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Carrots, No Stick, No Driver: The Employment Impact of Job Search Assistance in a Regime with Minimal Monitoring and Sanctions

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Abstract

This paper uses a high quality longitudinal dataset to assess the impact of an active labour market intervention consisting of referral for interview plus Job Search Assistance (JSA) with the public employment service in Ireland during a period when both job search monitoring and sanctions were virtually non-existent. We find that, relative to a control group with no intervention, unemployed individuals that received the interview letter and participated in JSA were 15 per cent less likely to have exited to employment prior to 12 months. The results hold when tested against the influences of both sample selection and unobserved heterogeneity bias. The negative treatment impact is attributed to individuals lowering their job search intensity on learning, through the JSA activation interview, of the lax nature of the activation process. The research, which is unusual in the international literature in allowing the assessment of the impact of job search assistance in the virtual absence of monitoring and sanctions, highlights the need for effective monitoring and sanctions as integral components of labour market activation programmes.

Key words: Unemployment, Labour Market Policy, Job Search

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I Introduction

There now exists a vast literature on the evaluation of active labour market programmes (ALMPs) (LaLonde, 1986; LaLonde, 1995; Dolton and O'Neill, 1996; Heckman, Ichimura and Todd, 1998; Heckman, LaLonde and Smith, 1999; Conniffe, Gash and O'Connell, 2000; Martin, 2000; Gerfin and Lechner, 2002; Black, Smith, Berger and Noel, 2003; Blundell, Costa Dias, Meghir and van Reenen, 2004; Kirby and Riley, 2004; Sianesi, 2004 and 2008; Van den Berg, van der Klaauw and van Ours, 2004; Heckman and Vytlacil, 2005; Abbring, van den Berg and van Ours, 2005; Arellano, 2010; Carcillo and Grubb, 2006; Geerdsen, 2006; Røed and Raaum, 2006; Cockx and Dejemeppe, 2007; Geerdsen and Holm, 2007; Eichhorst and Konle-Seidl, 2008; Jespersen, Munch and Skipper, 2008; Lalive, van Ours and Zweimüller, 2008; and Card, Kluve and Weber, 2010). Much of this literature uses administrative data to assess the impact of compulsory Job Search Assistance (JSA) delivered simultaneously in conjunction with monitoring and sanctions. In such circumstances, it is difficult to isolate the impact of JSA from these other activation tools. However, in the Irish case, where JSA has been employed as the core activation tool, but with minimal monitoring and no sanctions, allows us a unique opportunity to do exactly this. Specifically, using a high quality longitudinal dataset that combines administrative and survey based data, this paper evaluates the impact of JSA within a cohort of benefit recipients that registered an unemployment-related social welfare claim during a 13 week period between September and December 2006. The cohort was subsequently tracked through the social welfare administrative system for a period of 21 months. We found that, relative to a control group of non-activated benefit recipients, individuals in receipt of JSA only, with minimal monitoring and no sanctions, had lower probabilities of a successful labour market entry. We attribute this observed negative JSA effect to recipients reducing their job search intensity as a consequence of learning of the minimal monitoring and absence of sanctions in the activation process, relative to the control group, who had little or no knowledge of the nature of the activation process.

II Previous Literature

JSA includes a variety of measures that have the aim of increasing the effectiveness of job search. JSA typically consists of measures such as counselling, job search courses, vocational guidance, establishment of individual action plans, direct referrals to vacant jobs. JSA is frequently combined with monitoring of job search activities and the imposition of sanctions when jobseekers do not comply with the job search requirements for receipt of

unemployment benefits. JSA tends to be the least costly Active Labour Market Programme (ALMP) (Martin, 2000; and Kluve, 2006). Furthermore, compared to other ALMPs, such as training, public sector job creation programmes and wage subsidies, JSA interventions tend to perform well in evaluation studies. However, the evidence suggests that, in general, for JSA to be effective it needs to be combined with both sanctions and the systematic monitoring of benefit recipients' job search activities.

A number of studies suggest that JSA measures (e.g. interviews/counselling, job placement services, etc.,), when combined with monitoring of job search behaviour and the threat and/or imposition of sanctions for non-compliance, can be effective in increasing the transition from unemployment to work. Crépon, Dejemeppe and Gurgand (2005) found in their analysis of a rich administrative database in France, that JSA measures on their own can have positive employment effects. Other researchers, such as Cockx and Dejemeppe (2007) using Belgian data, have shown that the threat of monitoring job search activities in isolation from JSA measures (e.g. counselling) can increase unemployed workers probability of employment,[1] and increasingly so as benefit claimants approach the time at which monitoring takes place.[2] Positive results have also been found for the imposition of sanctions for non-compliance with job search requirements: an investigation of this measure by van den Berg *et al*., (2004) in the Netherlands found that sanctions substantially increased the individual transition rate from welfare to work (see also Abbring *et al*., 2005). Lalive, van Ours and Zweimüller (2005) found that both sanction warning and enforcement had a positive effect on the exit rate from unemployment in Switzerland.[3] Black, Smith, Berger and Noel (2003) assessed the impact of mandatory training and employment services delivered to unemployment insurance claimants in Kentucky under the Worker Profiling and Reemployment Services (WPRS) initiative. They found that random assignment to the treatment group, which was accomplished by taking advantage of capacity constraints in delivery of the programme, resulted in a reduction in the duration of benefit receipt and increased earnings of the treatment group relative to the control group. They attribute much of the impact to "threat effects" arguing that the reduction in the duration of benefit receipt

[1] The positive threat result derived by Cockx and Dejemeppe (2007) specifically relates to highly educated workers. Thus, the threat of increased monitoring may not be an effective tool for increasing transitions to employment of more disadvantaged workers. Breunig *et al*. (2003) found that a policy of increased monitoring and counselling in Australia was not an effective tool in assisting long-term unemployed workers to find jobs.

[2] There is some concern that the threat of increased monitoring may result in workers accepting lower quality jobs; however, Cockx and Dejemeppe (2007) argue that this outcome can be countered by appropriate scheduling of JSA.

[3] Lalive *et al*. (2005) concluded that their results were evidence of a strong ex-ante effect of a strict sanction policy.

was largely due to early exits from unemployment insurance, which tended to coincide with receipt of the letter informing claimants of their mandatory obligations under WPRS.A number of other evaluations have found that JSA measures coupled with increased monitoring of job search behaviour and sanctions have been successful in getting unemployed workers back to work.[4] Examples of this include Müller and Steiner (2008) for Germany, Svarer (2007) for Denmark, Blundell *et al***.** (2004), van Reenen (2003) and Dolton and O'Neill (1996) for the UK[5] and Sianesi (2008) for Sweden.[6] However, given that the majority of the evaluated programmes combined JSA measures with monitoring and sanction mechanisms, apart from Crépon *et al.*, (2005), it is difficult to disentangle the relative influences of each component on the transition from unemployment to work.

