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## Carrots without sticks: The impacts of job search assistance in a regime with minimal monitoring and sanctions

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Carrots without Sticks: The Impacts of Job Search Assistance in  
a Regime with Minimal Monitoring and Sanctions

Seamus McGuinness<sup>abc</sup>, Philip, J. O'Connell<sup>ab</sup> & Elish Kelly<sup>ab</sup>

**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. The results indicate that, relative to a control group with no intervention, unemployed individuals that were exposed to the interview letter and participated in JSA were 16 per cent less likely to have exited to employment prior to 12 months. The negative effects of the intervention approximately doubled when those that received a referral letter but did not attend a JSA interview were removed from the data. The results held when tested against the underlying assumptions of the model, and 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.

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# **Carrots without Sticks: The Impacts of Job Search Assistance in a Regime with Minimal Monitoring and Sanctions**

## **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 Rennen 2004; Kirby and Riley 2004; Sianesi 2004; Van den Berg, van der Klaauw and van Ours 2004; Heckman and Vytlačil 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). Within this literature it is usually the case that administrative data is used to assess the impact of compulsory Job Search Assistance (JSA) simultaneously delivered in conjunction with monitoring and sanctions. In such circumstances, it is difficult to isolate the impact of JSA from the other activation tools. However, the Irish case, where JSA has been employed as the core activation tool but with minimal monitoring and almost 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 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 reduced activation threat, via monitoring and/or sanctions, relative to the control group who had little or no knowledge of the nature of activation policy.

## **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, 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 (Martin, 2000; and Kluve, 2006). Furthermore, compared to other ALMPs, such as training, public sector job creation programmes and wage subsidies, JSA rates 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.), including the 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, for 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,<sup>1</sup> and increasingly so as benefit claimants approach the time at which monitoring takes place.<sup>2</sup> 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.<sup>3</sup> 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.<sup>4</sup> 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<sup>5</sup> and Sianesi (2008) for Sweden.<sup>6</sup> However, given that the majority of the evaluated programmes contained JSA measures, monitoring and sanction mechanisms, it is difficult to disentangle the relative influences of each component on the transition from unemployment to work. 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

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<sup>1</sup> 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.

<sup>2</sup> 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.

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

<sup>4</sup> Examples of US studies that have derived this result include Katz and Meyer (1990), Meyer (1990) and Meyer (1995).

<sup>5</sup> 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.

<sup>6</sup> 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.

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 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.

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 Kluge, 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).<sup>7</sup> Under the NEAP process, targeted groups of unemployment benefit recipients - those on either Jobseeker’s Allowance (JA) or Jobseeker’s Benefit (JB)<sup>8</sup> - are to be intervened with after a period of 13 weeks on the Live Register.<sup>9</sup> After this point, jobseekers are referred by the benefit agency, the Department for Social Protection (DSP), to the national training and employment authority, FÁS, 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

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<sup>7</sup> 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 *et al.*, 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.

<sup>8</sup> JA and JB are Ireland’s two unemployment benefits. JA is a means-tested payment and JB is based on social insurance contributions.

<sup>9</sup> The Live Register is Ireland’s official unemployment benefit recipient record.

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: (i) sanctions for voluntary job leaving, (ii) refusal of work and (iii) 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.<sup>10</sup> Thus, the activation framework in place in Ireland, 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.<sup>11</sup>

#### IV The Data

The population for our study relates to all individuals in the Republic of Ireland making 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.<sup>12</sup> 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.<sup>13</sup> We also incorporated additional background and activation-related information on the claimants from the Live Register files. Finally, this data was merged with the customer events files of FÁS, 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.

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<sup>10</sup> 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.

<sup>11</sup> 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) concludes 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. The results of the Indecon (2005) study also lack empirical rigour.

<sup>12</sup> 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 & Walsh, 2009).

<sup>13</sup> 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.

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. 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; thus, providing us with a workable control group. We refer to this non-intervention comparison group as Control Group I. The validity of this control group was confirmed through a series of thorough checks, at the individual claim level, carried out by both the DSP and FÁS. We also conducted a series of data-based integrity checks, which are discussed later.

It is also 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. We made use of this somewhat peculiar and counterintuitive policy rule to construct a second control group consisting of claimants with a previous JSA exposure<sup>14</sup> that took place more than two years prior to the current claim period. 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 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,<sup>15</sup> 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.<sup>16</sup> 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

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<sup>14</sup> We excluded claimants who were also referred for training during a previous spell.

<sup>15</sup> This describes the process whereby FÁS actively attempts to match claimants with existing job vacancies.

