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ORIGINAL ARTICLE

A new career in a new town. Job search methods and regional mobility of unemployed workers

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Abstract

Labour mobility is critical for adjusting imbalance between local labour markets. Yet, labour markets appear still very localized. Existing studies on job search report that the choice of search methods influences job outcomes, with social contacts accounting for a substantial fraction of job matches. Whether search methods are conducive to local or national jobs has not been examined yet. This paper establishes a link between job search and regional mobility, investigating the impact of search methods on unemployment exits within and across local labour markets. The effect of search methods is estimated by a Propensity Score Matching approach, using data from the British Household Panel Survey. Results show that only direct approach to employers enhances the job hazard with regional move. Conversely, social contacts and advertisements are found to increase the hazard to local employment, although the effect of social contacts wears off as the unemployment spell prolongs. No impact is found by Employment Agencies on either exit. These findings suggest that the widespread use of social contacts, while enhancing job matches in the local labour market, might contribute to restrict labour mobility. Therefore, they bear support to policies promoting diffusion and efficacy of alternative methods, particularly when the target is long-term unemployment. Results also point out the opportunity of reforms of the job search assistance and placement service offered by Employment Agencies, taking these limitations into account.

Keywords Local labour markets · Regional mobility · Job search methods · Unemployment duration · Social networks

JEL Classification J61 · J64 · R23

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1 Introduction

Regional labour mobility can compensate for disparities between regional labour markets (Blanchard and Katz 1992). This adjustment mechanism is hindered in European countries (Decressin and Fatas 1995; Jimeno and Bentolila 1998; Puhani 2001) relative to the US (Blanchard and Katz 1992) by institutional characteristics that limit within-country mobility, notably rigidities in the labour market (Bertola 1999; Hassler et al. 2005) and in the housing market (Hughes and McCormick 1987; Bover et al. 1989; Oswald 1999; Partridge and Rickman 1997; Nickell and Layard 1999). Mobility across EU NUTS2 regions is as low as 1%, that is half as much as across the United States (Bonin et al. 2008). Acknowledging the potential of labour mobility as an equilibrating factor, the European Commission has given strong emphasis to mobility in its employment strategies since the Lisbon strategy (European Commission 2001), and more recently in Horizon 2020 (European Commission 2010).

Mobility across labour markets can be limited if information about job opportunities is localized, despite suitable jobs can be available outside the local market. A large body of literature has documented the importance of social relationships in alleviating frictions in the job matching process (Ioannides and Loury 2004; Topa 2011). Evidence from survey data consistently report that a large fraction of job matches are created through friends and relatives, and hence this channel is considered very effective for finding job. However, social contacts have been also associated to some "unintended consequences", such as segmentation of the labour market or growing inequality in employment outcomes, which may raise equity concerns (Topa 2019). Furthermore, the widespread use of social contacts may hinder regional mobility, since social contacts can be localized and used in place of alternative methods, potentially leading to national jobs or to better job matches (Bentolila et al. 2010). This insight is corroborated by evidence that personal contacts are less important in the US (Blau and Robins 1990) and in the UK (Frijters et al. 2005; Battu et al. 2011; Bachmann and Baumgarten 2013), where regional mobility is higher than the EU average. In a UK-specific study, Manning and Petrongolo (2017) find that the likelihood of accepting jobs in another area is very low even for short distances, suggesting that labour markets are quite "local" . The aforementioned evidence poses concerns about the geographic enclosure of labour markets and raises interest for a joint investigation of the job search process and mobility.

Theoretical models of job search show that unemployed people redistribute search effort from national to local search to avoid costs of relocation (Dohmen 2005; Munch et al. 2006; Coulson and Fisher 2009; Rouwendal and Nijkamp 2010; Morescalchi 2016). Models incorporating the choice of search methods suggest that individuals decide on the amount of effort to allocate to each method by taking into account associated costs and benefits (Holzer 1988; Weber and Mahringer 2008).

Several empirical studies have investigated the returns to search methods in terms of the job finding rate, wages, job tenure or others (Holzer 1988; Osberg 1993; Addison and Portugal 2002; Weber and Mahringer 2008; Bentolila et al. 2010; Pellizzari 2010; Caliendo et al. 2011; Bachmann and Baumgarten 2013). However no studies have taken into consideration the geographic dimension of search methods so far. The present paper fills this gap by investigating the effect of search methods on regional mobility of unemployed workers.

Data from the British Household Panel Survey (BHPS) for the period 1996–2008 are used to estimate an unemployment duration competing-risks model. Using Travel To Work Areas (TTWA) as definition of local labour markets, exits to employment are decomposed between exits within and across local labour markets.

A Propensity Score Matching approach is implemented to estimate the effect of search methods on the competing hazards, where each of the following search methods is considered in turn as a treatment: (i) direct approach to employers (DAE); (ii) advertisements (ADS); (iii) employment agency (EA); (iv) social contacts (SOCNET). The effect of treatments is estimated by a semi-parametric Cox proportional hazard model on the matched sample (Austin and Fine 2019).

By establishing a link between search methods and regional mobility of unemployed workers, this paper contributes to existing knowledge about the labour market functioning in two key ways. First, it offers a new perspective to assess efficiency in the selection of search methods. In this context efficiency requires that less mobile workers allocate relatively more (respectively less) effort to methods that are conducive to local (respectively non-local) employment. This line of research is relevant to the literature on housing tenure and labour market. Theoretical models suggest that homeowners redistribute search effort between non-local to local labour market in order to avoid costly relocation, with resulting overall negative effect on search intensity and job finding rate (Morescalchi 2016). However, while (outright) homeowners are consistently found to use fewer search methods (Morescalchi 2016), most microeconometric studies find that homeowners have no longer, or even shorter, unemployment spells than renters (Munch et al. 2006; Battu et al. 2008; Morescalchi 2016). Since homeowners use more often search methods associated to shorter unemployment spells, it is possible that they are more efficient in the selection of search methods (Morescalchi 2016).

By investigating the effectiveness of search methods for local vis-à-vis non-local employment, together with search methods choice, the present analysis can inform whether homeowners search efficiently also in a regional perspective.

Second, the present paper offers insights for policy interventions in the job search process aiming to ease barriers to mobility. Moreover, it evaluates whether employment agencies play a significant role in the matching process and in particular whether they can be effective in enhancing labour mobility. This analysis appears of particular interest since the European Commission attributes high relevance to labour mobility policies and envisions a more comprehensive role for public employment agencies in the implementation of the European employment strategy (European Commission (2010), p. 7).

This paper has the following structure. Section 2 describes related literature. Section 3 describes the data. Section 4 describes the econometric methodology employed for estimating the competing-risks unemployment duration model, and Section 5 provides the results. Section 6 concludes. Finally, the Appendix reports additional information and evidence.

2 Related literature

The present paper is related to two strands of literature. The first one investigates the impact of job search methods on labour market outcomes of unemployed people. The second one focuses on the impact of mobility constraints on job search and finding in local vis-à-vis non-local labour markets. The tho strands are discussed in the following sections.

2.1 Search methods and labour market outcomes

Theoretical models of search method choice suggest that individuals allocate effort among methods taking into account their specific costs, in terms of time and money, and benefits, in terms of quantity and quality of offers (Holzer 1988; Weber and Mahringer 2008).

Using data from the EU-LFS for 2006–2008, Bachmann and Baumgarten (2013) report the following use shares by unemployed people: 65% public employment office, 21% private employment agency, 52% direct applications, 61% personal contacts, 42% inserting/answering advertisements, 68% studying advertisements, and 17% test interview and examinations. These shares exhibit some variation across EU countries, with the UK having remarkably below-average share for friends and relatives (50%) and remarkably above-average share for inserting/answering advertisements (59%) and for studying advertisements (82%).

The effects of search methods on labour market outcomes of unemployed people have been investigated empirically quite extensively. Table 1 summarizes results for a selection of survey-based studies, focusing on any of the five methods available in the present data, namely EA (public or private employment agency), SOCNET (social contacts), DAE (direct approach to employers), ADS (studied/replied to advertisements), SEMP (steps to start business). A distinction between Public Employment Service (PES) and Private Employment Agency (PEA) is also included whenever possible.

The choice of search methods can be measured in three ways. In case of unemployment, the worker is asked to report the methods currently used, either selecting multiple methods or only the main one. In case of employment, he is asked to report retrospectively the method leading to the present job; this is labeled in Table 1 as the successful method. The effect of each method can be compared against non-use of the focal method, or against use of a baseline method.

Table 1 shows that the interest has been primarily on the effect on the probability to find a job, on wage, and to a lesser extent on job tenure and others. As concerns the probability to find job, DAE stands out as the most effective method. Indeed it is consistently found to have positive effects in all eight studies considering exits to job. However, the effect of DAE on wage is mixed: it is positive in only one case (Green 2012), non-significant in three studies (Böheim and Taylor 2001; Weber and Mahringer 2008; Longhi and Taylor 2011) and even negative in one (Addison and Portugal 2002).

Evidence on the effect of ADS on the probability to find job is available for seven studies in Table 1. In three cases, ADS is found to have no effect on the probability to

Table 1 Survey evidence on the effect of job search methods of unemployed people

Article	Outcome	Sample	Country	Data	Period	model	Choice	PES	PEA	EA	SOCNET	ADS	DAE	SEMP
Holzer (1988)	job offer	unem. m.	US	NLSY	81	probit	multiple	0	n/a	n/a	+	+	0	n/a
Osberg (1993)	job	st unem. f.	CA	LFS	81, 83, 86	logit	multiple	0, 0, 0	0, 0, 0	n/a	0, 0, 0	n/a	0, +, 0	n/a
	job	lt unem. f.	CA	LFS	81, 83, 86	logit	multiple	0, 0, +	0, 0, n/a	n/a	0, 0, 0	n/a	0, 0, +	n/a
	job	st unem. m.	CA	LFS	81, 83, 86	logit	multiple	0, 0, 0	0, +, 0	n/a	–, +, 0	n/a	0, +, 0	n/a
	job	lt unem. m.	CA	LFS	81, 83, 86	logit	multiple	–, +, 0	n/a, 0, 0	n/a	0, 0, 0	n/a	+, +, +	n/a
Gregg and Wadsworth (1996)	job	unem. m.	UK	LFS	92	probit	multiple	+	+	n/a	+	0	+	n/a
	job	unem. f.	UK	LFS	92	probit	multiple	+	+	n/a	0	0	+	n/a
Böheim and Taylor (2001)	job	unem. m.	UK	BHPS	96–99	probit	multiple	n/a	n/a	0	0	0	+	0
	wage	empl. m.	UK	BHPS	96–99	selection	multiple	n/a	n/a	0	0	+	0	0
Addison and Portugal (2002)	job	unem.	PT	LFS	92–96	dur. model	multiple	0	n/a	n/a	0	0	+	+
	wage	empl.	PT	LFS	97	group reg.	success.	–, b.=oth.	n/a	n/a	–, b.=oth.	0, b.=oth.	–, b.=oth.	n/a
	job dur.	empl.	PT	LFS	94–97	dur. model	success.	+, b.=oth.	n/a	n/a	0, b.=oth.	0, b.=oth.	–, b.=oth.	n/a
Frijters et al. (2005)	job	unem.	PT	LFS	97–02	dur. model	main	b.	n/a	n/a	+, b.=PES	+, b.=PES	+, b.=PES	n/a
Weber and Mahringer (2008)	wage	empl.	AT	survey	97	OLS	success.	b.	n/a	n/a	0, b.=PES	0, b.=PES	0, b.=PES	n/a
	job dur.	empl.	AT	survey	97	OLS	success.	b.	n/a	n/a	0, b.=PES	0, b.=PES	+, b.=PES	n/a
Pellizzari (2010)	wage	empl.	EU	ECHP	94–99	ind. fe	success.	n/a	n/a	n/a	+ (3 ctry)	n/a	n/a	n/a
	wage	empl.	EU	ECHP	94–99	ind. fe	success.	n/a	n/a	n/a	0 (6 ctry)	n/a	n/a	n/a
	wage	empl.	EU	ECHP	94–99	ind. fe	success.	n/a	n/a	n/a	– (5 ctry)	n/a	n/a	n/a
	wage	empl.	US	NLSY	96–00	ind. fe	success.	n/a	n/a	n/a	0	n/a	n/a	n/a
Bentolila et al. (2010)	job	empl.	US	MCSUI	92–94	OLS	success.	n/a	n/a	n/a	+	n/a	n/a	n/a
	wage	empl.	US	MCSUI	92–94	OLS	success.	n/a	n/a	n/a	–	n/a	n/a	n/a
	job	empl. 1st job	EU	ECHP	95–01	OLS	success.	n/a	n/a	n/a	+	n/a	n/a	n/a
	wage	empl. 1st job	EU	ECHP	95–01	OLS	success.	n/a	n/a	n/a	–	n/a	n/a	n/a

Table 1 (continued)

