.05** |
| | | (0.02) | (0.02) |
| Age missing | | 0.31*** | 0.01 |
| | | (0.08) | (0.08) |
| Non-white | | -0.36*** | -0.31*** |
| | | (0.08) | (0.08) |
| Ethnicity missing | | 0.11 | -0.13 |
| | | (0.30) | (0.33) |
| Youngest child aged 6-10 | | | -0.03 |
| | | | (0.05) |
| Youngest child aged 11-18 | | | -0.06 |
| | | | (0.05) |
| Highest qual: a-level | | | 0.26*** |
| | | | (0.06) |
| Highest qual: o-level | | | 0.19*** |
| | | | (0.05) |
| Highest qual: other | | | 0.13* |
| | | | (0.08) |
| Worked <=12 months in past 3 years | | | -0.07 |
| | | | (0.06) |
| Worked 13-24 months in past 3 years | | | -0.18*** |
| | | | (0.06) |
| Weekly earnings in past year | | | 0.00* |
| | | | (0.00) |
| Earnings in past year missing | | | 0.23 |
| | | | (0.16) |
| Months on welfare in past 2 years | | | -0.01** |
| | | | (0.00) |
| District: Scotland | | | -0.20*** |
| | | | (0.06) |
| District: North East | | | -0.14** |
| | | | (0.06) |
| District: North West | | | -0.25*** |

| | | | |
|---|---|---|---|
| | | | (0.07) |
| District: Wales | | | -0.17*** |
| | | | (0.06) |
| RA in Oct 03-Dec 03 | | | 0.05 |
| | | | (0.14) |
| RA in Jan 04-Mar 04 | | | -0.01 |
| | | | (0.10) |
| RA in Apr 04-Jun 04 | | | -0.08 |
| | | | (0.12) |
| RA in Jul 04-Sep 04 | | | -0.08 |
| | | | (0.11) |
| RA in Oct 04-Dec 04 | | | -0.04 |
| | | | (0.09) |
| Month: February | | | -0.14* |
| | | | (0.08) |
| Month: March | | | 0.08 |
| | | | (0.14) |
| Month: April | | | 0.10 |
| | | | (0.12) |
| Month: May | | | 0.07 |
| | | | (0.12) |
| Month: June | | | 0.10 |
| | | | (0.09) |
| Month: July | | | -0.01 |
| | | | (0.08) |
| Month: August | | | 0.04 |
| | | | (0.10) |
| Month: September | | | 0.01 |
| | | | (0.10) |
| Month: October | | | 0.01 |
| | | | (0.09) |
| Month: November | | | -0.03 |
| | | | (0.09) |
| Month: December | | | -0.03 |
| | | | (0.11) |
| Receiving WTC | | | 0.03 |
| | | | (0.06) |
| _cons | 3.11*** | 1.83*** | 2.31*** |
| | (0.03) | (0.28) | (0.32) |
| N | 2839 | 2839 | 2839 |

Standard errors in parentheses

**Table A9.**

**Effect of the ERA treatment on how often individuals have money left over at the end of the week/month (GB excluding London, year 5 – higher score indicates have money left over more often)**

| | | | |
|---|---|---|---|
| ERA | -0.09* | -0.09 | -0.10* |
| | (0.05) | (0.05) | (0.05) |
| Age | | 0.02 | -0.01 |
| | | (0.02) | (0.02) |
| age squared (/100) | | -0.02 | 0.01 |
| | | (0.03) | (0.03) |
| Age missing | | 0.08 | -0.22 |
| | | (0.14) | (0.15) |
| Non-white | | -0.13 | -0.01 |
| | | (0.10) | (0.11) |
| Ethnicity missing | | 1.13 | 0.89 |
| | | (1.10) | (1.10) |
| Youngest child aged 6-10 | | | 0.10 |
| | | | (0.07) |
| Youngest child aged 11-18 | | | -0.01 |
| | | | (0.08) |
| Highest qual: a-level | | | 0.18** |
| | | | (0.09) |
| Highest qual: o-level | | | 0.18** |
| | | | (0.07) |
| Highest qual: other | | | 0.20* |
| | | | (0.11) |
| Worked <=12 months in past 3 years | | | -0.17* |
| | | | (0.09) |
| Worked 13-24 months in past 3 years | | | -0.25*** |
| | | | (0.09) |
| Weekly earnings in past year | | | 0.00*** |
| | | | (0.00) |
| Earnings in past year missing | | | 0.40 |
| | | | (0.27) |
| Months on welfare in past 2 years | | | 0.00 |
| | | | (0.00) |
| District: Scotland | | | 0.09 |
| | | | (0.09) |
| District: North East | | | -0.07 |
| | | | (0.08) |