<sup>16</sup> 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.

ensure the robustness of our estimates, we made a series of additional restrictions to the data. As our key measure of success relates to a successful exit to employment, individuals whose unemployment benefit claim was closed, but in respect of whom the reason for the closure was either unknown or related to a transfer to an alternative non-unemployment related benefit, were subsequently 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. We also excluded individuals referred to, or subsequently in receipt of, FÁS training on the grounds that persons assigned to training courses are likely to desist from job search until such time as the training commences. Such lock-in effects will, 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, 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. We also excluded the following types of claimants from our sample: (i) late interventions (FÁS interview or referral), the impact of which could not be effectively measured at the 52 week point;<sup>17</sup> (ii) re-entrants to the Live Register;<sup>18</sup> (iii) individuals undertaking some form of employment at the commencement of their claim<sup>19</sup> and, finally, (iv) unrecorded closures.<sup>20</sup> After these final exclusions, our Treatment Group consisted of 4,948 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). Obviously, many claimants in Control Group I would not have received employment advice by simple virtue of the fact that their claim ended prior to the 13 week duration FÁS referral point. Consequently, we further restrict Control Group I to individuals with unemployment durations of 20 weeks or more, which

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<sup>17</sup> The cut-off point was week 45.

<sup>18</sup> 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.

<sup>19</sup> 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.

<sup>20</sup> 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.



reduces the number in this control group to 1,450. Given that over 80 per cent of referrals had taken place within the Treatment Group by this point, we judge 20 weeks to represent a reasonable restriction period; however, we expose this assumption to robustness checks at a later stage. Control Group II relates to the population of 2,010 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 and who had a current duration on the Live Register of at least 20 weeks: within the context of our current sample, existing rules precluded over 25 per cent of, arguably the most disadvantaged, claimants from receiving employment advice or assistance under the NEAP programme. Finally, imposing the 20 week duration restriction on the Treatment Group, to ensure a like-with-like comparison with both control groups, reduced its size to 4,008 individuals.

One important implication of the data generation process is that, based on the population of 7,468 clients with at least 20 weeks duration, all of whom were eligible for assistance under the NEAP, approximately 20 per cent were not referred to FÁS by the DSP system. This suggests that, even if we ignore the practice of excluding previous NEAP participants, a large number of individuals may have fallen outside the activation net, with potentially serious negative consequences with respect to both individual employment chances and government exchequer costs. The reasons behind these non-referrals could not be clearly established during the validation checks with both the DSP and FÁS, despite the fact that both agencies conceded that an error had taken place. The most likely explanation put forward was that individuals were omitted due to local office capacity constraints i.e. a lack of available JSA interview slots in local FÁS offices: at the end of 13 weeks on the Live Register FÁS is sent a list by DSP of new claimants qualifying for NEAP, however, there may be a limited number of interview slots available in the local FÁS offices causing a proportion of eligible claimants to be deferred. It seems that there may not have been sufficient checks available within DSP to ensure that deferred individuals were prioritised when new slots became available, causing them to fall through the activation net. Descriptive validation checks confirmed that membership of Control Group I was generally random with few characteristic differences apparent relative to the main Treatment Group.<sup>21</sup> Nevertheless, this assumption will be tested formally within the empirical analysis section of the paper. Irrespective of the cause, what is obvious is that such a failure 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.

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

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<sup>21</sup> Results available from the authors on request.

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 20 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 similar pattern of exit to Control Group II. However, the Treatment Group appear considerably less likely to exit to employment when compared with Control Group I. This is confirmed by the fact that the percentage of claims closed to employment by the 12 month duration point stood at 41 per cent, 55 per cent and 41 per cent for the Treatment Group and Control Groups I and II respectively. Nevertheless, it would be incorrect 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 cut-off points and unemployment durations, and (iii) it allows for the straightforward calculation of relevant marginal effects. The difference-in-difference estimator relies on a dataset in which it is necessary to observe 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, this implies that 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 generate an estimate of the treatment effect using a Cox Proportional Hazard model.

## 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 both Control Groups combined, Model 2 compares the Treatment Group with Control Group I, while Model 3 compares the Treatment Group with Control Group II.

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, 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 the existence of dependent children, literacy difficulties, increased age, relatively high spousal earnings, a prior history of long-term employment, previous participation in a Community Employment (CE) scheme<sup>22</sup> 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.*, 2009). All models contain 26 dummies that control for the claimants geographical location although these are not reported here.<sup>23</sup>

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 7 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 16 per cent, the level of disadvantage was highest relative to Control Group I, whereas there was no difference between members of the Treatment Group and Control Group II (Model 3) in their likelihood of finding employment within a one year period. 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. There are two potential explanations for this result: the first relates to the possibility that claimants were simply given bad advice, while the second relates to the possibility that NEAP participants learn, as a consequence of the process itself, that they are unlikely to face any sanctions or rigorous monitoring as a consequence of a failure to obtain employment.<sup>24</sup> Such learning of the inconsequential nature of the NEAP interview process could, in turn, lead to some reduction in job search intensity. The insignificant impact relative to claimants with previous NEAP exposure seems consistent with this idea, as these claimants would have already been in possession of this information. This leads us to infer that a lack of effective monitoring and sanction mechanisms, tied to insufficient job search requirements (or placement/training

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<sup>22</sup> A relatively large-scale public sector direct employment scheme.