While there is some debate in the literature over the optimal combination of assistance, monitoring and the threat and/or enforcement of sanctions that is required, it would appear that, by and large, a combination of all components are needed to produce benefits for both unemployed workers and society at large (Martin, 2000; OECD, 2005 and 2006; and Kluve, 2006).

III Labour Market Activation in Ireland: The National Employment Action Plan

A limited activation programme targeting youth unemployed was introduced in Ireland in 1996. However, the use of activation measures began in earnest in September 1998 when the 'Preventative Strategy' was introduced under the National Employment Action Plan (NEAP).[7] Under the NEAP process, targeted groups of unemployment benefit recipients - those on either Jobseeker's Allowance (JA) or Jobseeker's Benefit (JB)[8] - are to be intervened

[4] Examples of US studies that have derived this result include Katz and Meyer (1990), Meyer (1990) and Meyer (1995).

[5] The Blundell *et al*. (2004) and van Reenen (2003) results are based on an evaluation of the 'New Deal for Young People' programme and relate to males, while the Dolton and O'Neill (1996) results come from an assessment of the 'Restart' programme.

[6] Sianesi (2008) evaluated six Swedish ALMPs and found that each had a negative short-term impact on their participants' employment probability relative to unemployed individuals that had JSA type assistance. Apart from wage subsidies, the same results on the other ALMPs held in the long-run as well.

[7] The NEAP was developed by the Irish government in response to the European Employment Strategy (EES). This strategy required each member state to develop a National Action Plan (NAP) setting out the actions that the country would undertake to implement the guidelines contained in the EES (Grubb, Singh and Tergeist, 2009). The Irish government developed its 'Preventative Strategy' (i.e. activation strategy) to meet the specific EES guideline of improving employability via a more systematic engagement of the employment services with the unemployed.

[8] JA and JB are Ireland's two unemployment benefits. JA is a means-tested payment and JB is based on social insurance contributions.

with after a period of 13 weeks on the Live Register.[9] After this point, jobseekers are referred by the benefit agency, the Department for Social Protection (DSP), to the then national training and employment authority, FÁS,[10] for an activation interview: Ireland is one of a small number of OECD countries where the placement function of the Public Employment Service (PES) is separate from the benefit function (Grubb, Singh and Tergeist, 2009). The activation interview aims to initiate a process whereby FÁS assists the unemployed individuals to reintegrate into the labour market, via intensive engagement, guidance and counselling, establishment of actions plans, the provision of employment and/or training programmes, work placement and/or job offers. Attendance at the initial FÁS interview is the benefit recipient's only *quasi-compulsory* contact with employment services under the NEAP (Grubb *et al*., 2009). Any subsequent activation measures are purely voluntary in nature, implying that refusal to participate beyond the interview stage or to actively seek employment will generally not be met with sanctions. Grubb *et al*., (2009, 85) argue that sanction rates in Ireland "are either the lowest or close to the lowest in international comparative terms in three areas: sanctions for voluntary job leaving, refusal of work and refusal of an ALMP place." Grubb *et al*., (2009) also note that Ireland's sanction rates in respect of insufficient job search "are below typical levels (in the latter 1990s) in the countries that require frequent reporting". The authors point out that such checks of job search in Ireland "although not frequent, follow a systematic schedule: DSP guidelines call for checks at seven months of unemployment, 12 or 15 months, depending on benefit type, and annually thereafter." However, it is not at all clear from our analysis of the administrative data used in this study that even such limited guidelines have been followed.[11] Thus, the activation framework in place in Ireland during the period that the data used in this paper were captured, which is characterised by an absence of sanctions and very infrequent monitoring, allows us to effectively isolate the specific impacts of JSA on an unemployment benefit recipient's labour market integration.[12]

[9] The Live Register is Ireland's official unemployment benefit recipient record.

[10] FÁS is currently being disbanded. In January 2012, the organisation's employment services and employment programmes were transferred to the Department of Social Protection, and its training function is to be taken over by the Department of Education and Skills.

[11] Only 30 per cent of a random sample of activated clients with an unbroken unemployment duration of at least 12 months were found to have received a second interview.

[12] The few previous evaluation studies that have been undertaken on the NEAP, specifically by O'Connell (2002) and Indecon (2005) concluded that, by and large, the NEAP has been an effective labour market policy tool. However, O'Connell (2002) argues that this positive finding in relation to the NEAP is tentative as the process had not been subject to a rigorous evaluation at the time he conducted his work, which the results of the Indecon (2005) study lack methodological rigour.

IV The Data

The population for our study relates to all individuals in the Republic of Ireland who made a claim for unemployment benefit over a 13 week period between September and December 2006. Attempts were made to issue a questionnaire to all claimants that collected additional data on key labour market variables.[13] This information was then matched with weekly Live Register files for the period September 2006 through to July 2008 in order to determine (i) the point at which unemployment benefit claims were approved, (ii) the point at which claims were closed and (iii) the reason behind any claim closure.[14] We also incorporated additional background and activation-related information on the claimants from the Live Register files. Finally, these data were merged with the customer events files of FÁS, then the national body with responsibility for managing JSA and training, in order to determine the point at which individuals received (i) a JSA referral letter, (ii) an actual interview and (iii) any referral to further training. The resulting dataset from this data construction process is quite unique in both its detailed individual controls and the accuracy of the activation information contained within it. It should be noted that the referral letter and interview are typically separated by a period of approximately 2 to 3 weeks, with the result that we cannot separate out these impacts in our analysis.

Despite the richness of the data at hand, the universal nature of the JSA referral process presented us with a potentially difficult task with respect to finding an acceptable comparison group to conduct a counter-factual evaluation of the JSA component of the NEAP process. Under the NEAP, all claimants should be automatically referred to FÁS by the DSP when their claim reaches the 13 week duration point on the Live Register. However, when we merged the customer Live Register data with the FÁS events file, we found that a substantial proportion of qualifying claimants were not, in fact, referred for activation. This policy failure, thus provides us with a workable control group. We refer to this non-intervention comparison group as Control Group I. The validity of this control group is central to the reliability of the results contained within this analysis. To ensure that the members of Control Group I were, in fact, eligible for assistance and that no other factors drove the non-referral process, the details of each claimant was sent to DSP for examination. It was confirmed through a series of thorough checks, at the individual claim level, carried

[13] The primary objective in administering this questionnaire was to develop a statistical profiling model for long-term unemployment risk in Ireland (see O'Connell, McGuinness, Kelly and Walsh, 2009).