Article	Outcome	Sample	Country	Data	Period	model	Choice	PES	PEA	EA	SOCNET	ADS	DAE	SEMP
Battu et al. (2011)	job	m. in wa	UK	LFS	98–01	logit	main	n/a	n/a	b.	0, b.=EA	0, b.=EA	+, b.=EA	n/a
	seniority	m. in wa	UK	LFS	98–01	logit	main	n/a	n/a	b.	0, b.=EA	+, b.=EA	+, b.=EA	n/a
Longhi and Taylor (2011)	job	unem. m.	UK	LFS	92–09	probit	main	b.	n/a	n/a	+, b.=PES	+, b.=PES	+, b.=PES	n/a
	job	unem. f.	UK	LFS	92–09	probit	main	b.	n/a	n/a	+, b.=PES	+, b.=PES	+, b.=PES	n/a
	wage	empl. m.	UK	LFS	92–09	OLS	main	b.	n/a	n/a	0, b.=PES	0, b.=PES	0, b.=PES	n/a
	wage	empl. f.	UK	LFS	92–09	OLS	main	b.	n/a	n/a	0, b.=PES	0, b.=PES	0, b.=PES	n/a
Green (2012)	wage	empl.	AU	SEUP	94–97	selection	success.	b.	n/a	n/a	+, b.=PES	0, b.=PES	+, b.=PES	n/a
	fires	empl.	AU	SEUP	94–97	dur. model	success.	b.	n/a	n/a	+, b.=PES	–, b.=PES	0, b.=PES	n/a
	quits	empl.	AU	SEUP	94–97	dur. model	success.	b.	n/a	n/a	+, b.=PES	+, b.=PES	0, b.=PES	n/a
Morescalchi (2016)	job	m. hoh	UK	LFS	99–09	dur. model	main	b.	+, b.=PES	n/a	0, b.=PES	+, b.=PES	+, b.=PES	n/a

Notes: Holzer (1988) considers 16–23 yo. Weber and Mahringer (2008) considers Styria region. Pellizzari (2010) finds positive effect for AT, BE, NL, no effect for DK, FR, DE, IE, LU, ES, negative effect for FI, GR (if job characteristics are omitted), IT, PT, UK. Bentolila et al. (2010) consider individuals $<$ 35 yo; the US sample is based on 3 cities (Atlanta, Boston and Los Angeles), while the EU sample comprises 13 countries altogether. Longhi and Taylor (2011) consider as outcomes also temporary contract and working-hours satisfaction. Symbols, abbreviations and acronyms: "+"=significant positive effect; "0"=non-significant effect; "–"=significant negative effect; "n/a"=not available; "PES'=Public Employment Service; "PEA'=Private Employment Agency; "EA'=Employment Agencies (both); "SOCNET"=Friends and relatives; "ADS"=Newspapers and advertisements; "DAE"=Direct Approach to Employers; "SEMP"=taken steps to start own business;"unem."=unemployed; "empl."=employed; "f."=females; "m."=males; "dur."=duration; "st"=short-term; "lt"=long-term; "hoh"=heads of household; "wa"=working age; "yo"=years old; "ind."=individual; "fe"=fixed-effects; "success."=successful; "b."=base; "oth."=other; "reg."=regression; "ctry"=country; "AU"=Australia; "AT"=Austria; "BE"=Belgium; "CA"=Canada; "NL"=Netherlands; "PT"=Portugal; "LFS"=Labour Force Survey; "MCSUI"=Multi-City Study of Urban Inequality; "NLSY"=National Longitudinal Survey of Youth; "BHPS"=British Household Panel Survey; "ECHP"=European Community Household Panel; "SEUP"=Survey of Employment and Unemployment Patterns find job (Gregg and Wadsworth 1996; Böheim and Taylor 2001; Addison and Portugal 2002). In the other four cases ADS is compared with employment agencies, and it is found to be more effective than PES in three cases (Frijters et al. 2005; Longhi and Taylor 2011; Morescalchi 2016), but no more effective than EA in one case (Battu et al. 2011). Moreover, Holzer (1988) finds that ADS increases the probability to receive a job offer.

Table 1 shows also that the interest of this literature has focused primarily on the effects of SOCNET and PES, that are considered as opposite examples of a very informal and very formal method (Van den Berg and van der Klaauw 2006). This section proceeds by reviewing in turn the literature for SOCNET and PES. The effect of SEMP is investigated in only two studies in Table 1 and hence it is not considered relevant in the present review.

The literature on the impact of social networks on labour market outcomes is particularly rich, and it is not restricted to survey evidence. Evidence from survey data consistently report that a large fraction of job matches are created through friends and relatives, and hence this channel is often considered to enhance employment inflows (Ioannides and Loury 2004; Topa 2011). Table 1 shows that among the ten studies considering the job finding or job offer as outcomes, positive effects of SOCNET are roughly as frequent as non-significant effects. Also, in the four UK studies comparing SOCNET with PES or EA, the effect is positive in two cases (Frijters et al. 2005; Longhi and Taylor 2011) and non-significant in other two cases (Battu et al. 2011; Morescalchi 2016). Therefore, although SOCNET is documented to be useful for finding job in some specific cases, it is not always effective, and it does not seem to be as effective as DAE. Indeed, among the nine studies considering SOCNET as well as DAE, six studies provide evidence that DAE is effective while SOCNET is not in at least one case (Osberg 1993; Gregg and Wadsworth 1996; Böheim and Taylor 2001; Addison and Portugal 2002; Battu et al. 2011; Morescalchi 2016), and only one study finds that SOCNET is more effective (Holzer 1988).

Evidence on the effect of SOCNET on wage is mixed. Table 1 shows that the effect of SOCNET on wage is positive in one study (Green 2012), non-significant in three studies (Böheim and Taylor 2001; Weber and Mahringer 2008; Longhi and Taylor 2011), and negative in two studies (Addison and Portugal 2002; Bentolila et al. 2010). Moreover, using a large dataset of European households (Pellizzari 2010) finds that the wage effect of social contacts has high cross-country variation.

Besides survey evidence, another strand of literature has investigated the impact of social networks in the labour market by explicitly modeling the mechanisms through which information about job opportunities/candidates is transmitted. Two main transmission mechanisms have been pointed out in this literature: referrals of network members by firm's employees (Dustmann et al. 2015; Brown et al. 2016) and interactions among potential employees (Topa 2001; Calvo-Armengol and Jackson 2004; 2007; Galeotti and Merlino 2014). In the first case, employees obtaining jobs through referrals by firm's employees are predicted to earn higher wages and have longer tenure, because their match-specific productivity is less uncertain. In the second case, individuals with larger social networks, and in particular with larger number of employed contacts, are predicted to have higher job finding rates because they can receive information about more job opportunities.[1] Predictions of these models have been tested typically with administrative data and by using various measures of network connections, such as neighbourhood (Bayer et al. 2008; Hellerstein et al. 2011; Hellerstein et al. 2014; Schmutte 2015), ethnicity (Beaman 2012; Dustmann et al. 2015), family (Kramarz and Nordström Skans 2014; Plug et al. 2018), firm (Cingano and Rosolia 2012), military service (Laschever 2009), and friendship connections (Cappellari and Tatsiramos 2015). Evidence from this literature generally reports that network connections enhance the employment probability and job stability, but have mixed effects on wage as well.[2]

Evidence on the effect of PES on the probability to find job, either alone or combined with PEA, is available for eight studies in Table 1. A positive effect is found by Gregg and Wadsworth (1996), and by Osberg (1993) only for long-term unemployed in two samples out of six, while two studies find no effect (Böheim and Taylor 2001; Addison and Portugal 2002), and four studies find a negative effect relative to other methods (Frijters et al. 2005; Battu et al. 2011; Longhi and Taylor 2011; Morescalchi 2016). Moreover, Holzer (1988) finds no effect on the probability to receive a job offer. Therefore, PES seems to be the least effective method in enhancing exits from unemployment. Similarly bad performance is found for wages (Böheim and Taylor 2001; Addison and Portugal 2002; Weber and Mahringer 2008; Longhi and Taylor 2011; Green 2012). PES can be compared to PEA in only three studies, pointing out similar job finding performance in two cases (Osberg 1993; Gregg and Wadsworth 1996), and a relative effectiveness of PEA in one case (Morescalchi 2016).

Although PES is often found to be ineffective for unemployed workers, it may be possible that PES is less effective because used at the last resort (Green 2012), when alternative search channels are not available (Bachmann and Baumgarten 2013) or have been already exhausted (Osberg 1993). Moreover, PES is typically approached by low quality workers, for whom it can be as efficient as other channels (Weber and Mahringer 2008).

Taking stock of the literature reviewed in this section, it appears that unemployed people tend to select search methods somehow efficiently (Holzer 1988; Weber and Mahringer 2008). SOCNET and PES are the two most popular methods and they are also associated to low search costs. Indeed, SOCNET can provide relevant information through informal contacts, while PES can offer a personalized service free of charge. However, while SOCNET is often found to enhance chances to exit unemployment, PES stands out as the least effective method. Relying on PES could be anyway efficient for those individuals who have limited chances to find a job with alternative methods. DAE and ADS can be associated to higher costs of search since they require active steps to gather information on jobs or to get in contact to employers, which can be associated to time as well as pecuniary costs. Therefore, although DAE stands out as the most effective method to find job, it is less used.

[1] This prediction rests on the assumption that, while employed workers share information about job opportunities with unemployed workers in their network, unemployed workers keep the information for themselves.

[2] See Loury (2006), Pellizzari (2010) and Bentolila et al. (2010) for explanations of the mixed results for the wage effect of SOCNET.

The present paper contributes to this literature by examining separately the productivity of search methods for local and non-local jobs. This regional separation offers a new perspective to assess efficiency in the selection of search methods, which is relevant to the literature on job search and regional mobility introduced in the following section.

2.2 Search and regional mobility

Regional mobility is typically incorporated in theoretical models of job search by allowing for two distinct labour markets, a local and a non-local one (Van den Berg and Gorter 1997; Dohmen 2005; Coulson and Fisher 2009; Van Vuuren 2009; Rouwendal and Nijkamp 2010; Munch et al. 2006; Morescalchi 2016). In this type of models, reservation wages for non-local jobs are higher than local jobs because the worker requires a compensation for mobility. These models are used to predict differences in labour outcomes among unemployed workers with different mobility costs, typically homeowners and renters. Higher mobility costs imply that homeowners have higher search intensity and higher probability to find job locally, but lower search intensity and lower probability to find job with a regional move (Munch et al. 2006; Morescalchi 2016). Morescalchi (2016) has recently demonstrated that the second effect prevails, yielding an overall negative impact of homeowership on search intensity and on the job finding rate. The result that homeowners should experience longer unemployment spells due to barriers to mobility is known in the literature as "Oswald's hypothesis" (Oswald 1996, 1997, 1999).

The existing empirical evidence has provided some support to the predictions that homeowners should exit unemployment more (respectively, less) rapidly for (non-)local jobs (Munch et al. 2006; Battu et al. 2008; Van Vuuren 2009). However, in contrast with "Oswald's hypothesis", most microeconometric studies have found that exits from unemployment are overall not slower, or even faster, for homeowners (Goss and Phillips 1997; Coulson and Fisher 2002; Flatau et al. 2003; Munch et al. 2006; Van Vuuren 2009; Battu et al. 2008; Morescalchi 2016). Some authors have proposed to compare homeowners with private renters only, since public renters may face constraints to mobility similarly to homeowners (Battu et al. 2008; Morescalchi 2016), such as below-market rent, long waiting lists, security of tenure, and restricted right transferability (Hughes and McCormick 1981, 1987; Battu et al. 2008). However, even when this distinction is taken into account, support to "Oswald's hypothesis" is rather limited (Flatau et al. 2003; Battu et al. 2008; Morescalchi 2016).

This evidence is in contrast not only with theoretical predictions, but also with evidence on job search intensity, since Morescalchi (2016) finds that (outright) homeowners use fewer search methods than renters, suggesting that they search indeed less intensively. Therefore Morescalchi (2016) has investigated the choice of search methods as an explanation of diverging unemployment outcomes between homeowners and renters. He finds that homeowners use relatively more often newspapers advertisements, and that renters use relatively more often public employment offices, whereas the latter search method is associated with relatively longer unemployment spells. Therefore, the combined evidence that homeowners use fewer search methods, but select those associated to higher returns, seems to suggest that they search more efficiently.

However, no evidence exists so far informing whether search methods are selected efficiently also from a spatial point of view. In the context of the choice of search methods, mobility costs increase (respectively reduce) relative expected profits associated to methods that are more conducive to local (respectively non-local) employment. Therefore, homeowners (respectively renters) should dedicate relatively more effort to search methods that are more productive in terms of local (respectively non-local) employment. By assessing the productivity of search methods separately for local vis-à-vis non-local jobs, together with search method choice, the present analysis can provide evidence on regional efficiency.

3 Data

The data set used is the British Household Panel Survey (BHPS), a nationally representative longitudinal survey collecting yearly interviews for about 10,000-15,000 individuals. Interviews normally take place in September. The BHPS started in 1991 and ran until 2008 for a total of 18 waves. Thereafter the BHPS became part of a new longitudinal study called Understanding Society.

The BHPS is a rich data set containing thorough information on individual and household characteristics. Relevant for the analysis of this paper, is the presence of detailed information on labour market histories. Each individual is asked to report information on the current labour market spell at the time of interview, as well as on all previous spells back to one year before. Making use of information on start and end date of each spell, it is possible to construct complete series of monthly labour market states.