<sup>23</sup> Results available from the authors on request.

<sup>24</sup> 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 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.

participation), 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 non-significant impact in Model 3.<sup>25</sup>

## <Table 2>

### *Estimating the Threat Effect of the Referral Letter*

Our conclusion that programme participants learn as a consequence of the activation interview of the weakness of monitoring and sanction mechanisms under the NEAP is, to some extent, testable by examining the impact of the activation referral letter on exit rates. A substantially higher rate of exit following the issuing of a letter would support the hypothesis that both the Treatment Group and, by inference, Control Group I perceive activation as a form of threat. Given that referral and interview are generally separated by a period of just two to three weeks, it is not possible to isolate the impact of a referral letter only. Nevertheless, according to our data, approximately 36 per cent of the Treatment Group failed to attend their first interview and excluding this component from our Treatment Group enables us to isolate the impact of the interview process. Specifically, comparing the coefficient effect of attending the interview with the estimated coefficient for referral plus interview gives us an assessment of the magnitude of the referral letter impact on the overall treatment.

However, with respect to our previous sample restrictions, it could be argued that by estimating a model containing a Treatment Group with durations of more than 20 weeks, we are potentially under-estimating the effect of referrals on the grounds that a proportion of claimants would have been closed off the Live Register as a consequence of the perceived sanction threat associated with referral letters received before week 20.<sup>26</sup> To account for this, we re-estimated our models with the revised restriction that individuals in both the Treatment Group and Control Group II had to have a minimum duration of 13 weeks<sup>27</sup> or more, while we maintain the 20 week restriction on Control Group I in order to, again, guard against the possibility that individuals were not referred merely as a consequence of their claims closing before this event could take place.<sup>28</sup> It should be noted that these restrictions will tend to overstate the impact of the NEAP, as the rate of claim closure will tend to be more rapid in the period leading up to week 20 and all such observations are excluded for

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<sup>25</sup> Results available from the authors on request.

<sup>26</sup> To reiterate, referral letters are typically sent out around week 15.

<sup>27</sup> This is the minimum point at which an individual could expect a referral under the NEAP.

<sup>28</sup> This should not actually occur as all referrals should automatically take place at the week 13 point.

Control Group I. Given the underlying cause of Control Group I membership<sup>29</sup>, the overwhelming majority of such closures should reasonably be attributed to Control Group I independent of the NEAP. Furthermore, it would be incorrect to assume that all exits between the referral letter and interview were entirely attributable to NEAP, particularly given that activation occurs at a point when survival rates decline at a naturally high rate (Figure 1); nevertheless, the comparison is still informative. Under the revised Control and Treatment Group assumptions, the NEAP coefficient adjusts from -7.2 to -3.9 (Model 1). When we exclude non-attendees from the sample, the estimated treatment effect of referral letter plus interview attendance then falls from -3.8 to -9.0. This suggests a large positive threat impact associated with the referral letter i.e. the negative effect in respect of the group that was both referred and interviewed is approximately twice that of the group that also included those who did not attend the interview.<sup>30</sup> This supports the view that new clients perceive the NEAP process with an element of fear and that participation in NEAP, and learning the nature of the process, sufficiently lowers the job search activities of the Treatment Group to generate a negative treatment effect relative to Control Group I.

### *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 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 one 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 probit model was well specified with a pseudo  $R^2$  of 0.15 (Appendix Table A1, Model 1) and, 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

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<sup>29</sup> This control group was eligible for treatment but not treated due to administrative error.

<sup>30</sup> Results available from the authors on request.

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 0.30 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 0.067 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 the data were sufficient 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.<sup>31</sup> The diagnostics indicate that Models 1 and 2 were perfectly balanced under the Kernel approach,<sup>32</sup> with all three models generating post-balancing pseudo  $R^2$  values close to zero under the Nearest Neighbour approach<sup>33</sup> all of which suggests that our data was sufficiently balanced to produce reliable results.

### <Table 3>

While, on average, the FÁS activation interview takes place on the seventeenth week of the claim, with referral typically taking place a week or two earlier, we must be mindful of the possibility that many of those in Control Group I might not have been subsequently referred or interviewed but merely dropped off the Live Register at a point before this was arranged. Therefore, on the grounds that 18 per cent of the Treatment Group were activated after the 20 week point, we cannot be certain that a similar portion of our Control Groups would not also have been subsequently activated. Thus, some sensitivity checks are still important to

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<sup>31</sup> 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.

<sup>32</sup> On the grounds that the chi-square statistic of the probit estimated within the matched sample was insignificant.