[14] We received 91 separate files containing information on the Live Register population in each week. In addition, we were given 91 closure file containing details of the claims that had been closed off the Register in the previous week.

out by both the DSP and FÁS, that Control Group I represented a legitimate control group. While no definitive explanation could be provided for the non-referral of control group members, both agencies conceded that referral did not take place in line with the rules of the programme. The most plausible explanation provided to us related to systematic administrative failures stemming from the imbalance between eligible claimants and interview slots. Each week a computer-generated list of claimants reaching the 13 week threshold and eligible for NEAP intervention was sent to individual DSP offices; however, it seems that if the number of potential referrals exceeded the number of available interview slots at local FAS offices, then some claimants were set aside to be referred at a later date. It seems that there were no checks or procedures in place to queue postponed referrals and, as a consequence, many were never activated. Descriptive validation checks confirmed that membership of Control Group I was generally random with few characteristic differences apparent relative to the main Treatment Group. In order to ensure that the generation of the control group was not systematic in nature and that case workers did not cream individuals with regard to available slots, we regressed both the treatment and control group on the intervention variable. The results indicate little variation on the characteristics of both groups, thus confirming the random nature of the control group generating process.[15] Irrespective of the cause, what is obvious is that the failure to refer a large proportion of claimants for activation would have been much less likely had the DSP and FÁS recording systems been fully integrated. The unintended benefit of this policy failure, however, was to generate a randomly selected control group with which to assess the impact of JSA. Thus, in many respects, the current study can be characterized as a natural experiment.

It has been the practice under the NEAP that unemployed individuals are activated only once. Thus, current claimants that went through the NEAP activation process during a previous unemployment spell are not intervened with a second time i.e. they are excluded from the activation process. The use of such a rationing rule would seem to exclude high-risk individuals, experiencing multiple spells of unemployment, from future assistance. We made use of this somewhat peculiar and counter-intuitive policy rule to construct a second control group consisting of claimants with a previous JSA exposure that took place more than two years prior to the current claim period. [16] The underlying rationale for this control group, hereafter referred to as Control Group II, is that we might expect a lower exit rate, relative to

[15] Results available from the authors.

[16] We excluded claimants who were also referred for training during a previous unemployment spell.

the Treatment Group, on the grounds that the impact of their previous support would have eroded over time. Furthermore, previous advice would be less relevant to both current labour market conditions and the claimant's human capital, and previous clients would not have current access to FÁS labour market placement services,[17] suggesting that effective JSA should lead to a positive, if somewhat reduced, treatment effect relative to this second control group.

The data generation process involved in the evaluation of the JSA component of the NEAP, which is described next, is set out in Table 1. Between September and December 2006, a total of 60,189 individuals signed on the Live Register to receive a social welfare payment. Just over 15,000 of these individuals failed to complete the questionnaire that was administered to collect additional labour market information.[18] When account is taken of this, and when non-awarded claims, duplicates and claim types ineligible for unemployment assistance are eliminated also, the sample for our JSA evaluation fell to 27,328.

In order to enhance the integrity of the comparison between treatment and control groups, and to ensure the robustness of our estimates, we made a series of additional restrictions to the data. 1. We are primarily interested in employment outcomes, so all 2,011 individuals whose claims were transferred to a non-unemployment related welfare payment were removed from the sample. 2. As our key measure of success relates to a successful exit to employment, 1,954 individuals, whose unemployment benefit claim was closed for an unknown/unstated reason, were also removed from the sample. However, interestingly, our central results were largely unchanged when our models were re-estimated on all closures, as opposed to employment closures only, suggesting that claim closures related to unknown or administrative factors did not represent a biasing factor in this instance. 3. We also excluded 1,491 individuals who were referred to, or subsequently received, FÁS training, on the grounds that persons assigned to training courses were likely to desist from job search until such time as the training commenced. Such lock-in effects would, by definition, downwardly bias any treatment effect. These eliminations resulted in a further reduction in our sample to 21,872, of which, 7,654 had previously been activated under the NEAP prior to the current study period (i.e. prior to September 2006), while the remaining 14,218 had not and, as such,

[17] This describes the process whereby FÁS actively attempts to match claimants with existing job vacancies.

[18] In effect, this represents a 25 per cent non-response rate to a census of new claimants of unemployment-related payments during the period. O'Connell *et al.* (2009) report that the non-respondent group did not differ in any significant respect from the observed 'sample' that completed the profiling questionnaire.

were classified as new NEAP clients. Of the new client component, 7,368 were referred to FÁS for employment advice and it is this grouping that represents our designated Treatment Group. 4. We also excluded the following types of claimants from our sample: (i) re-entrants to the Live Register;[19] (ii) unrecorded closures[20], and (iii) individuals undertaking some form of employment at the commencement of their claim[21]. After these final exclusions, our Treatment Group consisted of 5,314 individuals. On average, with respect to our Treatment Group, claimants received a referral for JSA during week 15 of the claim, with interviews typically conducted after approximately 17 weeks duration on the Live Register.

<Table 1>

The next step in our analysis was to construct two control groups. As stated, the first control group consists of 4,623 new clients that should have been, but were not, referred for JSA during the study period (Control Group I). Control Group II relates to the population of 2,677 previous NEAP clients that have been off the Live Register for at least two years, whose previous intervention was limited to the referral plus interview process. 5. Our fifth and final adjustment of the sample was to include only those individuals who qualified for activation under the NEAP: i.e. those with an unemployment duration of 13 weeks or more. After applying this restriction, the size of the treatment group was 4,979,[22] while Control Groups I and II adjusted to 2,101 and 2,272.[23] Furthermore, in order to account for the impacts of dynamic bias, we follow Sianesi (2004) by including all individuals not treated at a particular point in time within Control Group I. The cut-off point for inclusion within the treatment grouping was week 40 on the Live Register, thus ensuring that treated individuals had a minimum of 12 weeks to act on the advice received during the interview. It should, however, be noted that the general endogeniety bias that impacts selection of the control and treatment groups, i.e. that individuals not successful in finding a job will be treated at some point in the future, is less relevant in the Irish context. The DSP system was designed to

[19] The decision to exclude re-entrants is based on the assumption that a successful intervention is one which facilitates a sustained exit from the Live Register to employment. Furthermore, such individuals are difficult to classify given that they have exited the Register for a period but may be present on it at the key reference point.

[20] Instances where the individual has been dropped from the Live Register but no record of the claim is contained within any of the weekly closure files.

[21] Under welfare entitlement rules, claimants are permitted to engage in employment for a limited number of hours per week. Such individuals were excluded to guard against the possibility that such status may have been a driving factor in the generation of Control Group I.

[22] Approximately 10 per cent of the treatment sample received interviews prior to the 13 week point, presumably as a consequence of walk-ins. Individuals self-initiating contact with the activation service are likely to have higher levels of motivation etc, thus, their inclusion in the sample will leave the results more prone to unobserved heterogeneity bias.

[23] These figures are prior to the adjustment for dynamic bias. Following the re-coding of individuals treated after week 26, a further 981 individuals transferred from the treatment to control groups.

sweep the Live Register dataset to identify individuals who reached 13 week duration on the register, but there existed no further checks. Therefore, any individual not activated at the 13 week point would only subsequently be activated if (a) they voluntarily presented themselves for activation or (b) their non-activation status had been uncovered by a case worker and the situation rectified. Following our adjustment for dynamic bias, 341 individuals (16 per cent) were reassigned from the treatment to the control groups.[24]

Within the context of our current sample, existing rules precluded almost 25 per cent of, arguably the most disadvantaged, claimants from receiving employment advice or assistance under the NEAP programme. Another important implication of the data generation process is that, based on the population of 7,080 new clients with at least 13 weeks duration, all of whom were eligible for assistance under the rules of NEAP, approximately 30 per cent were not referred to FÁS by the DSP system. This demonstrates that, as a consequence of administrative failures and perverse rationing rules, a large number (almost half) of individuals fell outside the activation net, with potentially serious negative consequences with respect to both individual employment chances and government exchequer costs.