Information on job search is available only for unemployment spells ongoing at the time of interview. Specifically, the individual is asked to report whether he was looking for work in the previous four weeks as well as any search methods used in case of active search. In the present analysis, unemployment spells are defined by the combination of self-declared unemployment and active search. Unemployment spells thus defined can terminate with a transition to job or out of labour force. In case unemployment is observed in the last interview available, the spell is right censored since the end date cannot be observed.

Since information on job search is not available for spells started and completed within two subsequent interviews, these shorter spells have to be dropped from the sample, similarly to Battu et al. (2008). However, using information on duration and exits from the list of recalled spells in the latest wave, these spells can be used to construct a sampling weighting factor to reproduce the population of all spells. This weighting factor will be exploited to assess sensitivity of results. Note that it is not possible to distinguish between pure unemployment and inactivity spells in between waves since job search status is not observed.

The unemployed jobseeker can select any out of five search methods, namely (i) applied directly to an employer (DAE); (ii) studied or replied to advertisements

(ADS); (iii) contacted a private employment agency or Job Centre (EA); (iv) asked friends or contacts (SOCNET); (v) took steps to start own business (SEMP). Information about job search methods is present in the BHPS from wave 6 onwards, therefore only unemployment spells since 1996 are used in the present analysis. Evidence on the effect of public employment service may be partial since the data do not allow distinguishing between public and private employment agencies. Evidence from the UK LFS reveals that public agencies are used three times more often than private ones (Bachmann and Baumgarten 2013), suggesting that public agencies can be a major determinant of the combined effect.

In data with repeated measurement of spells, artificial spells can arise by discrepancies between what an individual recalls at a certain interview, and what was recorded at the previous interview. Following Upward (1999), and similarly to Battu et al. (2008) and Monchuk et al. (2014), these seam effects are dealt with by applying the principle that information recorded closest to a certain event is the most reliable. Namely, the following three general rules have been used: (i) if the earliest spell recalled at wave t starts on or before the date of interview in $t-1$, and labour force status has changed, the spell is considered a new one and the start date is set equal to the following month; (ii) if the earliest spell recalled at wave t starts on or before the date of interview in $t-1$, and labour force status is the same, the spell is considered the same; (iii) the start date of the unemployment spell is derived from the earliest interview in the spell.

In order to investigate the geographical scope of search methods, exits to job are decomposed between exits within and across local labour markets (Munch et al. 2006; Battu et al. 2008; Monchuk et al. 2014). Mobility across local labour markets is defined as a change of Travel To Work Area (TTWA) (Green and Owen 1990; Coombes et al. 1997; McCulloch 2003). TTWAs are defined to represent areas where the bulk of the resident population also work within. The UK is divided in 308 TTWAs according to the definition in use in the BHPS, with average population of 202,444, ranging from 7,336 for Knighton and Radnor to 6,768,503 for London.[3] An alternative possible definition of local labour market is the Local Authority District (LAD) (Battu et al. 2008).[4] TTWAs are preferred in the present analysis, because they are designed with the purpose of approximating local labour markets, while LADs reflect administrative local units. Results based on LADs are also reported as a robustness check (see Section 5.4 for discussion).

Exits to non-local jobs are defined as exits associated to a residential move to another TTWA occurring around the date of exit. Exits to local jobs are defined residually, comprising exits without move or associated to within-TTWA move. The identification of cross-TTWA moves is based on information recorded by the BHPS about the change of address in the last 12 months and the area of residence. A time

[3]Population figures are drawn from Nomis, Office for National Statistics, ONS UK (www.nomisweb.co.uk).

[4]The boundary definition of LADs in use in the present data is the one in place before the local government changes of 2009, with a total of 434 LADs. In 2000 the population of LADs was on average 135,682, ranging between 2,100 (Isles of Scilly) and 985,100 (Birmingham). Population estimates are drawn from Nomis, Office for National Statistics, ONS UK (www.nomisweb.co.uk).

window of 3 months before and 12 after the entry into job is used to define job-related regional moves. This choice is based on evidence that the majority of workers first accept a new job and then search for a new residence (Munch et al. 2006; Gregg et al. 2004). Moreover this choice is consistent with the sequence of events in the model, with job search choices coming before residential outcomes. The BHPS contains also a question on whether the residential move occurring in the last 12 months was related to job. It is found that only 5.7% of job related regional moves as defined above is not consistent with the specific BHPS question. This evidence provides strong support to the validity of the definition. These few unemployment spells have been deleted. Robustness checks are also reported by using larger intervals of 6 and 9 months before job entry (see Section 5.4 for discussion). The number of cross-TTWA moves increases by 9.1% and 15.2% respectively, and results remain largely similar in both cases.

Finally, since including ongoing spells would over-represent long duration spells (Lancaster 1990), only spells starting after September 1995 are used in the analysis (Battu et al. 2008). After deleting cases for individuals outside the age band 16–64 or with missing values in relevant variables, the resulting sample of unemployment spells is summarized in Table 2. On the total 1,656 unemployment spells observed, 105 (6.3%) end with exit to non-local job, 984 (59.4%) with exit to local job, 245 (14.8%) with exit out of the labour force, and 322 (19.4%) are right censored.

Table 2 reports also summary statistics on variables used in estimation (see Appendix A for description of variables). The value of these variables is taken from the last record before the end of the spell. Looking at the five search methods, ADS is the most used in total (79%), EA is the second most used (73%), DAE and SOCNET are used by a slightly lower fraction of unemployed people ($\sim$ 70%), while SEMP is used only by 9%. By comparing the two job inflows it can be noticed that individuals exiting to non-local employment are on average younger, more educated, more likely to be private renter but less likely to be social renter, more likely to have no dependent children and to be married, and finally they earned higher salary in the previous job. These individuals appear to have somehow better labour market characteristics, which is consistent with a lower average spell duration (5.1 vis-à-vis 8.9 months).

4 Econometric model

The probability of exiting unemployment was modeled by a semi-parametric Cox Proportional Hazard model with the following three competing risks (CR): (i) non-local employment (involving a cross-TTWA residential move), (ii) local employment (involving a within-TTWA move or no move), and (iii) inactivity. The CR-Cox model was combined with a Propensity Score Matching (PSM) approach in order to estimate the effect of search methods. Several studies have examined the application of PSM methods in survival analysis (see inter alia Gayat et al. 2012; Austin 2013, 2014). The effect of the treatment on the cause-specific hazard can be estimated by running a Cox model on the matched sample, using as explanatory variable an indicator denoting

Table 2 Summary statistics for unemployment spells

	Total	Non-local job	Local job	OLF	Unemployed
Nr. of spells	1656	105 (6.3%)	984 (59.4%)	245 (14.8%)	322 (19.4%)
Nr. of individuals	1403	98	842	233	322
	mean	mean	mean	mean	mean
Months unemployed	8.920	5.124	8.912	10.873	8.699
DAE	0.688	0.838	0.679	0.600	0.736
EA	0.732	0.733	0.739	0.698	0.736
SOCNET	0.705	0.724	0.740	0.612	0.661
ADS	0.793	0.848	0.811	0.751	0.752
SEMP	0.086	0.124	0.093	0.053	0.075
Age 16—24	0.429	0.571	0.377	0.437	0.537
Age 25—34	0.226	0.276	0.228	0.192	0.230
Age 35—44	0.159	0.076	0.184	0.151	0.118
Age 45—64	0.185	0.076	0.211	0.220	0.115
Female	0.376	0.476	0.335	0.555	0.332
Unemployment benefit	0.569	0.562	0.572	0.490	0.621
No qualifications	0.260	0.105	0.253	0.273	0.323
O Levels or equivalent	0.234	0.181	0.239	0.216	0.252
A Levels or equivalent	0.138	0.152	0.145	0.122	0.124
Nursing and other	0.222	0.238	0.237	0.220	0.174
1st degree or above	0.145	0.324	0.126	0.167	0.127
Married	0.406	0.467	0.430	0.384	0.332
Children 0—15 years	0.220	0.152	0.241	0.216	0.183
Homeowner	0.558	0.571	0.608	0.576	0.388
Social renter	0.311	0.086	0.313	0.306	0.382
Private renter	0.131	0.343	0.079	0.118	0.230
Last wage ('000)	0.985	1.140	1.025	0.847	0.918
Reservation wage ('000)	0.978	1.006	0.945	0.834	1.183
Employment growth	1.705	2.115	1.696	1.481	1.769
Managers, administrators	0.070	0.076	0.070	0.065	0.071
Professional, technical	0.077	0.143	0.080	0.073	0.047
Clerical, secretarial	0.118	0.152	0.130	0.102	0.084
Craft & related	0.101	0.076	0.116	0.061	0.096
Personal, protective	0.097	0.114	0.098	0.102	0.087
Sales	0.074	0.133	0.072	0.078	0.056
Plant, machine operatives	0.101	0.038	0.117	0.057	0.106
Other occupation	0.239	0.181	0.234	0.233	0.276
No job before	0.123	0.086	0.083	0.229	0.177
Manufacturing	0.156	0.086	0.187	0.114	0.118
Construction	0.038	0.038	0.049	0.008	0.028
Wholesale, retail, motors	0.079	0.076	0.091	0.053	0.059

Table 2 (continued)

	Total	Non-local job	Local job	OLF	Unemployed
Hotels, restaurants	0.075	0.152	0.066	0.073	0.081
Transport, etc.	0.045	0.067	0.046	0.041	0.040
Estate, renting, business	0.086	0.076	0.091	0.078	0.081
Education	0.030	0.086	0.024	0.012	0.040
Health, social work	0.039	0.086	0.030	0.069	0.025
Other social	0.039	0.019	0.041	0.037	0.040
Other sector	0.290	0.229	0.291	0.286	0.311
York. & Humb.; N. East	0.106	0.038	0.116	0.102	0.102
North-West	0.089	0.076	0.097	0.094	0.065
E. and W. Midlands	0.123	0.133	0.124	0.151	0.096
East Anglia	0.105	0.171	0.115	0.078	0.075
South East	0.100	0.190	0.097	0.094	0.084
South West	0.060	0.171	0.065	0.041	0.025
Wales	0.146	0.095	0.140	0.184	0.149
Scotland	0.179	0.105	0.164	0.192	0.239
Northern Ireland	0.092	0.019	0.083	0.065	0.165
Year 1996	0.060	0.095	0.059	0.057	0.056
Year 1997	0.072	0.105	0.077	0.057	0.059
Year 1998	0.065	0.095	0.065	0.061	0.059
Year 1999	0.068	0.048	0.073	0.082	0.050
Year 2000	0.091	0.143	0.084	0.098	0.087
Year 2001	0.141	0.133	0.117	0.159	0.205
Year 2002	0.109	0.057	0.111	0.102	0.127
Year 2003	0.103	0.095	0.098	0.139	0.096
Year 2004	0.088	0.029	0.106	0.073	0.062
Year 2005	0.095	0.095	0.118	0.045	0.065
Year 2006	0.106	0.105	0.092	0.127	0.134

treatment status (Austin and Fine 2019). The following Cox hazard model was hence fitted:

$$h\left(t|treat_i\right) = h_0\left(t\right)\exp\left(\beta\, treat_i\right), \tag{1}$$

where $h\left(t|treat_i\right)$ is the hazard that the given event occurs for individual i, conditional on the treatment indicator $treat_i$, and the individual-constant component $h_0\left(t\right)$ represents the fully unrestricted baseline hazard. As usual in CR models, competing events can be treated as censorings, i.e. subjects experiencing the competing event are treated as if they were no longer under observation since the time the event occurred (Austin and Fine 2019). Inclusion of covariates in Eq. 1 other than $treat_i$ is unnecessary since PSM has balanced the distribution of observed covariates between treatment and control groups. The target estimand is the hazard ratio associated to the treatment indicator, that is the ratio of hazards between treated and untreated individuals; note that this quantity is time-constant and equal to $\exp(\beta)$, since $h_0(t)$ cancel out when computing the ratio.

In this framework, each individual can be exposed to multiple concurrent treatments, consisting in the use of the following four methods: EA (public or private employment agency), SOCNET (social contacts), DAE (direct approach to employers), ADS (studied/replied to advertisements).[5] When multiple treatments are mutually exclusive, relative treatment effects can be estimated by a series of pairwise matchings between treatment groups, or by matching via a vector of treatment probabilities, known as Generalized Propensity Score (GPS) (Imbens 2000; Lechner 2001, 2002; Cattaneo 2010; Lopez and Gutman 2017). In case of concurrent treatments, these approaches can be implemented as well by forming all possible combinations across treatments (Siddique et al. (2019), Becker and Egger (2013), and Wooldridge (2010), Sec. 21.6.3). However, this approach is not easily tractable in the present setting because of the large number of treatment packages that would arise, i.e. $4 \cdot 4 = 16$. With several treatments, relative treatment effects estimated with pairwise matching may have limited external validity (Lopez and Gutman 2017), while GPS matching may be challenging for the definition of a meaningful common support region, as well as for assessing and correcting for covariate imbalance (McCaffrey et al. 2013). In addition, effects of individual treatments would be estimated in combination and hence not easily interpreted. Therefore, the approach followed in the present analysis consists of estimating the effect of each search method separately, replacing the treatment indicator $treat_i$ in Eq. 1 with each method in turn, and including residual methods in the PS. This approach allows disentangling the effect of each method by conditioning on the other methods in the PS. However, a drawback is that possible complementarities between methods are not taken into account, because average treatment effects are assumed independent of covariates.