<sup>33</sup> For model 1, the pseudo  $R^2$  fell from 0.15 to 0.007 under NN and 0.002 under the Kernel. For model 2, the pseudo  $R^2$  fell from 0.067 to 0.018 under NN and 0.003 under the Kernel. For model 3, the pseudo  $R^2$  fell from 0.30 to 0.02 under NN and 0.01 under the Kernel.

ensure that this was the case. As we raise the weekly threshold from 20 to 25 and 30 weeks, we find that the proportion of interviews taking place beyond these points fell to 13 and 6 per cent respectively. Therefore, even ignoring the findings of the validation process, by setting thresholds of 25 and 30 weeks we can rule out the possibility that any substantial proportion of our control groups would have been activated subsequently. The re-estimated models that take this issue into account are presented in Tables 4 and 5 respectively. While the marginal effects fall somewhat, the treatment effect still has a substantial negative impact both with respect to the overall sample and Control Group I. We find no substantial impact relative to Control Group II when the duration limit is extended to 25 weeks; however, based on the probit model there is a marginally positive impact when the time period is increased to 30 weeks. The most likely explanation is that the probit effect is being predominantly driven by a rising concentration of previously long-term unemployed individuals within Control Group II as the duration level rises, a factor which is then eliminated when matching is undertaken.

#### <Table 4>

#### <Table 5>

A further possible risk factor relates to the time-frame of the evaluation. Specifically, it may be 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 (Tables 6 and 7).<sup>34</sup> The overall pattern of results remains unchanged<sup>35</sup>; however, some movement is observed with respect to the magnitude of the negative 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 in the 15 month model before returning close to its original levels in the 18-month model. 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 magnitude of the treatment would be expected. Furthermore, when compared to individuals who

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<sup>34</sup> 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.

<sup>35</sup> 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.

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), current participants fared no better. This is of particular concern as, irrespective of the impact of the activation interview, the Treatment Group would have had FÁS placement services available to them over the entire course of this study period which, if effective, should have generated a positive effect at some point over the 18 month observation period.

<Table 6>

<Table 7>

### *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.<sup>36</sup> 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 become statistically unreliable 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 25 per cent (Table 8). The NN treatment estimate for Control Group I would require the presence of negative selection bias that increased the likelihood of treatment by 75 per cent before our estimate becomes questionable (Table 9). 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, particularly with respect to our key results for Control Group I.

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<sup>36</sup> 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 8>

<Table 9>

## 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 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 (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 7 per cent less likely to have exited to employment prior to 12 months duration on the Live Register. Disaggregating the control groups, we found that the general effect was driven by a 16 per cent disadvantage relative to Control Group I, while no difference was found in the exit rates of the Treatment Group and Control Group II. The data suggests that the referral letter had the standard effect of accelerating exits from the Live Register, which served to substantially reduce the damaging impacts of the activation process. It was found that the negative impact of intervention effectively doubled when the influence of the referral letter was removed from the data. We conclude from the analysis that participants attending an activation interview learned that their prior fears with respect to the extent of job search monitoring and sanctions were unfounded and consequently lowered their job search activity levels relative to the Control Group that had not experienced any 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 of the analysis held when we relaxed key assumptions underpinning the models and 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
<b><i>Original Live Register Population:</i></b>	60,189
Excluding non relevant claims, non awarded claims, duplicates and incomplete questionnaires	27,328
<b><i>Live Register Leavers' Sample Adjustments:</i></b>	
1. Non-labour market leavers eliminated from sample	2,011
2. Unknown reason for closure cases eliminated from sample	1,954
3. Individuals in receipt of training or referred to training	1491
<b><i>Initial NEAP Sample:</i></b>	21,872
1. Old FÁS clients	7,654
Of which historic interventions were light or no interventions	4,399
2. New FÁS clients	14,218
Of which - Interventions (Treatment Group)	7,368
- No interventions	6,850
<b><i>NEAP Sample Including Old Clients With Historic Light or No Interventions:</i></b>	18,617
1. Exclude individuals with interventions after week 45 from Treatment Group	828
2. Exclude re-entrants to Live Register from sample	4,580
3. Exclude un-recorded closures	333
4. Exclude those in employment at time of survey	628
<b><i>Final NEAP Sample:</i></b>	12,248
Of which	
1. Treatment Group	4,948
2. Control Group I: New clients (untreated)	4,623
3. Control Group II: Old clients with historic light or no interventions	2,677
<b><i>Restricting the Analysis to Observations after 20 Weeks Duration on the Live Register:</i></b>	7,468
1. Treatment Group	4,008
2. Control Group I: New clients (untreated)	1,450
3. Control Group II: Old clients with historic light or no interventions	2,010