We can get a preliminary feel for the extent to which the employment outcomes for the treatment and control groups differ by plotting the Kaplan-Meier (KM) survival function for each grouping. The KM plots the rate at which the proportions remaining on the Live Register decline with duration. Clearly, as we restrict membership of each group to individuals with minimum unemployment duration of 13 weeks, we observe no decline in the KM before that point (Figure 1). Similarly, as the event of interest relates to an exit to employment at or before 52 weeks, the KM function is not shown after this point. The KM suggests that the Treatment Group have a higher exit rate than Control Group II. However, the Treatment Group appear considerably less likely to exit to employment when compared with Control Group I. These KM patterns are confirmed by the fact that the percentage of claims closed to employment by the 12 month duration point stood at 50.7 per cent, 69.5 per cent and 40.8 per cent for the Treatment Group and I and II respectively. The markedly lower exit rate of the Treatment Group relative to Control Group 1 is alarming and provides an initial indication that some component of the activation process may actually substantially

[24] Over 70 per cent of treatment group interviews took place prior to the 20 week duration point. Week 15 represented the median week interview point,

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slow the rate of job search intensity among claimants. Nevertheless, it would be premature to attach a great deal of importance to this descriptive analysis as it fails to control for any observable differences in the characteristics of the treatment and control groups that potentially influence the probability of an exit to employment.

<Figure 1>

V Methodology

The range of potential methodological approaches to the evaluation of ALMPs includes matching estimates, duration models, and difference-in-difference estimates. We opt for a matching based approach as it has several advantages: (i) it facilitates a more straightforward mechanism to account for sample selection bias, (ii) it allows us to easily test the sensitivity of our model to various unemployment durations, and (iii) it allows for the straightforward calculation of relevant marginal effects. The difference-in-difference estimator would require a dataset which observes both a treatment and control group in two periods. However, as non-exit from the Live Register in period one is a necessary condition for treatment in period two, the method cannot be applied in this instance. The duration model approach is rejected on the basis that it is difficult to derive meaningful marginal effects and simultaneously control for the effects of sample selection and unobserved heterogeneity. Nevertheless, as a robustness check, we generated an estimate of the treatment effect using a Cox Proportional Hazard model and the principal conclusions were found to hold.[25]

VI Results

The Impact of Referral and Interview

We report the results for three separate models in Table 2: Model 1 compares the Treatment Group with Control Groups I and II combined; Model 2 compares the Treatment Group with Control Group I; and Model 3 compares the Treatment Group with Control Group II.

[25] Results available from the authors on request.

The results from our initial probit model indicate that exit from the Live Register to employment at or before the 52 week unemployment duration point was positively related to educational attainment, being male, having access to one's own transport, recent employment and job duration, a willingness to move for a job and low spousal income. On the other hand, the probability of an exit to employment was found to be negatively related to increased age, the existence of dependent children, being separated or divorced, literacy difficulties, relatively high spousal earnings, a prior history of long-term employment, previous participation in a Community Employment (CE) scheme[26] and receipt of the non-contributory based Jobseeker's Allowance (JA). These results were consistent across all three specifications, and align well with prior expectations and previous research into long-term unemployment profiling in Ireland (O'Connell *et al*., 2012). All models contain 26 dummies that control for the claimants geographical location, although these are not reported in Table 2.[27]

In relation to our variable of interest, which measures the combined impact of both the referral letter and the activation interview, after controlling for all observables it was found that members of the Treatment Group were 8 per cent less likely to exit to employment prior to being 12 months on the Live Register relative to the sample consisting of the combined control groups (Model 1). Model 2 revealed that, at 15 per cent, the level of disadvantage was highest relative to Control Group I, whereas Treatment Group members were 6 per cent more likely to exit to employment before 12 months duration relative to Control Group II (Model 3). The initial results from the three probit models suggest that the referral and interview component of the NEAP is ineffective in terms of achieving a successful re-entry to employment given that individuals with exposure to it (both Treatment and Control Group II) have substantially poorer labour market outcomes relative to Control Group I.

There are two potential explanations for this result: the first is that claimants were simply given bad advice, while the second relates to the possibility that NEAP participants learned, as a consequence of the process itself, that they were unlikely to face any sanctions or rigorous monitoring as a consequence of a failure to search for or obtain employment.[28]

[26] A relatively large-scale public sector job creation scheme.
[27] Results available from the authors on request.
[28] Another potential explanation for the negative result derived for the Treatment Group is that the quality of jobs available in the economy at that time did not match their skills and, thus, these individuals were postponing entry into the labour

The explanation relating to bad advice seems unlikely given that Control Group II would have had sufficient time to realise that previous advice given under the NEAP process was unhelpful, which should, in turn, have led to a significantly higher exit rate relative to Control Group I. The second interpretation is more plausible: the negative effect of the referral and interview process may be the result of NEAP clients learning in the course of the process that they were unlikely to undergo monitoring or face sanctions as a result of failure to search actively for, or obtain, employment, leading to some decline in job search activity among both the Treatment Group and Control Group II, resulting in poorer outcomes relative to Control Group I. Nevertheless, the positive treatment coefficient relative to claimants with previous NEAP exposure (Control Group II) is somewhat more difficult to explain, given that former NEAP clients would also have been aware of the lax activation regime. Thus we would not, a priori, expect any significant difference between them and the Treatment Group. However, it is likely that claimants with previous exposure to NEAP may have had a more developed understanding of the light-touch nature of activation, which may have resulted in a fall in job search intensity levels relative to the Treatment Group. Furthermore, the Treatment Group would have had access to the FÁS job placement services which should, in its own right, have also led to some positive impacts relative to Control Group II.

The superior performance of Control Group I leads us to infer that a lack of effective monitoring and sanction mechanisms, tied to insufficient job search requirements, represents a substantial factor in the observed negative effect of the NEAP referral and JSA interview process in Ireland. When we estimate the effect using a Cox Proportional Hazard model we also observe strong negative effects for Models 1 and 2, and a positive impact in Model 3.[29]

<Table 2>

Robustness Checks

If assignment to the Treatment Group was in some way systematic, for example, if individuals with superior (inferior) human capital characteristics were more likely to be

market until better quality jobs became available. However, Ireland was in the middle of an economic boom during the time period covered in this paper, a period characterised by relatively high wages, low unemployment and considerable inward migration to fill the labour shortages that existed in the country; thus, it is unlikely that suitable high paying jobs did not exist to entice the Treatment Group to re-enter the labour market.