PSM was applied by using an Inverse Probability of Treatment Weighting (IPTW) algorithm, where the PS was estimated running a binary model for any of the treatments considered.[6] IPTW is generally implemented by estimating a weighted model with weights equal to $1/(1 - PS)$ for control subjects (Hirano et al. 2003). Weights for treated subjects were set equal to $1/PS$ to give an interpretation to hazard ratios as treatment effects averaged over the entire population rather than over the treated population only (Austin 2013); note that this enables comparison between effects of search methods. These weights were hence used to estimate a weighted Cox model as in Eq. 1. The IPTW algorithm was chosen following recommendations of Austin (2013), who performed an extended series of Monte Carlo simulations to compare performance of alternative PSM methods in estimating the hazard ratio in a Cox model. Austin (2013) found that IPTW and Nearest Neighbour Matching (NNM) with Caliper provide estimates with minimal bias, while PSM estimators based on

[5] A PSM analysis was not performed for SEMP (steps to start business) because this method is used by a very small fraction of unemployed (9%); note that this method has received only scant attention by previous literature (see Section 2.1)

[6] The probit specification was preferred over the logit because it provided better matching diagnostics.

stratification and covariate adjustment provide biased estimates. IPTW was preferred over NNM in the present analysis because of better matching diagnostics and because it does not imply discarding unmatched cases. To assess robustness of results, a check was also performed employing a NNM algorithm (see Section 5.4).

Estimation of standard errors in PSM models require in general some care as one should take into account the matched nature of the sample (Abadie and Imbens 2008, 2016). Moreover, the bootstrap method may not be a valid alternative with some matching algorithms, although it should provide correct estimates with IPTW matching (Abadie and Imbens 2008). In the context of survival analysis, Austin (2013) found that conventional methods of variance estimation of hazard ratios in IPTW-weighted Cox models result in biased estimates. However, in comparing performance of alternative variance estimators for the same case, Austin (2016) found that bootstrap provides approximately correct estimates of standard errors and confidence intervals with the correct coverage rates, while naïve model-based and robust sandwich-type estimators provide biased estimates. Therefore, in the present analysis, standard errors of hazard ratios were estimated by bootstrap with 2,000 replications.

The validity of the matching strategy rests on the Conditional Independence Assumption (CIA), requiring that outcomes should be independent of treatment conditional on the PS (Rosenbaum and Rubin 1983). Therefore the model for the PS should include all relevant predictors of the outcome that influence selection into treatment at the same time (Caliendo and Kopeinig 2008). The reliability of CIA is sustained in the present analysis by the use of a rich set of predictors. This set comprises 55 variables, including individual characteristics (use of non-focal search methods, age, gender, education, receipt of unemployment benefit, reservation wage), previous job's characteristics (occupation, sector, wage), household characteristics (marital status, presence of children, housing tenure), and indicators accounting for macro-economic effects at the national (year dummies), regional (region dummies) and local level (LAD employment growth).[7] Details on the creation of these variables are presented in Appendix A, while summary statistics are reported in Table 2. The value of these variables is taken from the last record before the end of the spell and is assumed constant throughout the spell. Alternative estimates allowing for time-varying covariates are also presented (see Section 5.4 for discussion).

Despite the use of a comprehensive set of covariates, CIA may fail if there are relevant unobserved factors affecting concurrently outcomes and treatments. The reliability of CIA is discussed in the rest of this section by focusing on the determinants of outcomes rather than treatments, consistently with evidence that omitting relevant variables from the outcome model may result in a larger variance-penalty (Brookhart et al. 2006). The appropriateness of the covariate specification for the job hazards is discussed taking into account the job finding probability and the relative employment probability separately. First, following standard theoretical models of job search, the job finding probability can be modeled by the combination of job offers frequency, wage offered, and acceptance rate. These variables are in turn determined by the job

[7] Information on employment growth was not available at the TTWA level.

search intensity, employers' expectations about worker's productivity and reservation wage. Search intensity and reservation wage are controlled for in the proposed PS specification by search methods and self-reported reservation wage, respectively, but also indirectly by age, gender, education, unemployment benefit, and local labour market conditions. Expected worker's productivity is proxied for by age, education, and past job history; notably, past wage can proxy for unobserved ability.

Possible omitted variables from this specification may be personality and non-cognitive traits, which a recent strand of research has shown to influence unemployment duration (Sansale et al. 2019; Viinikainen and Kokko 2012; Uysal and Pohlmeier 2011) and job search behaviour (Caliendo et al. 2015; McGee 2015; DellaVigna and Paserman 2005). Personality traits are relevant for unemployment duration because they affect worker's disutility of search and preferences for work, which determine the reservation wage and the intensity of search, and because they influence worker's productivity (Sansale et al. 2019). Unfortunately information on personality traits is available in the BHPS for one wave only. However, since their effect on unemployment duration should be mediated by the reservation wage, search intensity, and worker's productivity, the mentioned predictors should account also for this source of variation. Second, relevant for the relative employment hazard is the inclusion of determinants of regional mobility. This condition appears largely satisfied by inclusion of housing tenure, marital status, children, age, education, unemployment benefit, previous job characteristics, and local labour market conditions.

5 Results

This section presents evidence on the relation between search methods and unemployment exits. Namely, Section 5.1 reports estimates of treatment selection models used to estimate the Propensity Score, and Section 5.2 presents diagnostics on the quality of the matching procedure; Section 5.3 reports main estimates of the effects of search methods on unemployment exits, and Section 5.4 reports robustness checks; finally, Section 5.5 reports additional evidence on the effect of search methods on post-unemployment outcomes.

5.1 Treatment selection models

Propensity Scores (PS) were estimated by running probit models for each of the four treatments considered, namely DAE, EA, SOCNET, and ADS. Table 3 reports estimates for these treatment selection models (full results showing all covariate coefficients are reported in Table 7 in Appendix B). Estimates show a somewhat strong correlation in the use of search methods. Remark that in estimating the effect of a given method on unemployment exits, such imbalance in the distribution of residual methods can be eliminated by matching.

The effect of other covariates on the selection of individual methods is in line with existing evidence (Osberg 1993; Schmitt and Wadsworth 1993; Böheim and Taylor 2001; Weber and Mahringer 2008; Bachmann and Baumgarten 2013; Morescalchi

Table 3 Treatment selection models

	DAE	EA	SOCNET	ADS
DAE		0.039	0.544***	0.576***
		(0.500)	(7.314)	(7.263)
EA	0.046		0.213***	0.471***
	(0.580)		(2.722)	(5.545)
SOCNET	0.551***	0.209***		0.120
	(7.409)	(2.702)		(1.434)
ADS	0.586***	0.471***	0.120	
	(6.932)	(5.407)	(1.366)	
SEMP	−0.009	0.101	0.270**	0.153
	(−0.071)	(0.795)	(2.090)	(1.090)
Age 25—34	−0.251**	−0.227**	−0.071	0.084
	(−2.518)	(−2.218)	(−0.728)	(0.766)
Age 35—44	−0.313***	−0.186	−0.134	0.219*
	(−2.711)	(−1.544)	(−1.171)	(1.712)
Age 45—64	−0.461***	−0.241**	−0.193*	0.223*
	(−4.184)	(−2.132)	(−1.748)	(1.857)
Female	−0.133	−0.226***	−0.097	0.150
	(−1.634)	(−2.711)	(−1.199)	(1.618)
Unemployment benefit	−0.070	0.394***	−0.090	0.190**
	(−0.942)	(5.260)	(−1.193)	(2.390)
Reservation wage (log.)	−0.011	−0.119	−0.118	−0.160*
	(−0.147)	(−1.316)	(−1.599)	(−1.736)
Last wage (log.)	0.100*	0.045	−0.005	−0.039
	(1.669)	(0.744)	(−0.084)	(−0.627)
O Levels or equivalent	0.047	0.067	0.052	0.131
	(0.482)	(0.651)	(0.521)	(1.274)
A Levels or equivalent	0.104	−0.005	0.090	0.211*
	(0.866)	(−0.038)	(0.737)	(1.649)
Nursing and other	0.150	0.016	−0.059	0.405***
	(1.435)	(0.145)	(−0.558)	(3.546)
1st degree or above	0.473***	−0.205	−0.105	0.551***
	(3.587)	(−1.583)	(−0.816)	(3.721)
Married	0.192**	0.063	−0.025	0.105
	(2.134)	(0.691)	(−0.291)	(1.033)
Children 0—15 years	0.144	−0.031	0.092	−0.088
	(1.421)	(−0.297)	(0.918)	(−0.776)
Social renter	-0.110	−0.083	0.015	−0.036
	(−1.312)	(−0.955)	(0.174)	(−0.396)
Private renter	0.131	0.069	−0.206*	−0.254**
	(1.179)	(0.615)	(−1.919)	(−2.167)

Table 3 (continued)

	DAE	EA	SOCNET	ADS
Employment growth	0.004	0.007	−0.002	0.003
	(0.568)	(1.037)	(−0.290)	(0.437)
Occupation	✓	✓	✓	✓
Sector	✓	✓	✓	✓
Region	✓	✓	✓	✓
Year	✓	✓	✓	✓
Nr. of observations	1656	1656	1656	1656

Notes: * significant 10%, ** significant 5%, *** significant 1%. t-statistics in parenthesis have been calculated with robust standard errors. Treatment selection equations have been estimated with a probit model. The following reference categories have been omitted for categorical variables: Age 16—24, No qualifications, Homeowner

2016). Age is found to influence negatively DAE and EA, and to a lesser extent SOCNET, while ADS appears to be used more frequently by individuals above 35. EA may be more frequently used by young men (below 25), because they lack experience in job search activity and require guidance, or because they have poor alternatives (see also Osberg (1993), Schmitt and Wadsworth (1993), and Böheim and Taylor (2001)). At the same time, young men may be more likely to use DAE (Schmitt and Wadsworth 1993; Böheim and Taylor 2001), because they have a smaller job network to draw information or referrals upon. Workers in the highest age category may be less likely to use SOCNET (Bachmann and Baumgarten 2013), because they may be more detached from social contacts and because their relatives may be less likely to be alive or to provide effective support in job search. The positive relation between ADS and age may reflect an association between this method and a more strategic behaviour (Weber and Mahringer 2008).

Education is found to increase the use of ADS and DAE, but no significant effects are found for SOCNET and EA. These findings are consistent with the notion that highly educated workers are more likely to use more costly search channels such as DAE and ADS, rather than SOCNET and EA, because they face higher offer arrival rates or lower search costs (Weber and Mahringer 2008). At the same time, since highly educated individuals may have access to a geographically larger labour market, they may be more likely to respond to advertisements placed in national or international media, and may be less reliant on local labour market contacts (Böheim and Taylor 2001). Moreover, they may apply more often directly to potential employers, because they may be more proactive in job search (Böheim and Taylor 2001).

It is worth noting that, while none of the reported effects for SOCNET and EA is significant, the coefficient of the highest category becomes significant if the lowest categories are grouped together. This suggests that highly qualified workers are less likely to rely on social contacts relative to workers with lower qualifications, and that they should be less dependent on job search counseling from EA by virtue of higher skills in job search activities.

The reservation wage decreases the use of all search methods, consistently with theory predicting a negative relation with search intensity. However, only the effect on ADS is statistically significant, suggesting that the worker may expect to receive lower wage offers with this method. Workers earning higher wage in the last job are more likely to use DAE, suggesting a connection between ability and use of more costly methods similarly to education. Receipt of unemployment benefits increases the likelihood of using EA, as one would expect given that in the UK eligibility is conditional on demonstration of active search and frequent contact with the Jobcentre (Manning 2009; Petrongolo 2009). Benefit claimants are also more likely to use ADS, possibly reflecting such institutional requirements (Böheim and Taylor 2001). Females are less likely to use EA, but no significant effect is found for DAE, SOCNET and ADS. Married status has only a positive impact on use of DAE, while presence of dependent children has no significant effects on any method. Employment growth at the local level has no significant effects.

Finally, housing tenure appears to influence only the propensity to use SOCNET and ADS; namely, private renters have lower likelihood to use such methods relative to homeowners, while no significant differences are found between social renters and homeowners. Homeowners and social renters should be more well-established in local communities, therefore they may have a larger network of social connections for job search (Osberg 1993). At the same time, they may have easier access to sources of information about local job opportunities, and hence they may be more likely to use ADS for local jobs (Morescalchi 2016). Since homeowners and social renters have larger mobility constraints (see discussion in Section 2.2), selection of SOCNET and ADS may be an efficient search strategy for them as long as such channels are more conducive to local employment. This issue will be investigated in Section 5.3

5.2 Diagnostics on PS matching procedure

This Section provides an overview on the quality of PS model specification and matching for the treatments considered. Evidence and additional details are reported in Appendix C.

The adequacy of the specification for the PS can be checked by assessing covariate balance conditional on the estimated PS (Imbens and Rubin (2015), Ch. 13). This strategy exploits the independence property between the treatment indicator and the vector of covariates for any given value of the PS. Comparisons of covariate means between treatment and control groups by blocks of the PS distribution (see Fig. 2) suggest that the proposed PS specifications appear adequate, in the sense that they lead to covariate balance that is similar to what one would expect if treatments were randomized within blocks (Imbens and Rubin 2015).