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

	Model 1	Model 2	Model 3
	Both Control	Control Group	Control
	Groups	I	Group II
<b>NEAP Intervention:</b>			
FÁS Referral plus Interview	-0.07*** (0.013)	-0.16*** (0.017)	0.02 (0.001)
<b>Personal and Family Characteristics:</b>			
Male	0.06*** (0.014)	0.08*** (0.016)	0.06*** (0.015)
<i>Age Reference Category: Aged 18-24</i>			
Age 25-34	-0.06*** (0.018)	-0.02 (0.023)	-0.04** (0.020)
Age 35-44	-0.11*** (0.020)	-0.09*** (0.026)	-0.10*** (0.022)
Age 45-54	-0.10*** (0.023)	-0.08*** (0.029)	-0.08*** (0.025)
Age 55+ Years	-0.22*** (0.022)	-0.26*** (0.026)	-0.18*** (0.025)
<i>Health Reference Category: Bad/Very Bad Health</i>			
Very Good Health	0.15** (0.065)	0.13 (0.082)	0.11 (0.070)
Good Health	0.11* (0.067)	0.09 (0.084)	0.08 (0.073)
Fair Health	0.04 (0.071)	0.04 (0.089)	0.01 (0.076)
<i>Marital Status Reference Category: Single</i>			
Married	-0.03 (0.021)	-0.04* (0.025)	-0.02 (0.023)
Cohabits	-0.02 (0.030)	-0.05 (0.035)	-0.02 (0.032)
Separated/Divorced	-0.06** (0.030)	-0.07** (0.037)	-0.06* (0.032)
Widowed	-0.00 (0.075)	-0.03 (0.083)	-0.04 (0.084)
Children	-0.04*** (0.010)	-0.04*** (0.012)	-0.04*** (0.011)

Table 2: continued

	Model 1	Model 2	Model 3
	Both Control	Control Group	Control
	Groups	I	Group II
<i>Spousal Earnings Reference Category: None</i>			
Spouse Earnings €250	0.14*** (0.036)	0.13*** (0.041)	0.16*** (0.040)
Spouse Earnings €251-€350	-0.01 (0.090)	0.02 (0.099)	0.00 (0.094)
Spouse Earnings €351 and Above	-0.05** (0.023)	-0.06** (0.027)	-0.05* (0.025)
<b>Human Capital Characteristics:</b>			
<i>Education Reference Category: Primary or Less</i>			
Junior Certificate	-0.00 (0.021)	0.02 (0.026)	0.00 (0.022)
Leaving Certificate	0.05** (0.021)	0.10*** (0.026)	0.04* (0.023)
Third-level	0.15*** (0.023)	0.17*** (0.028)	0.14*** (0.025)
Apprenticeship	0.03 (0.018)	0.01 (0.022)	0.03* (0.020)
Literacy/Numeracy Problems	-0.06*** (0.024)	-0.06** (0.030)	-0.06** (0.025)
English Proficiency	0.01 (0.035)	0.01 (0.040)	0.02 (0.038)
<b>Employment/Unemployment/Benefit History:</b>			
<i>Employment History Reference Category: Never</i>			
Employed in Last Month	0.08** (0.040)	0.09* (0.049)	0.10** (0.043)
Employed in Last Year	0.06 (0.042)	0.07 (0.051)	0.08* (0.046)
Employed in Last 5 Years	-0.02 (0.042)	-0.01 (0.053)	0.01 (0.047)
Employed Over 6 Years Ago	-0.05 (0.052)	-0.08 (0.067)	-0.02 (0.057)

Table 2: continued

	Model 1	Model 2	Model 3
	Both Control	Control	Control
	Groups	Group I	Group II
<i>Job Duration Reference Category:</i>			
<i>Never Employed</i>			
Job Duration Less than Month	0.09*	0.12**	0.05
	(0.046)	(0.057)	(0.049)
Job Duration 1-6 Months	0.11***	0.16***	0.10**
	(0.038)	(0.048)	(0.041)
Job Duration 6-12 Months	0.09**	0.13***	0.06
	(0.040)	(0.051)	(0.043)
Job Duration 1-2 Years	0.07*	0.14***	0.04
	(0.041)	(0.051)	(0.043)
Job Duration 2+ Years	-0.00	0.02	-0.03
	(0.038)	(0.048)	(0.040)
Would Move for a Job	0.05***	0.05***	0.05***
	(0.013)	(0.016)	(0.014)
<i>Social Welfare Payment Type Reference Category:</i>			
<i>Jobseeker's Benefit</i>			
Jobseeker's Allowance	-0.18***	-0.18***	-0.17***
	(0.015)	(0.018)	(0.016)
Signing on the Live Register for 12 Months Plus	-0.19***	-0.18***	-0.13***
	(0.017)	(0.034)	(0.019)
CE Scheme Previous 5 Years	-0.04	0.03	0.03
	(0.045)	(0.068)	(0.047)
On CE Scheme for 12 Months Plus	-0.14***	-0.22***	-0.12**
	(0.046)	(0.063)	(0.048)
<b>Geographic Location Information:</b>			
<i>Location Reference Category: Rural</i>			
Village	-0.02	-0.01	-0.04
	(0.021)	(0.025)	(0.023)
Town	-0.02	-0.01	-0.00
	(0.021)	(0.024)	(0.022)
Large Town/City	-0.02	-0.01	-0.02
	(0.021)	(0.025)	(0.022)