[29] Results available from the authors on request.

assigned to treatment, then failure to take account of such non-random assignment would upwardly (downwardly) bias the estimated NEAP programme impact. Evaluation studies of this kind typically deal with this issue by employing a Propensity Score Matching (PSM) estimation framework (Rosenbaum and Rubin, 1983). There are a number of PSM algorithms that can be estimated and, while each has some obvious advantages and drawbacks, no single method is generally considered to be superior (Caliendo and Kopeinig, 2008). We estimate the impact of the treatment (i.e. the referral plus interview process) on the probability of an exit to employment using two of the most commonly adopted PSM algorithms: (i) Nearest Neighbour with replacement (NN), and (ii) Kernel.

The PSM Stage 1 pooled probit model was well specified with a pseudo R^2 of 0.15 (Appendix Table A1, Model 1). Relative to both control groups, we found that the factors that raised an individual's probability of being referred for interview were being aged below 25, having a third-level qualification, having difficulties in English proficiency (possibly relating to immigrants), reporting good health, being in receipt of JB (which indicates having been recently employed), having a high earning spouse, no previous exposure to long-term unemployment or the CE scheme and not having access to one's own transport. Some geographic differences were also found. Intuitively, the probit model makes sense, given that the rule restricting access to the NEAP on a second occasion (Control Group II) would have tended to skew the profile towards younger more educated first time claimants. When we examine the Stage 1 probits related to the individual control groups, we find that the bulk of distinguishing factors do indeed relate to Control Group II, which is confirmed by the fact that the pseudo R^2 for this model is 0.28 indicating some distinct differences relative to the Treatment Group (see Appendix Table A1). In contrast, the pseudo R^2 of the model contrasting the Treatment Group with Control Group I is just 0.058 with relatively few statistical differences emerging between both groups, confirming our assertion that any differences between the two groups are relatively minor in nature, which supports the absence of any systematic selection process with respect to membership of Control Group I.

The results from the Stage 2 procedure are reported in Table 3, and are very much in line with those reported in the earlier probit models (Table 2). This outcome suggests that sample selection is not an important factor in this study. Nevertheless, we should not take these results as final without conducting some integrity checks to ensure that individuals within the Treatment Group were, indeed, matched with Control Group counterparts holding

similar characteristics i.e. that our data is balanced.[30] The diagnostics indicate that Models 1 and 2 were perfectly balanced under the Kernel approach,[31] with all three models generating post-balancing pseudo R^2 values close to zero under the Nearest Neighbour approach[32] all of which suggests that our data was sufficiently balanced to produce reliable results.

<Table 3>

It is possible that the full effect of the referral plus interview process was not apparent at the 12 month point. To guard against the possibility of delayed impacts, we re-estimate the models to consider the effect of the treatment on the probability of an exit to employment at both 15 and 18 month time points[33] (Tables 6 and 7).[34] The overall pattern of results remains unchanged;[35] however, some movement is observed with respect to the magnitude of the marginal effects associated with the treatment. Relative to the 12 month model, the negative impact of the treatment under the various estimation procedures falls back somewhat for the pooled controls (Model 1) in the 15 month model before rising again in the 18-month model. The negative impact of the treatment increases over both periods relative to Control Group I (Model 2), while the positive impact of the treatment relative to Control Group II falls off somewhat between 15 and 18 months (Model 3). Overall, the 15 and 18 month model results reinforce the hypothesis that claimants' who have participated in the NEAP referral and interview process subsequently relax their job search activity, as it is difficult to envisage any plausible alternative explanation that would persist over an 18 month observation period. For example, one would expect that if bad advice at the initial interview was a strong explanatory factor driving our earlier results, claimants should have realised this and adjusted their behaviours accordingly over the longer time period, and a substantial decline in the negative

[30] Specifically, this test involves re-running the Stage 1 treatment model on a sample consisting of the treated and matched individuals. On the basis that both sets of individuals should share all, or most, characteristics, no differences should be apparent and the probit model should be at, or close to, statistical significance with the pseudo R^2 statistic also close to zero.

[31] On the grounds that the chi-square statistic of the probit estimated within the matched sample was insignificant.

[32] For model 1, the pseudo R^2 fell from 0.113 to 0.007 under NN and 0.001 under the Kernel. For model 2, the pseudo R^2 fell from 0.055 to 0.007 under NN and 0.002 under the Kernel. For model 3, the pseudo R^2 fell from 0.235 to 0.012 under NN and 0.00 under the Kernel.

[33] Within these models, the FÁS interview cut-off points for reassignment from the treatment to control groups was raised to 50 and 60 weeks respectively.

[34] These relate to weeks 78 and 91 in the data, which, allowing for the population capture period between weeks 1 to 13, allows for a minimum time period of 65 weeks (15 months) and 78 weeks (18 months) between the initial claim and the assessment point.

[35] Again, the probit measures show some positive effects relative to Control Group II which are not present within the matching estimates which will adjust for a rising concentration of long-term unemployed within the Control group II samples.

magnitude of the treatment relative to Control Group I would be expected. Furthermore, when compared to individuals who participated in an activation interview during a previous unemployment spell that took place more than two years prior to the current study (i.e. Control Group II), the advantage of current participants declined over time, reinforcing the view that the initial advantage may have been drive by access to FÁS placement services.

<Table 4>

<Table 5>

Unobserved Heterogeneity

PSM analysis is underpinned by a strong assumption of unconfoundedness, whereby it is assumed that we observe all variables that influence both the outcome and treatment effects. While we possess a very detailed dataset, it is not unreasonable to question whether our results may still be prone to bias as a consequence of some unobserved factor that simultaneously influences both the outcome variable and assignment to the treatment. However, it should also be noted that, given the apparently arbitrary criteria for allocation to the treatment and control groups, no obvious confounding influences are apparent. Nevertheless, as an additional check against the effects of unobserved heterogeneity bias, we test the sensitivity of our PSM estimates to unobserved factors using the MHBOUNDS procedure in STATA.[36] The intuition here is that we introduce an unobserved factor that simultaneously reduces the likelihood of an exit to employment and increases the likelihood of allocation to the Treatment Group (termed negative selection bias) by 10 per cent to assess if our estimated treatment effect remains statistically reliable. Thus, the approach measures the sensitivity of our estimates to unobserved confounding influences. The analysis reveals that our Nearest Neighbour with replacement (NN) PSM estimate for the combined sample would remain statistically reliable in the presence of an unobserved confounding factor that simultaneously decreased the likelihood of exit and increased the likelihood of allocation to the combined treatment group by 500 per cent (Table 6). The NN treatment estimate for Control Group I is also robust to the presence of negative selection bias that increased the likelihood of treatment by 500 per cent (Table 7). Therefore, while we cannot rule out the presence of unobserved heterogeneity we can conclude that such impacts would need to be particularly strong in order to cast doubt on our estimated treatment effects.

[36] The analysis conducted in this study was undertaken using a statistical package called STATA, and MHBOUNDS is a procedure within STATA designed to deal with the issue of unobserved heterogeneity.