The quality of the matching approach can be diagnosed by examining two assumptions required for consistent estimation, namely the common support condition and covariate balance. These two issues are discussed in turn.

The common support or overlap assumption requires that any individual has a positive conditional probability to be part of the treatment or control group. When this condition is violated, there exist individuals in the control group that are not comparable to individuals in the treatment group, and viceversa, which may represent a major source of evaluation bias as conventionally measured (Heckman et al. 1997).

This condition was implemented by deleting all observations with a value of the PS that is smaller than the minimum or larger than the maximum of the opposite group (Caliendo and Kopeinig 2008). Only small portions of samples were deleted after applying this rule (DAE: 2%; EA: 0.4%; SOCNET: 0.7%; ADS: 4.5%); therefore, no concerns on estimates representativeness appear to arise (Bryson et al. 2002). Overlap in the region of common support can be checked by visual inspection of the PS density distributions of treatment and control groups thus defined (Lechner 2008). Figure 3 shows that PS distributions become rather similar after employing matching weights, suggesting that matching leads to adequate overlap. To assess robustness of estimates, a more restrictive rule for the common support was also experimented by trimming cases outside the 5-95% range of the PS (see Section 5.4 for discussion).

A second source of evaluation bias can be represented by covariate imbalance, that is differences in covariate distributions between treatment and control groups (Heckman et al. 1997).

Covariate imbalance is most commonly measured by standardized differences in variable means between treated and untreated groups (Rosenbaum and Rubin 1985). Figure 4 shows that, for all treatments, standardized differences decrease remarkably after employing matching weights and are generally well below conventional thresholds, suggesting that matching does a very good job in balancing covariates. Moreover, note that, since any imbalance in covariate distributions is reflected in differences in the PS distribution, the check on PS distributions already discussed indicates also balance in joint covariate distributions (Imbens and Rubin (2015), Section 14.4). Finally, by inspecting evidence for single search methods, it appears that ADS has the worst balance scores, although within acceptable bounds. While this imbalance appears minor, it is worth checking sensitivity of estimates to alternative matching with possibly improved balance. The mentioned robustness check on PS trimming is appropriate even for this purpose (see Section 5.4).

5.3 Search methods and unemployment exits

This Section reports estimates of the effect of search methods on the probability to leave unemployment for three competing exits, namely non-local job, local job, and out of labour force (OLF). Estimates of a competing-risks (CR) Cox model on the unmatched sample are reported in Appendix D as a reference (see Table 8). These results show that SOCNET and ADS increase the probability to find a local job, respectively by 48.5% and 18.5%. No other significant effects were found for any treatment, although the effect of DAE on the mobility hazard is close to the 10% significance threshold. Estimates of effects of other covariates are in line with economic interpretations proposed in the literature.

Table 4 reports estimates of CR Cox models using Propensity Score Matching (PSM) with Inverse Probability of Treatment Weighting (IPTW). The Table reports hazard ratios for each of the four treatments by row, and for each competing exit by column. Results show that SOCNET and ADS have positive impact on the probability

Table 4 Effect of search methods on cause-specific hazards. Propensity Score Matching Estimates with Inverse Probability of Treatment Weighting

	Non-local job	Local job	OLF
DAE	1.912*	0.927	0.876
	(1.924)	(−1.161)	(−0.916)
Nr. of observations	1623		
EA	0.982	1.059	1.084
	(−0.073)	(0.790)	(0.489)
Nr. of observations	1650		
SOCNET	0.959	1.313***	0.947
	(−0.165)	(3.775)	(−0.384)
Nr. of observations	1645		
ADS	0.960	1.257**	0.993
	(−0.110)	(2.010)	(−0.032)
Nr. of observations	1582		

Notes: * significant 10%, ** significant 5%, *** significant 1%. Reported coefficients are hazard ratios. t-statistics in parenthesis have been calculated by bootstrapping standard errors with 2000 replications. Estimates have been performed by a weighted Cox proportional hazard model on the treatment indicator, where weights have been computed with a propensity score-based IPTW algorithm. Separate models have been estimated for each search method on specific common support samples. See Table 3 for estimates of treatment selection equations

to find job locally. Specifically, SOCNET enhances the likelihood to leave unemployment for a local job by 31.3%, and ADS by 25.7%. No significant effects were found on exit to non-local employment for either methods. Opposite results were found for DAE, with a positive (mildly) significant effect on the mobility hazard and no significant effect on the local hazard. As concerns EA, no significant effects were found on either hazard. Finally, effects on the OLF hazard are not significant for any search method. Note that this evidence overall is qualitatively in line with evidence in Table 8 (Appendix D) for the unmatched sample, although the effect of DAE on the mobility hazard is stronger and becomes significant after matching.

Conditional hazard functions for local jobs and non-local jobs were estimated to investigate possible unemployment duration dependence. Figure 1 reports hazard functions estimated for each treatment model with matching weights, conditioning on the value of treatment being equal to 1. Both hazards are found to grow at the beginning of the unemployment spell, and decline roughly monotonically thereafter; however, the decay in the mobility hazard starts much earlier, and the initial growth is less pronounced in this case. Some theoretical arguments put forward in the literature suggest that the job finding rate might decline with unemployment duration; leading explanations are human capital deterioration and employer screening (see Machin and Manning (1999), for a review). However, existing evidence on unemployment duration dependence is in general mixed and controversial (Ljungqvist and Sargent 1998). In fact, while several studies have reported negative duration dependence (see, inter alia, Imbens and Lynch (2006) and Shimer and Werning (1998)), other studies have shown that the dependence may disappear or even become positive when worker's observed characteristics (Machin and Manning 1999) or unobserved heterogeneity (Van den Berg and Van Ours 1996) are taken into account, and hence a genuine causal effect may not be clearly established (Kroft et al. 2013). One factor that may counteract the decline in job finding rates over unemployment duration is the possible concurrent decline in reservation wages (Machin and Manning 1999), which may arise by tightening liquidity constraints, finite lives, changes in the wage offer distribution, or just as a consequence of declining job finding rates (Devine and Kiefer 1991). Since the reservation wage cannot decrease below the level of the unemployment benefit, it is possible that the decline will stop at some point. Such dynamics in the reservation wage appear consistent with the hump-shaped behaviour of job finding rates observed in Fig. 1, where the initial increase may reflect a decline in the reservation wage, and the subsequent decrease may follow its stabilization. Furthermore, since the reservation wage for non-local jobs is shifted up by mobility costs, which are likely to be fixed over unemployment duration, the observed earlier reversal in the mobility hazard may reflect earlier stabilization of the non-local reservation wage.

Estimates in Table 4 are based on the assumption that the effect of search methods on unemployment exits is constant throughout the unemployment spell. In order to investigate possible changes in the effectiveness of search methods, the following augmented Cox model was estimated allowing for shifts at months $t = 3, 12$:

$$h\left(t|treat_i\right) = h_0\left(t\right)\exp\left(\beta_1\, treat_i + \beta_2\, I\left(t > 3\right) treat_i + \beta_3\, I\left(t > 12\right) treat_i\right), \tag{2}$$

where $I\left(\right)$ is the indicator function equal to 1 if the condition is true and 0 otherwise. According to this specification, the effect of the treatment on the linear index is β_1 in $(0, 3]$, $(\beta_1 + \beta_2)$ in $(3, 12]$, and $(\beta_1 + \beta_2 + \beta_3)$ in $(12, \infty)$. This model was estimated expanding the data in duration intervals and including interactions between treatment and interval indicators. Usual weights based on the PSM-IPTW algorithm were employed in estimation. Table 5 reports estimates of hazard ratios for the shift parameters. Results show that in most cases the effect of search methods on job

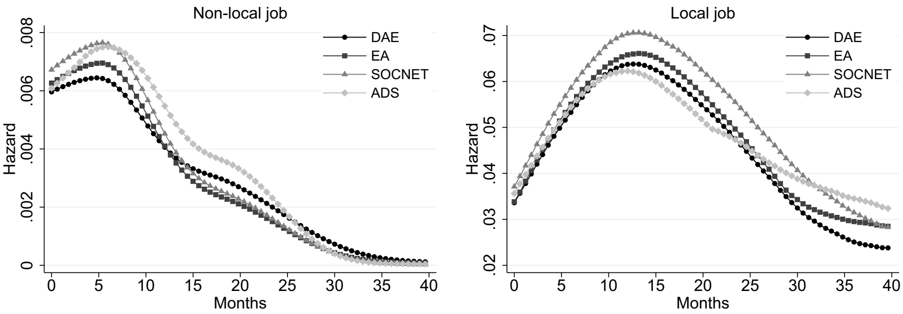

Fig. 1 Conditional Hazard Functions

Table 5 Time-varying effect of search methods on cause-specific hazards. Propensity Score Matching Estimates with Inverse Probability of Treatment Weighting

	Non-local job	Local job	OLF
DAE	1.441	0.938	1.277*
	(1.261)	(−0.937)	(1.767)
DAE ($t > 3$)	3.551***	1.159	0.422***
	(3.193)	(1.588)	(−4.396)
DAE ($t > 12$)	0.239***	0.746***	0.782
	(−2.871)	(−2.919)	(−1.239)
Nr. of observations	2985		
EA	0.997	1.102	1.182
	(−0.016)	(1.258)	(1.075)
EA ($t > 3$)	0.958	1.093	0.788
	(−0.156)	(0.851)	(−1.172)
EA ($t > 12$)	1.111	0.752**	0.984
	(0.214)	(−2.425)	(−0.068)
Nr. of observations	3034		
SOCNET	1.332	1.513***	0.944
	(1.393)	(5.135)	(−0.443)
SOCNET ($t > 3$)	0.547**	0.817*	1.173
	(−2.076)	(−1.917)	(0.853)
SOCNET ($t > 12$)	0.538	0.796**	0.837
	(−1.346)	(−2.036)	(−0.944)
Nr. of observations	3027		
ADS	0.856	1.051	0.849
	(−0.668)	(0.549)	(−0.938)
ADS ($t > 3$)	1.322	1.393***	2.334***
	(0.730)	(2.684)	(3.577)
ADS ($t > 12$)	0.884	1.248*	0.781
	(−0.191)	(1.746)	(−0.994)
Nr. of observations	2902		

Notes: * significant 10%, ** significant 5%, *** significant 1%. Reported coefficients are hazard ratios. t-statistics in parenthesis have been calculated by bootstrapping standard errors with 2000 replications. Estimates have been performed by a weighted Cox proportional hazard model on the treatment indicator with duration interactions, where weights have been computed with a propensity score-based IPTW algorithm. The data have been expanded to accommodate for duration intervals. Separate models have been estimated for each search method on specific common support samples. See Table 3 for estimates of treatment selection equations

hazards starts declining at some point during the unemployment spell: the effect of DAE is found to decline after 12 months for both local and non-local exits; the same is found for the effect of EA on local exits, while no significant shifts are found for non-local exits; a decay in the effect of SOCNET is observed as early as after

3 months for both exits, with a further decline after 12 months; ADS stands out as a particular case since the effect on local exits is found to rise progressively after 3 and 12 months, with no significant effects on non-local exits. In general, a decline in the effectiveness of search methods with unemployment duration may be explained by a decline in worker's job search efficiency, possibly due to human capital deterioration and discouragement. By cumulating coefficients in Table 5, it is possible to identify intervals where the overall effect reported in the analysis with time-invariant effect is generated. [8] Such exercise shows that the overall positive impact of DAE on the mobility hazard arises in the first year of unemployment, with a maximum in the 3–12 months interval. The positive effect of SOCNET on the local hazard arises similarly in the first year, however the maximum impact is reached in the first 3 months (51.3%), with subsequent drop to 23.6% in the 3–12 interval. The effect of ADS on local exits exhibits an opposite trend with respect to SOCNET, as it is null in the initial interval, it becomes positive in the 3–12 interval (46.4%), and grows to 82.7% after 12 months. A more rapid decline in the effectiveness of SOCNET may suggest that unemployed workers tend to access social contacts sequentially over the unemployment spell, taking advantage of the most productive contacts soon after becoming unemployed, and resorting to less profitable ones after their exhaustion. Moreover, because of detachment from the workplace and homophile effects, the social network of the unemployed worker may become increasingly composed by similarly unemployed contacts, that are less productive channels (Bramoullé and Saint-Paul 2010). At the same time, unemployed workers may become more efficient in using ADS to compensate for the decline in the productivity of social contacts, possibly explaining its growing hit rates.

5.4 Robustness checks

A number of alternative models were estimated to check robustness of estimates reported in Table 4. These checks are discussed in turn in this Section, and estimates are reported in Appendix E.