| | | | |
|---|---|---|---|
| District: North West | | | -0.20** |
| | | | (0.09) |
| District: Wales | | | -0.03 |
| | | | (0.10) |
| RA in Oct 03-Dec 03 | | | -0.32 |
| | | | (0.21) |
| RA in Jan 04-Mar 04 | | | -0.10 |
| | | | (0.16) |
| RA in Apr 04-Jun 04 | | | -0.28 |
| | | | (0.20) |
| RA in Jul 04-Sep 04 | | | -0.12 |
| | | | (0.17) |
| RA in Oct 04-Dec 04 | | | -0.21 |
| | | | (0.15) |
| Month: February | | | -0.15 |
| | | | (0.12) |
| Month: March | | | -0.04 |
| | | | (0.20) |
| Month: April | | | 0.08 |
| | | | (0.18) |
| Month: May | | | 0.15 |
| | | | (0.18) |
| Month: June | | | 0.06 |
| | | | (0.15) |
| Month: July | | | 0.06 |
| | | | (0.11) |
| Month: August | | | 0.08 |
| | | | (0.15) |
| Month: September | | | -0.23 |
| | | | (0.15) |
| Month: October | | | -0.15 |
| | | | (0.13) |
| Month: November | | | -0.11 |
| | | | (0.14) |
| Month: December | | | -0.06 |
| | | | (0.16) |
| Receiving WTC | | | -0.12 |
| | | | (0.09) |
| _cons | 2.57*** | 2.31*** | 2.95*** |
| | (0.04) | (0.40) | (0.46) |
| N | 2830 | 2830 | 2830 |

Standard errors in parentheses

## APPENDIX B

### The Issue of Who Becomes Dissatisfied

A natural question to ask is: which kinds of individuals become particularly dissatisfied?

To explore this, we construct a change-in-satisfaction variable called Δ*satis* which is the difference between the satisfaction level at year 2 and the satisfaction level at year 5.

Satisfaction in both years is ranked 1-5 so Δ*satis* can take values from -4 to +4, with a positive number indicating increased satisfaction. The distribution in the treatment group (for those interviewed in both year 2 and year 5) is:

| Δ*satis* | Freq. | Percent | Cum. |
|---:|---:|---:|---:|
| -4 | 5 | 0.30 | 0.30 |
| -3 | 42 | 2.55 | 2.85 |
| -2 | 109 | 6.61 | 9.45 |
| -1 | 329 | 19.94 | 29.39 |
| 0 | 682 | 41.33 | 70.73 |
| 1 | 336 | 20.36 | 91.09 |
| 2 | 118 | 7.15 | 98.24 |
| 3 | 26 | 1.58 | 99.82 |
| 4 | 3 | 0.18 | 100.00 |
| Total | 1,650 | 100.00 | |

To address the question of who in the treatment group is becoming less satisfied, we regress Δ*satis* on a number of variables (for treatment group members only). This is shown in the table below, Table B1. The first column shows that satisfaction improved significantly more among those who were already working (albeit part-time) at the time of randomization. The other background characteristics considered don't seem to matter. The second column of results introduces a measure of financial difficulty at the time of the year 2 survey. Those whose financial situation was relatively easy at year 2 are more likely to experience a reduction in life satisfaction. Lastly, column 3 introduces life satisfaction at year 2 as an additional regressor. Those with higher satisfaction at that time are more likely to experience reduced satisfaction come year 5. We show this column separately since this could just be regression to the mean. However, it is interesting to note that including this variable makes one of the children variables significant – those with older children are more likely to experience a reduction in life satisfaction. This is potentially consistent with a small amount of evidence that adolescent children themselves may suffer when the parent engages in a work-program incentive (Zaslow et al. 2001).

**Table B1.**

**Changes in life satisfaction among individuals in the treatment group**

In this table, all the observations are of people in the treatment group. The dependent variable is the change in life satisfaction, so a negative coefficient indicates someone who did worse than others within the treated sub-sample.