<Table 6>

<Table 7>

VII Summary and Conclusions

This study is distinctive in three respects. First, it analyses the impact of a job search assistance (JSA) programme in an environment where both monitoring and sanctions are virtually non-existent. This is in contrast to the bulk of the literature, which is unable to disentangle the effects of JSA from these other components. Second, the data are based on a unique combination of a specially designed questionnaire administered at the beginning of unemployment spells matched with official administrative data that tracked individuals through up to 18 months of unemployment. Third, the control groups in the study consist of individuals who qualify for activation but were missed as a consequence of administrative errors in an apparently random manner (Control Group I), and individuals excluded from assistance on the grounds that they had previous exposure to the activation system (Control Group II).

We found that, relative to a population consisting of both control groups, individuals receiving a referral letter and\or an activation interview were 8 per cent less likely to have exited to employment prior to12 months duration on the Live Register. Disaggregating the control groups, we found that the general effect was driven by a 15 per cent disadvantage relative to Control Group I, while a positive difference of 6 per cent was observed with respect to Control Group II. We conclude from the analysis that participants attending an activation interview learned that any prior fears that they might have held with respect to the extent of job search monitoring and sanctions were unfounded and, consequently, may have lowered their job search activity levels relative to Control Group I who had no exposure to the activation process. We also conclude that it is unlikely that the estimated treatment effect can be attributed to poor quality advice during the interview process on the grounds that the negative effect remains apparent over the medium-term during which individuals would have had ample opportunity to revise their expectations and behaviour. The results were found to be robust to the influences of both sample selection and unobserved heterogeneity bias. The research highlights the ineffectiveness of JSA when delivered within a policy environment not underpinned by effective job search monitoring and sanctions.

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Tables

Table 1: Treatment and Control Group Information

NEAP Data	Numbers
Original Live Register Population:	60,189
Excluding non relevant claims, non awarded claims, duplicates and incomplete questionnaires	27,328
Live Register Leavers' Sample Adjustments:	
1. Transferred to alternative (non-unemployment) welfare payment	-2,011
2. Unknown reason for closure cases eliminated from sample	-1,954
3. Individuals in receipt of training or referred to training	-1,491
Initial NEAP Sample:	21,872
Old FÁS clients	7,654
Of which historic interventions were light or no interventions	4,399
New FÁS clients	14,218
Of which - Interventions (Treatment Group)	7,368
- No interventions	6,850
NEAP Sample Including Old Clients With Historic Light or No Interventions:	18,617
4.i. Exclude re-entrants to Live Register from sample	*5,018*
4.ii Exclude un-recorded closures	*337*
4.iii Exclude those in employment at time of survey	648
Final NEAP Sample:	12,614
Of which	
1. Treatment Group	5,314
2. Control Group I: New clients (untreated)	4,623
3. Control Group II: Old clients with historic light or no interventions	2,677
Restricting the Analysis to Observations after 13 Weeks Duration on the Live Register:	9,352
5.1. Treatment Group	4,979
5.2. Control Group I: New clients (untreated)	2,101
5.3. Control Group II: Old clients with historic light or no interventions	2,272

Table 2: Probit Models for Exit to the Labour Market Prior to 12 Months Unemployment Duration

	Model 1 Both Control Groups	Model 2 Control Group I	Model 3 Control Group II
NEAP Intervention:			
FÁS Referral plus Interview	-0.08***	-0.15***	0.06***
	(0.012)	(0.014)	(0.015)
Personal and Family Characteristics:			
Male	0.07***	0.09***	0.07***
	(0.013)	(0.014)	(0.014)
Age Reference Category: Aged 18-24			
Age 25-34	-0.06***	-0.01	-0.02
	(0.017)	(0.020)	(0.020)
Age 35-44	-0.11***	-0.08***	-0.08***
	(0.020)	(0.024)	(0.022)
Age 45-54	-0.11***	-0.08***	-0.08***
	(0.023)	(0.027)	(0.025)
Age 55+ Years	-0.25***	-0.27***	-0.22***
	(0.023)	(0.027)	(0.026)
Health Reference Category: Bad/Very Bad Health			
Very Good Health	0.11*	0.13*	0.07
	(0.059)	(0.074)	(0.066)
Good Health	0.07	0.09	0.04
	(0.059)	(0.074)	(0.067)
Fair Health	-0.03	-0.01	-0.05
	(0.063)	(0.078)	(0.068)
Marital Status Reference Category: Single			
Married	-0.02	-0.04	-0.01
	(0.020)	(0.023)	(0.022)
Cohabits	-0.01	-0.04	-0.02
	(0.028)	(0.033)	(0.032)
Separated/Divorced	-0.08***	-0.10***	-0.08***
	(0.030)	(0.036)	(0.032)
Widowed	-0.01	-0.04	-0.03
	(0.069)	(0.075)	(0.081)
Children	-0.05***	-0.05***	-0.04***
	(0.009)	(0.011)	(0.010)

Table 2: continued

	Model 1 Both Control Groups	Model 2 Control Group I	Model 3 Control Group II
Spousal Earnings Reference Category: None			
Spouse Earnings €250	0.12***	0.11***	0.14***
	(0.032)	(0.035)	(0.037)
Spouse Earnings €251-€350	0.00	0.02	-0.00
	(0.081)	(0.085)	(0.088)
Spouse Earnings €351 and Above	-0.09***	-0.11***	-0.08***
	(0.022)	(0.024)	(0.024)
Human Capital Characteristics:			
Education Reference Category: Primary or Less			
Junior Certificate	-0.00	0.01	0.01
	(0.020)	(0.024)	(0.022)
Leaving Certificate	0.06***	0.09***	0.06**
	(0.020)	(0.023)	(0.022)
Third-level	0.18***	0.18***	0.16***
	(0.020)	(0.024)	(0.024)
Apprenticeship	0.04***	0.03*	0.05**
	(0.017)	(0.019)	(0.019)
Literacy/Numeracy Problems	-0.08***	-0.08***	-0.08***
	(0.023)	(0.028)	(0.025)
English Proficiency	-0.03	-0.03	-0.02
	(0.034)	(0.038)	(0.037)
Employment/Unemployment/Benefit History:			
Employment History Reference Category: Never			
Employed in Last Month	0.11***	0.12***	0.12***
	(0.038)	(0.045)	(0.042)
Employed in Last Year	0.07*	0.08*	0.08*
	(0.039)	(0.045)	(0.044)
Employed in Last 5 Years	-0.02	-0.01	0.00
	(0.041)	(0.049)	(0.046)
Employed Over 6 Years Ago	-0.02	-0.05	-0.02
	(0.051)	(0.064)	(0.057)