First, Table 9 reports estimates based on a more restrictive definition of common support, where cases with Propensity Score (PS) lower than 5% and higher than 95% were deleted (Wooldridge (2010), Sec. 21.3.3). By limiting the influence of extreme cases, PS trimming procedures may in general reduce estimation bias, possibly at the expense of an increase in variance and loss of representativeness (Caliendo and Kopeinig 2008). With this rule, no observations off of the common support were found for DAE and SOCNET in addition to those identified by the reference rule, respectively 2% and 0.7%. A larger sample restriction was implemented for ADS and EA, instead, being equal respectively to 10.1% vis-à-vis 4.5%, and to 1.3% vis-à-vis 0.4%. Following these restrictions, estimates for DAE and SOCNET were unchanged, while estimates for EA and ADS were affected only minimally, even

[8] Defining the hazard ratios reported in Table 5 for one possible method/exit combination as HR_0, HR_3 ($t > 3$), and HR_{12} ($t > 12$), the absolute hazard ratio for the interval 3–12 can be derived as $\exp(\log(HR_0) + \log(HR_3))$, and the absolute hazard ratio for the interval 12–∞ as $\exp(\log(HR_0) + \log(HR_3) + \log(HR_{12}))$. Note that the absolute hazard ratio for the interval 0–3 correspond to HR_0.

though a more substantial restriction was implemented in the latter case. Robustness to trimming mitigates concerns of possible bias arising by failure of the overlap condition. Moreover, such evidence can be taken into account to assess robustness to possible improvement in covariate balance. In fact, after implementing the more restrictive rule, a measurable improvement was observed for ADS, with standardized differences shrinking from 3.5 to 2.7 on average, and the B-statistic from 8.9 to 3. Since estimates for ADS are minimally affected, imbalance in the baseline matching does not appear relevant.

Second, Table 10 reports estimates based on an alternative PSM algorithm. A Nearest Neighbour Matching (NNM) algorithm with caliper was chosen because, unlike alternative algorithms, it can deliver unbiased estimates of hazard ratios in a Cox model (Austin 2013). The caliper condition imposes a tolerance level on the maximum PS distance (caliper) between matched cases, which was set equal to 0.05 in the present analysis. A radius approach using all cases within the caliper was preferred over one-to-one matching because the latter provided worse covariate balance and required to discard a considerable number of observations. Table 10 shows that estimates of hazard ratios are very similar to baseline estimates, suggesting that results are robust.

Third, Table 11 and 12 report results considering a longer time window to identify cross-TTWA moves, with an interval before job exit of 6 or 9 months instead of 3 months, respectively. In both cases results are largely unaffected, suggesting that estimates are robust.

Fourth, Table 13 reports results based on Local Authority Districts (LAD) as definition of local labour market, instead of Travel To Work Areas (TTWA). Results are qualitatively very similar to main estimates, suggesting robustness even in this case.

Fifth, the baseline model was estimated with a sampling weighting factor to correct for possible under-representation of shorter unemployment spells. This weighting factor was created with data on self-reported unemployment spells in between waves, for which job search is not observed, to reproduce the structure of all spells. Weights were created similarly to IPTW weights estimating a binary model for observed (cross-wave) vis-à-vis unobserved (in between-wave) spells, including all self-reported unemployment spells. The model was estimated with interaction terms between exit type and duration in addition to baseline covariates, and retrieving covariate values for unobserved spells from the earliest wave after termination. Figure 5 compares duration density distributions of spells in the estimation sample (observed) with the population of all spells (observed and unobserved); note that only unemployment jobseeking spells were used to represent observed spells in this Figure. The upper part of Fig. 5 shows that the distribution mass of spells in the estimation sample is shifted toward the right, confirming that in between-wave spells tend to be shorter; the lower part of the Figure shows that distributions become rather similar after employing sampling weights. Table 14 reports estimates of the baseline IPTW-PSM model, weighted by the product between IPTW and sampling weights. Estimates are not qualitatively affected by the correction, suggesting that baseline estimates appear representative of the whole population of spells.

Sixth, possible measurement error due to assuming time-constant search methods was taken into account. The use of search methods may change during the unemployment spell, possibly because some methods may turn out to be ineffective and hence abandoned, or may be exhausted or unavailable after some time. Since information on search methods is drawn from the last record available, and spread over the entire spell, method switching occurring at other stages may not be captured. This issue does not appear too serious when looking at the longitudinal use of methods. In fact, a given method is consistently reported or not reported in two consecutive waves by $68-73.2\%$ individuals (i.e. DAE: 72%, EA: 73.2%, SOCNET: 68%, ADS: 70%) , denoting some persistence. Considering that 77.7% of spells terminate by one year of unemployment, it is possible that such measurement error affects only a minority of cases.

Nevertheless, robustness checks were performed estimating models with time-varying search methods. To allow for within-spell variability, the sample was expanded by dividing unemployment spells in intervals associated to each wave the spell spans over; no cases were added for spells spanning one wave only. Variable values within each interval were imputed adopting the following rules: the earliest record was used for backward imputation until the start date; the latest record was used for forward imputation until the end date; intervals in-between waves were imputed using the latest record. With these rules, the average duration between data collection and the furthest imputed month is equal to 6.4 months; therefore, possible method switching may be rather infrequent within unemployment intervals thus defined. Two types of models were hence estimated, one with time-constant, and the other with time-varying weights (Ali et al. 2013, 2016). In the first case, CR-IPTW models were estimated by means of a time-constant PS derived as the probability of using the given method any time during the spell and taking covariate values from the earliest record (Ali et al. 2013). In the second case, a Marginal Structural Model (MSM) allowing for time-varying weights was used (Robins et al. 2000; Hernán et al. 2000). This model can be estimated by IPTW method, weighting each person-time observation by an inverse function of the exposure probability conditional on the history of treatment and covariates. The model was estimated in two steps, using data expanded in person-month form (see Fewell et al. (2004). for implementation). First, cumulative IPTW weights were derived by estimating a pooled logistic model for each treatment, controlling for baseline values of treatment, as well as current and baseline values of covariates.

Cumulative censoring weights were similarly created and multiplied to treatment weights. In the second step, a weighted pooled logistic regression was fitted to estimate the effect of the time-varying treatment on the relevant exit. Note that the odds ratios estimated by this model correspond to the hazard ratios of an equivalent Cox model, because the hazard of treatment in any single month is small (D'Agostino et al. 1990). Estimates of the two aforementioned types of models are reported in Tables 15–16. In both cases results are qualitatively similar to the ones obtained assuming time-constant search methods; if anything, all the significant effects become slightly larger, although of comparable size with respect to the baseline. Overall this evidence suggests that possible measurement error arising by time-variability in treatments does not appear to introduce meaningful bias.

As a final robustness check, a model allowing for individual unobserved heterogeneity, also called "frailty" in survival analysis, was estimated. This model takes into account that subjects who are more "frail" will experience earlier exits, possibly leading to a selected sample of survivors. The hazard rate for this analysis is modeled as follows:

$$h\left(t|treat_i\right) = h_0\left(t\right)\alpha_i \exp\left(\beta\, treat_i\left(t\right)\right), \tag{3}$$

where the subject-specific frailty term α_i is assumed to follow a Gamma distribution, and the treatment $treat_i$ is allowed to vary over time. Hazard functions as in Eq. 3 were estimated with CR-IPTW models, using the same sample and matching weights of the analysis with time-varying treatments and time-constant PS discussed in the previous check: in this case individual heterogeneity is allowed for exploiting information from multiple-record spells. Standard errors for hazard ratios were bootstrapped using 200 replications instead of 2000 as in the rest of the analysis, because of substantially larger computational time; (Efron and Tibshirani (1993), pp. 52) suggest that 200 replications are generally sufficient for estimating standard errors. Results are reported in Table 17, and show that estimates are only marginally affected by this correction.

5.5 Search methods and post-unemployment outcomes

This section reports additional evidence on the relation between search methods and post-unemployment outcomes. Three measures of quality of subsequent employment were considered as outcomes: net monthly wage, job tenure, and type of contract (permanent vis-à-vis temporary). Estimates were carried out by linear models with IPTW-PSM, using same matching weights as in the unemployment duration analysis presented in Section 5.3.

Table 6 reports estimates of average treatment effects for the total sample of job inflows, and for the two sub-samples of local and non-local inflows separately. Results show that DAE has positive effect on wage but no significant effects on job tenure and probability of permanent contract. The wage effect of DAE is also significant in both sub-samples, with a much larger size for non-local exits. SOCNET has no significant effects on wages, but positive effects on the two measures of job stability; effects on job stability are significant also for local jobs, but not for non-local jobs. No significant effects on any employment outcomes were found for EA and ADS.

Existing evidence on the effects of DAE on wage and job stability is rather mixed (see Section 2.1). It is typically thought that making contact with and applying to firms directly without information or referrals from employed contacts may generate a relatively high number of job offers (Weber and Mahringer 2008), however it may be costly (Holzer 1988) and associated to a larger risk of bad match. Therefore it is possible that wages required for job offers generated in this way are larger to compensate for such costs. Along this line of interpretation, evidence in Table 6 of a positive wage effect in combination with no effect on job stability may reflect higher wage demand rather than increased productivity of job match. This interpretation is also in line with the greater wage effect for non-local jobs, since uncertainty about job match and other possible search costs may be larger in this case. However, the positive impact on the mobility hazard documented in Section 5.3 may suggest that DAE can generate a relatively large number of offers for this type of jobs, outweighing the

Table 6 Effect of search methods on post-unemployment outcomes. Propensity Score Matching Estimates with Inverse Probability of Treatment Weighting

	Wage (log.)		
	Total	Non-local job	Local job
DAE	0.176***	0.365*	0.148**
	(2.911)	(1.953)	(2.268)
Nr. of observations	872	94	778
EA	0.021	−0.053	0.032
	(0.367)	(−0.380)	(0.538)
Nr. of observations	888	104	784
SOCNET	0.078	0.042	0.091
	(1.322)	(0.406)	(1.357)
Nr. of observations	886	103	783
ADS	−0.042	−0.074	−0.027
	(−0.584)	(−0.480)	(−0.373)
Nr. of observations	846	98	748
	Employment duration (log.)		
	Total	Non-local job	Local job
DAE	0.020	0.133	−0.016
	(0.204)	(0.493)	(−0.149)
Nr. of observations	900	94	806
EA	−0.008	0.051	−0.011
	(−0.075)	(0.439)	(−0.095)
Nr. of observations	917	104	813
SOCNET	0.196**	0.009	0.241**
	(2.131)	(0.066)	(2.443)
Nr. of observations	916	103	813
ADS	−0.004	−0.070	0.017
	(−0.034)	(−0.294)	(0.147)
Nr. of observations	875	98	777
	Permanent contract		
	Total	Non-local job	Local job
DAE	−0.022	−0.042	−0.025
	(−0.595)	(−0.581)	(−0.634)
Nr. of observations	838	88	750
EA	0.001	−0.003	0.000
	(0.023)	(−0.036)	(0.008)
Nr. of observations	853	98	755
SOCNET	0.134***	0.011	0.158***
	(3.318)	(0.134)	(3.623)

Table 6 (continued)

	Wage (log.)		
	Total	Non-local job	Local job
Nr. of observations	850	97	753
ADS	0.014	−0.051	0.027
	(0.211)	(−0.611)	(0.378)
Nr. of observations	809	92	717

Notes: * significant 10%, ** significant 5%, *** significant 1%. t-statistics in parenthesis have been calculated by bootstrapping standard errors with 2000 replications. Estimates have been performed by OLS on the treatment indicator, where weights have been computed with a propensity score-based IPTW algorithm. Separate models have been estimated for each search method on specific common support samples. See Table 3 for estimates of treatment selection equations

effect of higher wage demand. Overall evidence suggests that, while DAE may lead to non-local employment earlier and with higher remuneration, these returns may not reflect productivity gains.

Findings on the effects of SOCNET reported in Table 6 are generally in line with existing evidence. In fact, network connections are typically found to enhance job finding rates and job stability, but effects on wage are generally mixed. The finding that SOCNET influences positively job finding and employment stability only at the local level appears consistent with the role of geographical proximity in enhancing information flows in the job personal network (Bayer et al. 2008; Hellerstein et al. 2011; Hellerstein et al. 2014; Schmutte 2015). In fact, since social contacts are thought to facilitate job matching either through referrals or through information exchange about employers, it is possible that geographical distance between the relevant nodes may weaken such mechanisms, attenuating the moderating effect on job match uncertainty. Therefore, the present findings suggest that arguments raised in the literature to explain positive employment outcomes of SOCNET may operate only at the local level.

A number of arguments have been raised in the literature to explain mixed wage effects of social contacts, even in combination with positive employment outcomes. First, Loury (2006) suggests that social contacts can either lead to jobs with longer tenure and higher wage, indicating a better match, or to jobs with longer tenure and lower wage, indicating a limited access to alternative job offers; in the latter case, SOCNET may be used as a last resort because other channels may not be available. Therefore, the null wage effect documented in Table 6, together with positive effect on job stability, may indicate a combination of the two scenarios. Second, Bentolila et al. (2010) suggest that SOCNET can lead to easier access to job at the expense of lower wage, because job found through a social contact may match more contact's than worker's characteristics. Third, Pellizzari (2010) finds relevant cross-country and cross-industry variation in the wage premium of jobs found through social contacts, and that a large fraction of this variability is explained by the efficiency of formal recruitment strategies: when formal recruitment strategies are more efficient, firms are more selective with candidates met through social networks, and hence pay relatively lower wages.

6 Conclusions

This paper investigated the effect of job search methods on regional mobility of unemployed workers. A competing-risks model for the duration of unemployment was estimated using data from the British Household Panel Survey for the period 1996–2008. Exits to employment were decomposed between exits within and across local labour markets, using Travel-to-Work-Areas as definition of local labour markets. The effect of each search method on competing hazards was estimated separately by a Cox proportional hazard model, in combination with a Propensity Score Matching (PSM) approach.