**Table 2: continued**

	<b>Model 1</b>	<b>Model 2</b>	<b>Model 3</b>
	<b>Both Control</b>	<b>Control</b>	<b>Control</b>
	<b>Groups</b>	<b>Group I</b>	<b>Group II</b>
Own Transport	0.08*** (0.014)	0.08*** (0.018)	0.07*** (0.015)
Public Transport	-0.00 (0.018)	-0.02 (0.021)	-0.00 (0.019)
Observations	7,441	5,444	6,005

**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**

	<b>FÁS Interview</b>	<b>FÁS Interview</b>
	<b>(Nearest Neighbour)</b>	<b>(Kernel)</b>
Control Group I & II (Model 1)	-0.074 (0.018)***	-0.076 (0.013)***
Control Group I (Model 2)	-0.149 (0.022)***	-0.151 (0.017)***
Control Group II (Model 3)	0.007 (0.028)	0.006 (0.020)

**Table 4: Probit & PSM Estimates of Treatment Effect: 25 Week Threshold**

	<b>FÁS Interview</b>	<b>FÁS Interview</b>	<b>FÁS Interview</b>
	<b>(Probit)</b>	<b>(Nearest Neighbour)</b>	<b>(Kernel)</b>
Control Group I/ II (Model 1)	-0.041 (0.013)***	-0.043 (0.019)***	-0.046 (0.014)***
Control Group I (Model 2)	-0.104 (0.018)***	-0.115 (0.025)***	-0.106 (0.019)***
Control Group II (Model 3)	-0.018 (0.016)	-0.020 (0.029)	0.008 (0.021)

**Table 5: Probit & PSM Estimates of Treatment Effect: 30 Week Threshold**

	<b>FÁS Interview</b>	<b>FÁS Interview</b>	<b>FÁS Interview</b>
	<b>(Probit)</b>	<b>(Nearest Neighbour)</b>	<b>(Kernel)</b>
Control Group I/ II (Model 1)	-0.020 (0.013)*	-0.027 (0.019)	-0.027 (0.013)**
Control Group I (Model 2)	-0.077 (0.018)***	-0.062 (0.026)***	-0.080 (0.020)***
Control Group II (Model 3)	0.027 (0.016)	0.005 (0.029)	0.020 (0.020)

**Table 6: Estimates of Treatment Effect: 15 Month Model**

	FÁS Interview (Probit)	FÁS Interview (Nearest Neighbour)	FÁS Interview (Kernel)
Control Group I/ II (Model 1)	-0.056 (0.014)***	-0.064 (0.019)***	-0.060 (0.014)***
Control Group I (Model 2)	-0.142 (0.018)***	-0.118 (0.023)***	-0.137 (0.018)***
Control Group II (Model 3)	0.041 (0.018)**	0.035 (0.020)	0.023 (0.022)

**Table 7: Estimates of Treatment Effect: 18 Month Model**

	FÁS Interview (Probit)	FÁS Interview (Nearest Neighbour)	FÁS Interview (Kernel)
Control Group I/ II (Model 1)	-0.075 (0.014)***	-0.085 (0.018)***	-0.071 (0.014)***
Control Group I (Model 2)	-0.174 (0.017)***	-0.159 (0.021)***	-0.153 (0.017)***
Control Group II (Model 3)	0.035 (0.018)*	0.024 (0.032)	0.026 (0.022)

**Table 8: Mantel-Haenszel Bounds for NN Estimate for Control Groups I & II**

Gamma	Q_mh+	Q_mh	p_mh+	p_mh-
1	4.53	4.53	0.00	0.00
1.05	5.36	3.70	0.00	0.00
1.1	6.15	2.92	0.00	0.00
1.15	6.90	2.16	0.00	0.02
1.2	7.63	1.45	0.00	0.07
1.25	8.33	0.76	0.00	0.23
1.3	9.00	0.09	0.00	0.46
1.35	9.65	0.49	0.00	0.31
1.4	10.27	1.10	0.00	0.14
1.45	10.88	1.69	0.00	0.05
1.5	11.46	2.27	0.00	0.01
1.55	12.03	2.82	0.00	0.00
1.6	12.58	3.36	0.00	0.00
1.65	13.12	3.88	0.00	0.00
1.7	13.64	4.38	0.00	0.00
1.75	14.14	4.88	0.00	0.00
1.8	14.64	5.35	0.00	0.00
1.85	15.12	5.82	0.00	0.00
1.9	15.59	6.27	0.00	0.00
1.95	16.05	6.71	0.00	0.00
2	16.50	7.15	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 9: Mantel-Haenszel Bounds for NN Estimate for Control Group I**