Table 2: continued

	Model 1 Both Control Groups	Model 2 Control Group I	Model 3 Control Group II
Job Duration Reference Category:			
Never Employed			
Job Duration Less than Month	0.09**	0.11**	0.06
	(0.041)	(0.048)	(0.047)
Job Duration 1-6 Months	0.11***	0.15***	0.12***
	(0.035)	(0.041)	(0.039)
Job Duration 6-12 Months	0.11***	0.14***	0.09**
	(0.036)	(0.042)	(0.041)
Job Duration 1-2 Years	0.08**	0.13***	0.06
	(0.037)	(0.043)	(0.042)
Job Duration 2+ Years	0.01	0.03	-0.02
	(0.036)	(0.043)	(0.039)
Would Move for a Job	0.06***	0.07***	0.06***
	(0.012)	(0.014)	(0.014)
Social Welfare Payment Type Reference Category:			
Jobseeker's Benefit			
Jobseeker's Allowance	-0.23***	-0.23***	-0.22***
	(0.014)	(0.017)	(0.015)
Signing on the Live Register for 12 Months Plus	-0.21***	-0.17***	-0.12***
	(0.017)	(0.035)	(0.020)
CE Scheme Previous 5 Years	-0.04	0.03	-0.04
	(0.044)	(0.060)	(0.047)
On CE Scheme for 12 Months Plus	-0.19***	-0.27***	-0.13***
	(0.047)	(0.064)	(0.051)
Geographic Location Information:			
Location Reference Category: Rural			
Village	-0.03	-0.02	-0.04**
	(0.020)	(0.023)	(0.022)
Town	-0.02	-0.01	-0.01
	(0.019)	(0.022)	(0.021)
Large Town/City	-0.02	-0.02	-0.01
	(0.019)	(0.022)	(0.021)

Table 2: continued

	Model 1 Both Control Groups	Model 2 Control Group I	Model 3 Control Group II
Own Transport	0.08***	0.09***	0.08***
	(0.013)	(0.016)	(0.015)
Public Transport	-0.00	-0.01	0.00
	(0.016)	(0.019)	(0.018)
Observations	9,315	7,057	7,232
Pseudo R^2	0.153	0.160	0.142

Note: Standard errors in parentheses.

* significant at 10 per cent; ** significant at 5 per cent; *** significant at 1 per cent.

Table 3: PSM Estimates of Treatment Effect

	FÁS Interview (Nearest Neighbour)	**FÁS Interview** (Kernel)
Control Group I & II (Model 1)	-0.064 (0.015)***	-0.077 (0.0012)***
Control Group I (Model 2)	-0.103 (0.017)***	-0.126 (0.013)***
Control Group II (Model 3)	0.060 (0.021)***	0.059 (0.016)***

Table 4: Estimates of Treatment Effect: 15 Month Model

	FÁS Interview (Probit)	**FÁS Interview** (Nearest Neighbour)	**FÁS Interview** (Kernel)
Control Group I/ II (Model 1)	-0.071 (0.013)***	-0.067 (0.016)***	-0.067 (0.012)***
Control Group I (Model 2)	-0.141 (0.014)***	-0.126 (0.018)***	-0.129 (0.014)***
Control Group II (Model 3)	0.072 (0.017)***	0.061 (0.025)***	0.069 (0.018)***

Table 5: Estimates of Treatment Effect: 18 Month Model

	FÁS Interview (Probit)	**FÁS Interview** (Nearest Neighbour)	**FÁS Interview** (Kernel)
Control Group I/ II (Model 1)	-0.095 (0.012)***	-0.091 (0.016)***	-0.083 (0.012)***
Control Group I (Model 2)	-0.179 (0.014)***	-0.155 (0.017)***	-0.156 (0.013)***
Control Group II (Model 3)	0.041 (0.017)**	0.031 (0.025)**	0.032 (0.019)*

Table 6: Mantel-Haenszel Bounds for NN Estimate for Control Groups I & II

Gamma	Q_mh+	Q_mh	p_mh+	p_mh-
1	5.09	5.09	0.00	0.00
1.2	8.63	1.57	0.00	0.06
1.4	11.63	1.35	0.00	0.09
1.6	14.26	3.92	0.00	0.00
1.8	16.59	6.20	0.00	0.00
2	18.69	8.24	0.00	0.00
2.2	20.61	10.10	0.00	0.00
2.4	22.38	11.80	0.00	0.00
2.6	24.01	13.37	0.00	0.00
2.8	25.54	14.84	0.00	0.00
3	26.98	16.21	0.00	0.00
3.2	28.33	17.50	0.00	0.00
3.4	29.61	18.71	0.00	0.00
3.6	30.83	19.87	0.00	0.00
3.8	31.99	20.97	0.00	0.00
4	33.10	22.02	0.00	0.00
4.2	34.17	23.02	0.00	0.00
4.4	35.19	23.98	0.00	0.00
4.6	36.18	24.91	0.00	0.00
4.8	37.13	25.80	0.00	0.00
5	38.05	26.66	0.00	0.00

Note: Gamma : odds of differential assignment due to unobserved factors
Q_mh+ : Mantel-Haenszel statistic (assumption: overestimation of treatment effect)
Q_mh- : Mantel-Haenszel statistic (assumption: underestimation of treatment effect)
p_mh+ : significance level (assumption: overestimation of treatment effect)
p_mh- : significance level (assumption: underestimation of treatment effect)

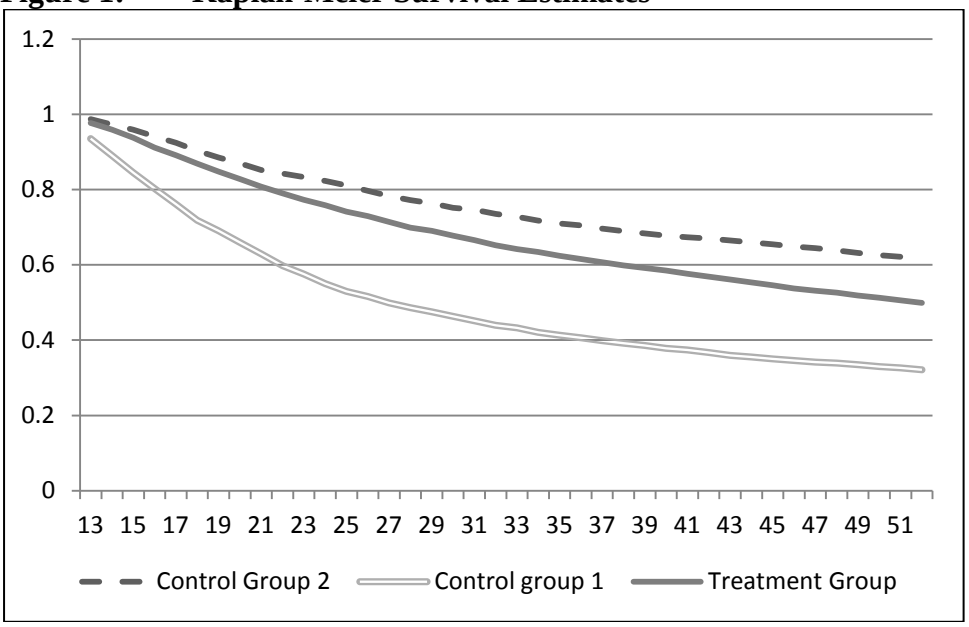
Table 7: Mantel-Haenszel Bounds for NN Estimate for Control Group I