*Equations for Δsatis = the change in life satisfaction between Year 2 and Year 5*

| | Δ*satis* | Δ*satis* |
|---|---|---|
| Age | -0.001 | 0.024 |
| | (0.03) | (1.07) |
| Age squared | -0.004 | -0.043 |
| | (0.12) | (1.40) |
| Ethnic minority | 0.121 | -0.083 |
| | (1.16) | (0.94) |
| Youngest child age 6-10 | -0.031 | -0.069 |
| | (0.41) | (1.07) |
| Youngest child age 11-18 | -0.128 | -0.179 |
| | (1.47) | (2.44)** |
| Qualification a-level or higher | 0.042 | 0.079 |
| | (0.62) | (1.42) |
| London | 0.143 | 0.037 |
| | (1.48) | (0.45) |
| PT work (on WTC) when randomized | 0.192 | 0.256 |
| | (3.06)*** | (4.82)*** |
| Level of financial difficulties, year 2 | | 0.128 |
| | | (4.42)** |
| Level of life satisfaction, year 2 | | -0.665 |
| | | (25.63)** |
| Constant | -0.025 | 1.761 |
| | (0.06) | (4.59)*** |
| $R^2$ | 0.01 | 0.30 |
| *N* | 1,650 | 1,647 |

t-statistics in parentheses; ** $p$<0.05; *** $p$<0.01

In the second column, the variable for the level of life satisfaction (year 2) enters negatively with a substantial coefficient. This is likely to be because of mean reversion. We leave in this column for completeness.

## APPENDIX C

### Exploring the Effect of Non-Response on the Estimated Impacts

In this part of the Appendix we consider the issue of survey non-response and whether it might compromise our main findings.

We begin by briefly describing the characteristics of the sample and the surveys. We then exploit the existence of linked administrative data to see how earnings impacts for survey respondents differ from those for respondents and nonrespondents combined. Lastly, we present the results from an estimation approach that aims to control for nonresponse. These results provide some reassurance that our estimated life satisfaction impacts are not affected by attrition.

*The sampling approach and survey response*
Individuals were recruited to the experiment between 27 October 2003 and 1 April 2005. For some of these, survey interviews were attempted 2 and then 5 years later. We call this subgroup the **fielded** sample (N=5,441). It is a random sample of individuals entering the experiment between certain dates:

- for the NDLP group, these dates were 1 December 2003 – 30 November 2004
- for the WTC group, these dates were 1 December 2003 – 31 January 2005.

Surveys were carried out (roughly) 2 and 5 years after randomization. Our results are based on women responding to both of these surveys. We refer to this as the **respondent** sample (N=3,212). The respondent sample is 59% of the fielded sample. Put another way, there was a 59% response rate.

*Comparing impact estimates for the fielded sample and the respondent sample*
Using administrative records, we can observe earnings outcomes for all individuals in the fielded sample, regardless of survey response. This allows us to compare earnings impacts based on the **fielded** sample with those based on the **respondent** sample. We consider earnings in both the 2005/6 and 2008/9 financial years.[2] Panel a) of Table C1 shows that the fielded sample gives earnings impacts that are statistically significant in 2005/6 but not in 2008/9. Panel b) shows that the respondent sample gives earnings impacts that are statistically significant in both years.

*Controlling for nonresponse*

Assume the outcome of interest, y*, is observed only for a selected group (the respondents). Write the selection equation and outcome equations respectively as

$$s_i^* = a + z_i\alpha + \epsilon_i$$
$$y_i^* = b + x_{i\beta} + v_i$$

[2] Administrative earnings data are only available on a financial year basis. Values above £52,000 are set to £52,000. Our main results use earnings data collected through survey. This has the advantage of corresponding to weekly earnings two and five years post randomization.

We observe whether someone responds ($s_i = 1$) and, for those who do, their value of $y_i^*$:

$s_i = 1$ if $s_i^* > 0, 0$ otherwise
$y_i = y_i^*$ if $s_i^* = 1$, unobserved otherwise.