Gamma	Q_mh+	Q_mh	p_mh+	p_mh-
1	8.96	8.96	0.00	0.00
1.05	9.68	8.25	0.00	0.00
1.1	10.36	7.57	0.00	0.00
1.15	11.02	6.92	0.00	0.00
1.2	11.66	6.30	0.00	0.00
1.25	12.27	5.71	0.00	0.00
1.3	12.86	5.14	0.00	0.00
1.35	13.43	4.60	0.00	0.00
1.4	13.98	4.07	0.00	0.00
1.45	14.51	3.57	0.00	0.00
1.5	15.03	3.08	0.00	0.00
1.55	15.54	2.61	0.00	0.00
1.6	16.03	2.15	0.00	0.02
1.65	16.50	1.71	0.00	0.04
1.7	16.97	1.28	0.00	0.10
1.75	17.42	0.87	0.00	0.19
1.8	17.86	0.46	0.00	0.32
1.85	18.30	0.07	0.00	0.47
1.9	18.72	0.24	0.00	0.40
1.95	19.13	0.61	0.00	0.27
2	19.54	0.98	0.00	0.16

**Note:** Gamma : odds of differential assignment due to unobserved factors

Q\_mh+ : Mantel-Haenszel statistic (assumption: overestimation of treatment effect)

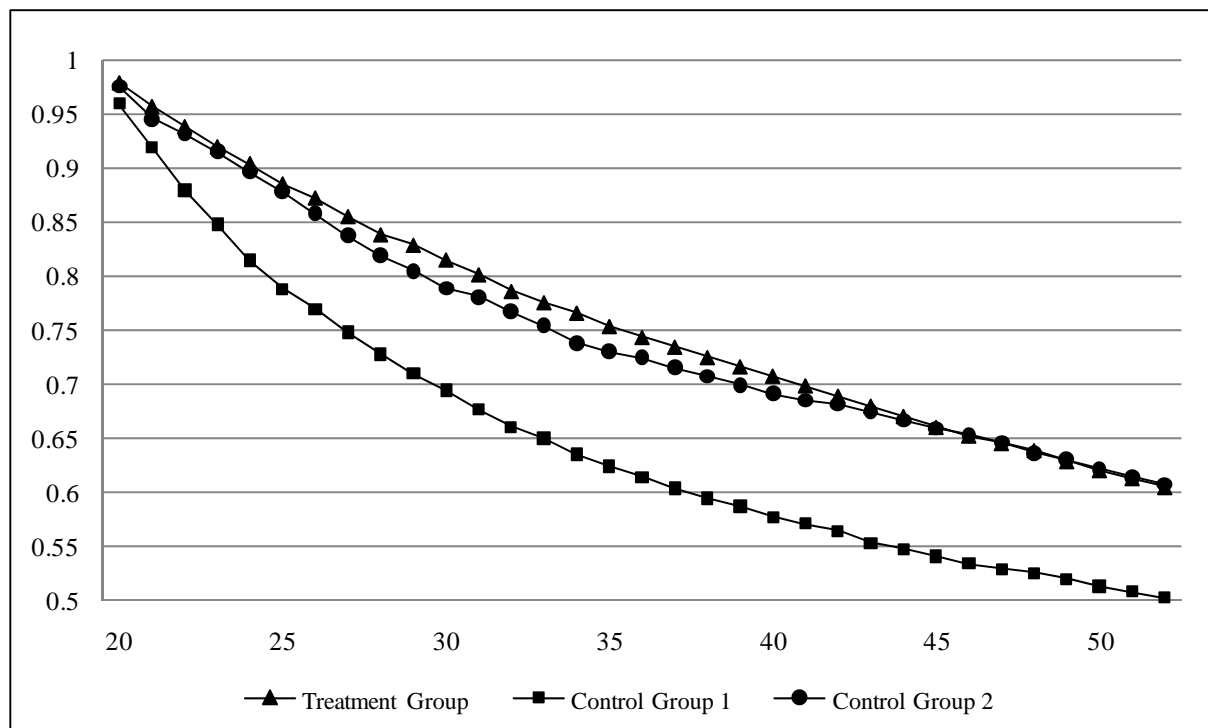
Q\_mh- : Mantel-Haenszel statistic (assumption: underestimation of treatment effect)

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 Probits from PSM Estimations**

	Model 1	Model 2	Model 3
	Both Control	Control Group	Control
	Groups	I	Group II
<b>Personal and Family Characteristics:</b>			
Male	-0.01 (0.014)	0.02 (0.014)	-0.05*** (0.016)
<i>Age Reference Category: Aged 18-24</i>			
Age 25-34	-0.18*** (0.019)	-0.00 (0.019)	-0.33*** (0.023)
Age 35-44	-0.08*** (0.023)	0.09*** (0.020)	-0.26*** (0.027)
Age 45-54	-0.08*** (0.026)	0.06*** (0.023)	-0.24*** (0.031)
Age 55+ Years	-0.15*** (0.029)	-0.13*** (0.031)	-0.07* (0.039)
<i>Health Reference Category: Bad/Very Bad Health</i>			
Very Good Health	0.13** (0.058)	0.07 (0.060)	0.15** (0.063)
Good Health	0.12** (0.058)	0.07 (0.056)	0.12** (0.060)
Fair Health	0.13** (0.058)	0.06 (0.054)	0.13** (0.052)
<i>Marital Status Reference Category: Single</i>			
Married	0.03 (0.022)	0.00 (0.021)	0.04* (0.024)
Cohabits	0.04 (0.031)	-0.02 (0.031)	0.08*** (0.030)
Separated/Divorced	0.05* (0.031)	0.01 (0.031)	0.08*** (0.029)
Widowed	0.04 (0.075)	0.00 (0.068)	0.09 (0.081)
Children	0.01 (0.009)	-0.00 (0.009)	0.01 (0.010)