Gamma	Q_mh+	Q_mh	p_mh+	p_mh-
1	8.8	8.8	0.0	0.0
1.2	12.0	5.7	0.0	0.0
1.4	14.7	3.0	0.0	0.0
1.6	17.1	0.7	0.0	0.2
1.8	19.3	1.2	0.0	0.1
2	21.2	3.1	0.0	0.0
2.2	23.0	4.7	0.0	0.0
2.4	24.6	6.2	0.0	0.0
2.6	26.1	7.6	0.0	0.0
2.8	27.5	8.9	0.0	0.0
3	28.9	10.1	0.0	0.0
3.2	30.2	11.2	0.0	0.0
3.4	31.4	12.3	0.0	0.0
3.6	32.5	13.3	0.0	0.0
3.8	33.6	14.3	0.0	0.0
4	34.7	15.2	0.0	0.0
4.2	35.7	16.1	0.0	0.0
4.4	36.7	16.9	0.0	0.0
4.6	37.6	17.7	0.0	0.0
4.8	38.5	18.5	0.0	0.0
5	39.4	19.3	0.0	0.0

Note: Gamma : odds of differential assignment due to unobserved factors
Q_mh+ : Mantel-Haenszel statistic (assumption: overestimation of treatment effect)
Q_mh- : Mantel-Haenszel statistic (assumption: underestimation of treatment effect)
p_mh+ : significance level (assumption: overestimation of treatment effect)
p_mh- : significance level (assumption: underestimation of treatment effect)

Figures

Figure 1: Kaplan-Meier Survival Estimates



Appendix

Table A1: Stage 1 Probit Models from PSM Estimations

	Model 1 Both Control Groups	Model 2 Control Group I	Model 3 Control Group II
Personal and Family Characteristics:			
Male	-0.01	0.02	-0.03**
	(0.012)	(0.013)	(0.014)
Age Reference Category: Aged 18-24			
Age 25-34	-0.13***	0.01	-0.25***
	(0.016)	(0.018)	(0.020)
Age 35-44	-0.06***	0.08***	-0.20***
	(0.020)	(0.020)	(0.024)
Age 45-54	-0.05**	0.07***	-0.19***
	(0.023)	(0.023)	(0.027)
Age 55+ Years	-0.12***	-0.12***	-0.08**
	(0.026)	(0.029)	(0.035)
Health Reference Category: Bad/Very Bad Health			
Very Good Health	0.13**	0.08	0.15**
	(0.054)	(0.060)	(0.059)
Good Health	0.12**	0.08	0.11**
	(0.054)	(0.058)	(0.057)
Fair Health	0.12**	0.07	0.11**
	(0.056)	(0.057)	(0.054)
Marital Status Reference Category: Single			
Married	0.04**	0.02	0.06***
	(0.019)	(0.020)	(0.022)
Cohabits	0.03	0.00	0.07**
	(0.027)	(0.029)	(0.029)
Separated/Divorced	0.06**	0.04	0.08***
	(0.029)	(0.031)	(0.029)
Widowed	0.05	0.01	0.09
	(0.067)	(0.066)	(0.075)
Children	0.01	0.00	0.01
	(0.009)	(0.009)	(0.010)

Table A1: continued

	Model 1 Both Control Groups	Model 2 Control Group I	Model 3 Control Group II
Spousal Earnings Reference Category: None			
Spouse Earnings €250	0.04	0.05*	0.03
	(0.033)	(0.032)	(0.038)
Spouse Earnings €251-€350	0.17**	0.11	0.18**
	(0.079)	(0.071)	(0.076)
Spouse Earnings €351 and Above	0.02	0.04**	-0.05*
	(0.022)	(0.022)	(0.027)
Human Capital Characteristics:			
Education Reference Category: Primary or Less			
Junior Certificate	-0.02	-0.03	-0.02
	(0.019)	(0.021)	(0.021)
Leaving Certificate	0.00	-0.03	0.04*
	(0.020)	(0.022)	(0.021)
Third-level	0.04**	-0.03	0.12***
	(0.021)	(0.023)	(0.022)
Apprenticeship	-0.00	-0.01	0.01
	(0.016)	(0.017)	(0.018)
Literacy/Numeracy Problems	0.00	0.00	0.01
	(0.023)	(0.024)	(0.025)
English Proficiency	0.10***	0.02	0.13***
	(0.032)	(0.032)	(0.030)
Employment/Unemployment/Benefit History:			
Employment History Reference Category: Never			
Employed in Last Month	-0.06	-0.07	-0.05
	(0.040)	(0.047)	(0.046)
Employed in Last Year	-0.08**	-0.08**	-0.08**
	(0.033)	(0.039)	(0.038)
Employed in Last 5 Years	-0.06	-0.06	-0.04
	(0.035)	(0.041)	(0.039)
Employed Over 6 Years Ago	0.02	0.00	0.03
	(0.036)	(0.040)	(0.038)

Table A1: continued

	Model 1 Both Control Groups	Model 2 Control Group I	Model 3 Control Group II
Job Duration Reference Category:			
Never Employed			
Job Duration Less than Month	-0.08**	0.01	-0.15***
	(0.037)	(0.040)	(0.040)
Job Duration 1-6 Months	-0.05	0.02	-0.13***
	(0.037)	(0.039)	(0.044)
Job Duration 6-12 Months	-0.08**	0.02	-0.16***
	(0.038)	(0.042)	(0.047)
Job Duration 1-2 Years	-0.08*	-0.03	-0.13**
	(0.045)	(0.052)	(0.055)
Job Duration 2+ Years	-0.08**	0.01	-0.15***
	(0.037)	(0.040)	(0.040)
Would Move for a Job	0.01	0.00	0.03**
	(0.012)	(0.013)	(0.014)
Social Welfare Payment Type Reference Category:			
Jobseeker's Benefit			
Jobseeker's Allowance	-0.08***	-0.04***	-0.13***
	(0.014)	(0.015)	(0.016)
Signing on the Live Register for 12 Months Plus	-0.39***	-0.09***	-0.52***
	(0.013)	(0.033)	(0.016)
CE Scheme Previous 5 Years	-0.11**	-0.03	-0.15***
	(0.044)	(0.055)	(0.051)
On CE Scheme for 12 Months Plus	-0.02	0.08	-0.07
	(0.053)	(0.059)	(0.058)

Table A1: continued

	Model 1 Both Control Groups	Model 2 Control Group I	Model 3 Control Group II
Geographic Location Information:			
Location Reference Category: Rural			
Village	-0.01	0.00	-0.03
	(0.020)	(0.021)	(0.023)
Town	-0.01	0.00	-0.02
	(0.019)	(0.020)	(0.022)
Large Town/City	-0.02	0.01	-0.06**
	(0.019)	(0.020)	(0.022)
Own Transport	-0.01	0.00	-0.03
	(0.020)	(0.021)	(0.023)
Public Transport	-0.01	0.00	-0.02
	(0.019)	(0.020)	(0.022)
Pseudo R^2	0.120	0.058	0.235
Observations	9,316	7,057	7,230

Note: Standard errors in parentheses.

* significant at 10 per cent; ** significant at 5 per cent; *** significant at 1 per cent.