Our concern is that there might be unobserved influences on response that also influence the outcome of interest, $y_i^*$. We can re-write our equations of interest to include additional terms $e_i$ and $u_i$ that represent these unobserved effects.

$$s_i^* = a + z_i\alpha + e_i + \epsilon_i$$
$$y_i^* = b + x_{i\beta} + u_i + v_i$$

where $\epsilon_i \sim N(0,1)$, $v_i \sim N(0,\sigma)$ and $corr(\epsilon_i, v_i) = 0$. Clearly, since $e_i$ and $u_i$ are individual-specific, they cannot be separately identified from the idiosyncratic error terms, $\epsilon_i$ and $v_i$. To proceed, we assume that each individual belongs to one of a finite number of groups, defined according to their combination of $e_i$ and $v_i$. This is in the spirit of Heckman and Singer (1984). We again re-write our equations of interest to reflect this:

$$s_i^* = a + z_i\alpha + e_{g_i} + \epsilon_i$$
$$y_i^* = b + x_{i\beta} + u_{g_i} + v_i$$

where $g_i \in \{1,2,..M\}$ and *M* represents the number of groups in the population. Both $e_{g_i}$ and $u_{g_i}$ are unobserved but, across the sample as a whole, each group $g$ exists with proportions given by the probability $p^g$. For a given *M*, the two equations can be estimated jointly. Writing individual *i*'s contribution to the likelihood conditional on being in group $g$ as $L_i^g$, the unconditional likelihood across the full sample is:

$$L = \prod_{i=1}^{N} \sum_{g=1}^{M} p^g L_i^g.$$

This is estimated for an arbitrary choice of *M*. To arrive at a preferred specification, we begin by estimating with $M = 1$ (no unobserved heterogeneity) and then repeat the estimation, incrementing *M* until either there is no real improvement in the likelihood or it fails to converge. We use a Vuong test of non-nested hypotheses to inform our preferred choice of *M*.
The results are shown in Table C1. The case of *M=1* corresponds to the results for the respondent sample that take no account of selection (panel b). For both 2005/6 and 2008/9 earnings, comparisons of the *M=2* results with the *M=1* results have large negative Vuong statistics. The hypothesis that the *M=2* and *M=1* specifications are equally valid is emphatically rejected (p-value of 0.000). The sign of the Vuong statistics implies that *M=2* is to be preferred over *M=1*. Hence, test results lead us to strongly prefer a specification that allows for unobserved influences on response that are correlated with unobserved influences on earnings. We note also that as *M* increases, the estimated earnings impacts for the respondent sample move closer to the impacts estimated on the fielded sample.

By contrast, when considering life satisfaction as the dependent variable, the Vuong statistics suggest the *M=1* and *M=2* specifications are equally valid (p-values of 0.19 and 0.34 in years 2 and 5, respectively). In conclusion, this analysis suggests that sample selection may be relevant when considering earnings but not when considering satisfaction.

**Table C1. Estimated impacts for the fielded and respondent samples**

| | Earnings 2005/06 | Earnings 2008/09 | Satisfaction, 2 years post-RA | Satisfaction, 5 years post-RA |
|---|---|---|---|---|
| *a) Fielded sample* | | | | |
| Impact | 343.29** | 126.22 | | |
| | (146.75) | (196.64) | | |
| *b) Respondent sample, not controlling for selection* | | | | |
| *M=1* | | | | |
| Impact | 606.12*** | 600.41** | 0.03 | -0.07* |
| | (203.34) | (266.06) | (0.04) | (0.04) |
| *b) Respondent sample, controlling for selection* | | | | |
| *M=2* | | | | |
| Impact | 568.80*** | 429.56* | 0.02 | -0.04 |
| | (174.76) | (237.40) | (0.03) | (0.03) |
| Comparison of *M=2* and *M=1:* | | | | |
| Vuong test statistic | -26.77 | -5.14 | -1.32 | -0.96 |
| p-value | 0.00 | 0.00 | 0.19 | 0.34 |
| *M=3* | | | | |
| Impact | 448.10*** | 358.62 | 0.07 | 0.00 |
| | (157.80) | (283.56) | (0.05) | (0.03) |
| Comparison of *M=3* and *M=2:* | | | | |
| Vuong test statistic | -0.37 | -0.52 | -0.01 | -0.04 |
| p-value | 0.71 | 0.60 | 0.99 | 0.97 |

* $p$<0.1; ** $p$<0.05; *** $p$<0.01. Standard errors in parentheses. Models include a full set of covariate regressors.