Results show that social contacts (SOCNET) and advertisements (ADS) enhance the probability to find job locally, but have no effect on the probability to find job outside the local labour market. Conversely, direct approach to employers (DAE) enhances exits to non-local employment, but has no effect on local exits. Employment agencies (EA) are ineffective for both employment hazards. When allowing for time-varying effects over the unemployment spell, an opposite trend was found for the local hazard between SOCNET and ADS, with SOCNET declining in impact and becoming ineffective after one year of unemployment, and ADS becoming more effective. This finding suggests that social contacts may be approached sequentially by productivity and may become increasingly composed by unemployed workers (Bramoullé and Saint-Paul 2010). At the same time, the decline in the productivity of social contacts may be compensated by a more efficient use of ADS.

An analysis of post-unemployment outcomes revealed that SOCNET has also a positive impact on the stability of local jobs, although no effects were found on the stability of non-local jobs and on wages. Such evidence appears consistent with the influence of geographical proximity on information exchange in the job network (Bayer et al. 2008; Hellerstein et al. 2011; Hellerstein et al. 2014; Schmutte 2015), and suggests that the well-documented efficacy of social connections in promoting employment stability may work at the local level only. Failure to observe a positive wage impact of SOCNET may be explained by firms being more selective with candidates met through formal networks (Pellizzari 2010), but may also indicate limited access to more productive search channels (Loury 2006), or a trade-off between easier access to job and match quality (Bentolila et al. 2010). Overall, the present analysis suggests that SOCNET is an effective method for accessing and maintaining local employment, although such employment may not correspond to a more productive match.

The evidence presented in this paper rests on the validity of the Conditional Independence Assumption (CIA) required by the PSM method. CIA states that assignment to search methods should be as if randomized once bias arising by observed covariates is eliminated. While the reliability of this assumption is sustained in the present analysis by a large and comprehensive set of predictors, CIA may not hold if relevant factors affecting the choice of search method and outcomes are omitted. Therefore, potential presence of residual confounding factors should be kept in mind in interpreting results, and implications should be taken with caution. Future research controlling for unobserved factors would be opportune to backup the present evidence. Availability of instrumental variables may solve this possible endogeneity. Ideally, randomized experiments would be needed for understanding in a definite way the influence of job search methods on matching frictions (Topa 2019).

The findings of this paper enrich existing evidence on the effect of job search methods on unemployment exits by establishing a link with regional mobility. This novel approach contributes to knowledge on regional labour markets in two key ways. First, the distinction between local and non-local exits provides a new regional perspective to assess efficiency in the selection of search methods. When mobility is allowed for, "regional efficiency" requires that workers with higher mobility costs allocate relatively more (respectively less) effort to methods that are conducive to local (respectively non-local) employment. Therefore, the present results suggest that less mobile workers should use relatively more often SOCNET and ADS, while more mobile workers should use relatively more often DAE. These findings can complement the prominent body of literature investigating the influence of homeownership on labour market outcomes. This literature suggests that homeowners concentrate search effort in the local labour market to avoid relocation, with an overall negative effect on search intensity; however, they are often found to escape unemployment earlier, possibly because they search more efficiently (Munch et al. 2006; Morescalchi 2016). Since treatment model estimates reported that homeowners are more likely to use SOCNET and ADS than private renters, the present evidence suggests overall that homeowners allocate search effort efficiently in a regional perspective. A similar argument may apply also to social renters, since they face mobility constrains similarly to homeowners, and have similar propensity to use SOCNET and ADS.

Second, the findings of this paper provide a guidance to policy makers aiming to attenuate frictions in the job matching process, in particular by lessening impediments to regional mobility. Evidence that social contacts enhance job matching only in the local labour market suggests that the widespread use of this method might contribute to restrict labour mobility, bearing support to policies weakening the relative importance of family and social ties in the search process (Bentolila et al. 2010). In addition, potentiating alternative search strategies appears particularly relevant when the goal is supporting job search of long-term unemployed, given the relatively fast decline in the productivity of social contacts. These shortcomings add up to a number of unintended consequences of this search method already mentioned in the literature (Topa 2019). Moreover, evidence that the widely used Employment Agencies are ineffective for both local and national jobs urges policy makers to consider reforms of the counseling and placement service offered by the Agencies, having in mind these shortcomings. The following two instruments are supported by the present evidence: (i) a nationwide placement system allowing the Public Employment Service to monitor job vacancies throughout the country (Gregg et al. 2004; Bonin et al. 2008); (ii) instruments promoting integration of mobile workers and their families, such as supporting the job search of spouses (Bonin et al. 2008).

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Appendix A: Description of variables

This section lists and describes the variables used in estimation.

– Search methods dummies. DAE: applied directly to an employer. ADS: studied or replied to advertisements. EA: contacted a private employment agency or Job Centre. SOCNET: asked friends or contacts. SEMP: took steps to start own business.
– Age. 4 age bands: 16–24, 25–34, 35–44, 45–65.
– Female. Binary variable identifying females.
– Unemployment benefit. Binary variable identifying whether the individual has received unemployment benefit or income support as an unemployed person in the last year.
– Highest education. Categorical variable identifying the highest educational qualifications, with the following states: No qualifications; O Levels or equivalent; A Levels of equivalent; nursing and other qualifications; first degree or above (including teaching).
– Marital status. Binary variable identifying married people (or living as a couple).
– Children 0–15 years. Binary variable indicating whether the individual has own children under age of 16 in the household.
– Housing tenure. Categorical variable identifying the following categories: homeowners; social renters; private renters.
– Last wage. Monthly net wage earned in the last job. Expressed in 2008 real GBP. Missing cases were imputed by estimating a wage equation with the count of search methods and unemployment duration in addition to covariates used in the analysis.
– Reservation wage. Self-reported amount in response to the following question: "What is the lowest weekly take-home pay you would consider accepting for a job?" Normalized to monthly value and expressed in 2008 real GBP. Missing cases were imputed similarly to last wage.
– Employment growth. Growth rate (%) in employment at the Local Authority District level. This information was not available for LADs of Northern Ireland for the period under investigation, hence the national value was used. The series is drawn from Nomis, Office for National Statistics (ONS), UK (www.nomisweb.co.uk).
– Last job occupation. Defined by the 1990 Standard Occupational Classification (SOC), with the following possible categories: managers and administrators; professional, associate professional and technical occupations; clerical and secretarial occupations; craft and related occupations; personal and protective

service occupations; sales occupations; plant and machine operatives; other occupation; no previous job.

- Last job industry sector. Industrial sectors are defined using the 1992 Standard Industrial Classification (SIC). SIC 1992 is divided in the following sectors: (A) Agriculture, Hunting and Forestry; (B) Fishing; (C) Mining and Quarrying; (D) Manufacturing; (E) Electricity, Gas and Water Supply; (F) Construction; (G) Wholesale and Retail Trade: Repair of Motor Vehicles, Motorcycles and Personal Household Goods; (H) Hotels and Restaurants; (I) Transport, Storage and Communication; (J) Financial Intermediation; (K) Real Estate, Renting and Business Activities; (L) Public Administration and Defence: Compulsory Social Security; (M) Education; (N) Health and Social Work; (O) Other Community, Social and Personal Service Activities; (P) Private Households with Employed Persons; (Q) Extra-Territorial Organisations and Bodies. For the present analysis, the following categories have been aggregated into a residual category called "Others", due to their limited representation: (A), (B), (C), (E), (J), (L), (P) and (Q). For waves before the 12th, industry sector is recorded with the 1980 classification, therefore codes have been converted to the 1992 classification using Jennifer Smith's one-to-one mapping. The table is downloadable at https://www2.warwick.ac.uk/fac/soc/economics/staff/jcsmith/sicmapping/resources/direct/.
- Region dummies. Defined as follows: Yorkshire and Humberside, and North East; North-West; Midlands; East Anglia; South East; South West; Wales; Scotland; Northern Ireland.
- Year dummies. 1996–2008

Appendix B: Full results for treatment selection models

Table 7 Treatment selection models. Full set of coefficients for estimates in Table 3

	DAE	EA	SOCNET	ADS
DAE		0.039	0.544***	0.576***
		(0.500)	(7.314)	(7.263)
EA	0.046		0.213***	0.471***
	(0.580)		(2.722)	(5.545)
SOCNET	0.551***	0.209***		0.120
	(7.409)	(2.702)		(1.434)
ADS	0.586***	0.471***	0.120	
	(6.932)	(5.407)	(1.366)	
SEMP	−0.009	0.101	0.270**	0.153
	(−0.071)	(0.795)	(2.090)	(1.090)
Age 25—34	−0.251**	−0.227**	−0.071	0.084
	(−2.518)	(−2.218)	(−0.728)	(0.766)
Age 35—44	−0.313***	−0.186	−0.134	0.219*
	(−2.711)	(−1.544)	(−1.171)	(1.712)

Table 7 (continued)

	DAE	EA	SOCNET	ADS
Age 45—64	−0.461***	−0.241**	−0.193*	0.223*
	(−4.184)	(−2.132)	(−1.748)	(1.857)
Female	−0.133	−0.226***	−0.097	0.150
	(−1.634)	(−2.711)	(−1.199)	(1.618)
Unemployment benefit	−0.070	0.394***	−0.090	0.190**
	(−0.942)	(5.260)	(−1.193)	(2.390)
Reservation wage (log.)	−0.011	−0.119	−0.118	−0.160*
	(−0.147)	(−1.316)	(−1.599)	(−1.736)
Last wage (log.)	0.100*	0.045	−0.005	−0.039
	(1.669)	(0.744)	(−0.084)	(−0.627)
O Levels or equivalent	0.047	0.067	0.052	0.131
	(0.482)	(0.651)	(0.521)	(1.274)
A Levels or equivalent	0.104	−0.005	0.090	0.211*
	(0.866)	(−0.038)	(0.737)	(1.649)
Nursing and other	0.150	0.016	−0.059	0.405***
	(1.435)	(0.145)	(−0.558)	(3.546)
1st degree or above	0.473***	−0.205	−0.105	0.551***
	(3.587)	(−1.583)	(−0.816)	(3.721)
Married	0.192**	0.063	−0.025	0.105
	(2.134)	(0.691)	(−0.291)	(1.033)
Children 0—15 years	0.144	−0.031	0.092	−0.088
	(1.421)	(−0.297)	(0.918)	(−0.776)
Social renter	−0.110	−0.083	0.015	−0.036
	(−1.312)	(−0.955)	(0.174)	(−0.396)
Private renter	0.131	0.069	−0.206*	−0.254**
	(1.179)	(0.615)	(−1.919)	(−2.167)
Employment growth	0.004	0.007	−0.002	0.003
	(0.568)	(1.037)	(−0.290)	(0.437)
Managers, administrators	0.031	0.186	0.232	0.157
	(0.164)	(0.957)	(1.224)	(0.774)
Professional, technical	0.175	−0.038	0.240	0.241
	(0.901)	(−0.192)	(1.241)	(1.100)
Clerical, secretarial	0.137	0.312*	−0.065	0.141
	(0.877)	(1.907)	(−0.431)	(0.843)
Craft & related	0.175	0.305*	0.437***	−0.015
	(1.024)	(1.724)	(2.618)	(−0.086)
Personal, protective	0.012	−0.027	0.249	−0.038
	(0.070)	(−0.151)	(1.401)	(−0.201)

Table 7 (continued)

	DAE	EA	SOCNET	ADS
Sales	0.052	0.115	0.559***	0.404**
	(0.294)	(0.638)	(3.108)	(1.969)
Plant, machine operatives	0.302*	0.155	0.343**	0.081
	(1.715)	(0.886)	(2.049)	(0.449)
Other occupation	0.008	0.100	0.316**	0.158
	(0.061)	(0.756)	(2.470)	(1.163)
Manufacturing	−0.031	−0.100	0.087	−0.052
	(−0.259)	(−0.818)	(0.749)	(−0.416)
Construction	−0.137	0.029	0.437**	−0.162
	(−0.694)	(0.142)	(2.009)	(−0.841)
Wholesale, retail, motors	0.105	0.038	0.128	0.346*
	(0.725)	(0.259)	(0.848)	(1.829)
Hotels, restaurants	0.353**	−0.120	−0.002	0.065
	(2.170)	(−0.791)	(−0.015)	(0.364)
Transport, etc.	0.065	0.068	0.297	0.010
	(0.342)	(0.369)	(1.627)	(0.053)
Estate, renting, business	0.063	0.161	0.066	0.133
	(0.462)	(1.099)	(0.487)	(0.849)
Education	−0.177	−0.106	0.177	0.455
	(−0.849)	(−0.480)	(0.808)	(1.615)
Health, social work	0.080	0.445**	0.112	−0.114
	(0.405)	(2.182)	(0.572)	(−0.514)
Other social	−0.156	−0.252	0.146	0.079
	(−0.818)	(−1.298)	(0.749)	(0.364)
North-West	−0.123	−0.143	−0.065	−0.027
	(−0.784)	(−0.926)	(−0.424)	(−0.161)
E. and W. Midlands	−0.303**	−0.167	−0.269*	0.246
	(−2.077)	(−1.152)	(−1.926)	(1.588)
East Anglia	−0.270*	−0.183	0.051	0.054
	(−1.810)	(−1.199)	(0.340)	(0.344)
South East	−0.105	−0.269*	−0.077	0.293*
	(−0.689)	(−1.728)	(−0.508)	(1.712)
South West	0.037	0.212	0.280	0.214
	(0.197)	(1.095)	(1.519)	(1.062)
Wales	−0.258*	0.087	0.069	−0.003
	(−1.839)	(0.602)	(0.504)	(−0.020)
Scotland	−0.130	0.152	0.051	0.214
	(−0.936)	(1.063)	(0.380)	(1.475)