**Table A1: continued**

	<b>Model 1</b>	<b>Model 2</b>	<b>Model 3</b>
	<b>Both Control</b>	<b>Control Group</b>	<b>Control</b>
	<b>Groups</b>	<b>I</b>	<b>Group II</b>
<i>Spousal Earnings Reference Category: None</i>			
Spouse Earnings €250	0.01 (0.036)	0.03 (0.032)	0.01 (0.041)
Spouse Earnings €251-€350	0.24*** (0.081)	0.15** (0.058)	0.20*** (0.065)
Spouse Earnings €351 and Above	0.05** (0.025)	0.06*** (0.021)	0.00 (0.029)
<b>Human Capital Characteristics:</b>			
<i>Education Reference Category: Primary or Less</i>			
Junior Certificate	-0.01 (0.021)	-0.01 (0.021)	-0.00 (0.022)
Leaving Certificate	0.02 (0.021)	-0.01 (0.021)	0.05** (0.022)
Third-level	0.07*** (0.023)	0.00 (0.023)	0.12*** (0.022)
Apprenticeship	-0.01 (0.019)	-0.01 (0.018)	-0.01 (0.020)
Literacy/Numeracy Problems	0.01 (0.025)	0.01 (0.024)	0.01 (0.026)
English Proficiency	0.13*** (0.033)	0.02 (0.032)	0.17*** (0.027)
<b>Employment/Unemployment/Benefit History:</b>			
<i>Employment History Reference Category: Never</i>			
Employed in Last Month	-0.07 (0.040)	0.07 (0.041)	-0.18*** (0.043)
Employed in Last Year	-0.03 (0.041)	0.07* (0.038)	-0.14*** (0.050)
Employed in Last 5 Years	-0.07 (0.042)	0.06 (0.039)	-0.21*** (0.052)
Employed Over 6 Years Ago	-0.06 (0.050)	0.03 (0.048)	-0.19*** (0.062)

**Table A1: continued**

	<b>Model 1</b>	<b>Model 2</b>	<b>Model 3</b>
	<b>Both Control</b>	<b>Control</b>	<b>Control</b>
	<b>Groups</b>	<b>Group I</b>	<b>Group II</b>
<i>Job Duration Reference Category:</i>			
<i>Never Employed</i>			
Job Duration Less than Month	-0.08* (0.044)	-0.10* (0.053)	-0.07 (0.050)
Job Duration 1-6 Months	-0.11*** (0.037)	-0.12*** (0.044)	-0.10** (0.041)
Job Duration 6-12 Months	-0.07* (0.039)	-0.08* (0.046)	-0.07* (0.043)
Job Duration 1-2 Years	-0.01 (0.039)	-0.06 (0.045)	0.00 (0.040)
Job Duration 2+ Years	0.08** (0.036)	-0.05 (0.040)	0.17*** (0.034)
Would Move for a Job	0.01 (0.014)	0.00 (0.014)	0.02 (0.015)
<i>Social Welfare Payment Type Reference Category:</i>			
<i>Jobseeker's Benefit</i>			
Jobseeker's Allowance	-0.07*** (0.016)	-0.03* (0.016)	-0.09*** (0.017)
Signing on the Live Register for 12 Months Plus	-0.43*** (0.015)	-0.10*** (0.034)	-0.57*** (0.017)
CE Scheme Previous 5 Years	-0.12** (0.048)	0.00 (0.055)	-0.19*** (0.055)
On CE Scheme for 12 Months Plus	-0.01 (0.057)	0.05 (0.059)	-0.02 (0.057)
<b>Geographic Location Information:</b>			
<i>Location Reference Category: Rural</i>			
Village	-0.01 (0.022)	0.01 (0.021)	-0.02 (0.025)
Town	0.00 (0.022)	0.02 (0.020)	-0.02 (0.024)
Large Town/City	-0.01 (0.022)	0.02 (0.021)	-0.04 (0.024)

**Table A1: continued**

	<b>Model 1</b>	<b>Model 2</b>	<b>Model 3</b>
	<b>Both Control</b>	<b>Control</b>	<b>Control</b>
	<b>Groups</b>	<b>Group I</b>	<b>Group II</b>
Own Transport	-0.04*** (0.015)	-0.06*** (0.015)	-0.01 (0.016)
Public Transport	0.01 (0.018)	-0.00 (0.018)	0.02 (0.021)
Observations	7,441	5,444	6,005

**Note:** Standard errors in parentheses.

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



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