Table 7 (continued)

	DAE	EA	SOCNET	ADS
Northern Ireland	−0.032	−0.635***	−0.098	0.580***
	(−0.188)	(−3.719)	(−0.585)	(2.935)
Year 1997	−0.142	0.008	0.005	0.343*
	(−0.764)	(0.040)	(0.025)	(1.671)
Year 1998	−0.145	0.086	−0.167	0.070
	(−0.752)	(0.433)	(−0.868)	(0.350)
Year 1999	0.006	0.169	−0.075	0.494**
	(0.032)	(0.833)	(−0.388)	(2.283)
Year 2000	0.079	0.122	−0.113	0.175
	(0.432)	(0.656)	(−0.618)	(0.910)
Year 2001	−0.087	0.033	−0.017	0.173
	(−0.514)	(0.191)	(−0.097)	(0.982)
Year 2002	−0.216	0.112	0.277	0.233
	(−1.197)	(0.618)	(1.482)	(1.213)
Year 2003	−0.044	0.306	−0.045	0.039
	(−0.242)	(1.602)	(−0.247)	(0.204)
Year 2004	−0.112	0.247	0.027	0.200
	(−0.602)	(1.288)	(0.142)	(1.002)
Year 2005	0.058	−0.038	−0.111	0.133
	(0.315)	(−0.205)	(−0.601)	(0.698)
Year 2006	−0.042	0.132	−0.094	0.188
	(−0.237)	(0.724)	(−0.523)	(1.006)
Nr. of observations	1656	1656	1656	1656

Notes: * significant 10%, ** significant 5%, *** significant 1%. t-statistics in parenthesis have been calculated with robust standard error

Appendix C: Diagnostics on PS matching procedure

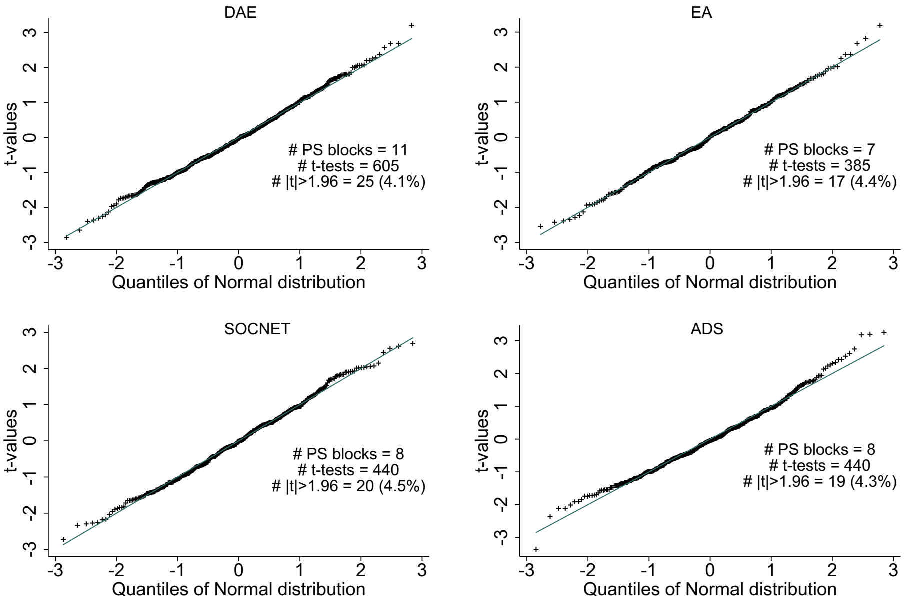

Fig. 2 QQ plots for difference-in-means t-tests within-PS blocks. Notes: Plots report t-statistics of covariate difference-in-means tests by treatment status against the corresponding quantiles of the Normal distribution. Tests were performed within blocks of the PS with no significant differences in PS means. Tests behave approximately as if they were independent draws from a Normal distribution

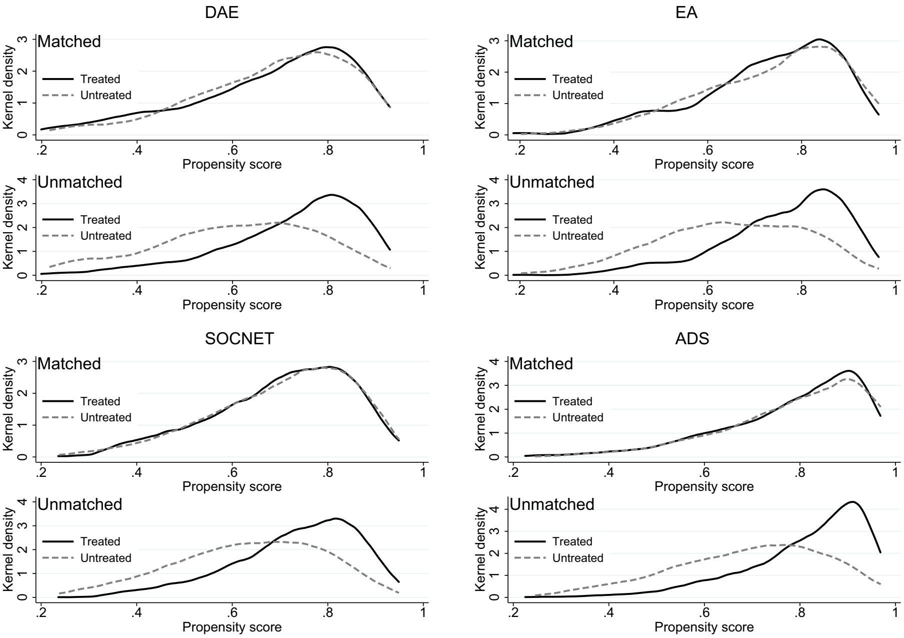

Fig. 3 Common support region. Overlap check between PS distributions

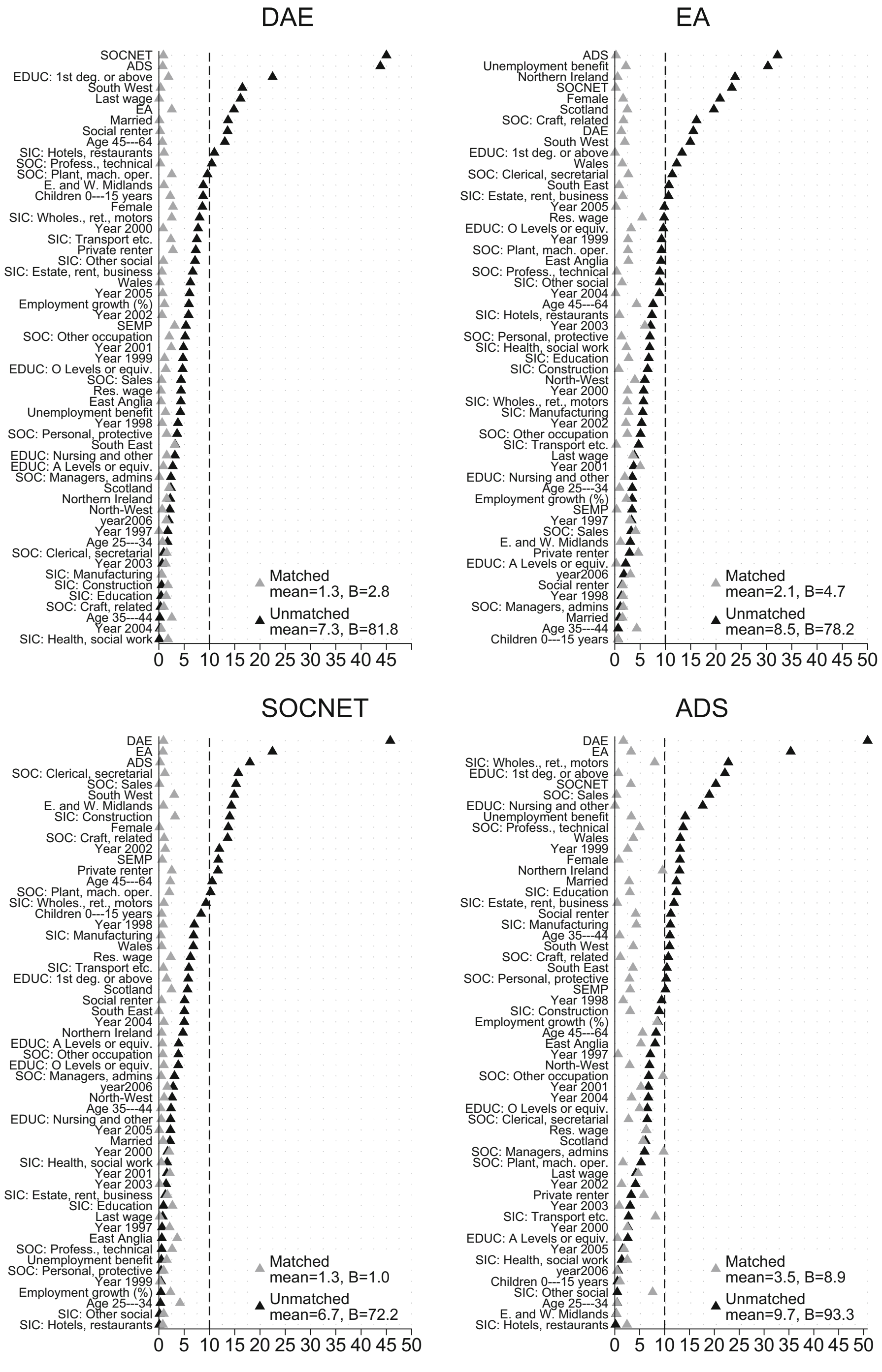

Fig. 4 Standardized differences (%) in covariate means between treated and untreated; suggested cutoff is 10% (Austin and Stuart 2015) . Rubin's B suggested cutoff is 25% (Rubin 2001)

Appendix D: Estimates of competing-risks unemployment duration model without matching

Table 8 Effect of search methods on cause-specific hazards. Unmatched sample

	Non-local job	Local job	OLF
DAE	1.709	0.926	0.828
	(1.605)	(−1.093)	(−1.319)
EA	0.900	1.076	1.071
	(−0.480)	(1.002)	(0.441)
SOCNET	1.195	1.485***	1.001
	(0.703)	(5.321)	(0.008)
ADS	1.342	1.185**	1.017
	(0.888)	(2.036)	(0.099)
SEMP	1.343	0.918	0.700
	(0.971)	(−0.756)	(−1.219)
Age 25—34	0.554**	0.805**	0.771
	(−2.140)	(−2.342)	(−1.253)
Age 35—44	0.186***	0.814*	0.727
	(−3.196)	(−1.870)	(−1.366)
Age 45—64	0.113***	0.612***	0.760
	(−4.603)	(−4.689)	(−1.312)
Female	1.113	1.068	1.961***
	(0.431)	(0.883)	(4.216)
Unemployment benefit	0.882	0.662***	0.545***
	(−0.553)	(−5.834)	(−3.919)
Reservation wage (log.)	0.911	0.877**	0.731***
	(−0.420)	(−2.112)	(−2.930)
Last wage (log.)	2.035***	1.527***	1.169
	(3.242)	(5.197)	(1.075)
O Levels or equivalent	1.620	1.326***	0.945
	(1.180)	(3.101)	(−0.288)
A Levels or equivalent	1.581	1.365***	1.211
	(1.010)	(2.861)	(0.815)
Nursing and other	2.368**	1.146	1.098
	(2.087)	(1.345)	(0.467)
1st degree or above	2.641**	1.054	1.696**
	(2.316)	(0.435)	(2.098)
Married	1.416	1.056	1.012
	(1.402)	(0.650)	(0.072)
Children 0—15 years	0.705	0.981	1.016
	(−1.099)	(−0.206)	(0.082)

Table 8 (continued)

	Non-local job	Local job	OLF
Social renter	0.366***	0.802***	0.804
	(−2.686)	(−2.803)	(−1.266)
Private renter	2.045***	0.717***	1.085
	(2.804)	(−2.928)	(0.395)
Employment growth	0.976	0.992	0.996
	(−0.947)	(−1.266)	(−0.347)
Occupation	✓	✓	✓
Sector	✓	✓	✓
Region	✓	✓	✓
Year	✓	✓	✓
Nr. of observations	1656	1656	1656

Notes: * significant 10%, ** significant 5%, *** significant 1%. Reported coefficients are hazard ratios. t-statistics in parenthesis have been calculated with robust standard errors. Estimates have been performed by a Cox proportional hazard model

Appendix E: Robustness Checks

Table 9 Effect of search methods on cause-specific hazards. Propensity Score Matching Estimates with Inverse Probability of Treatment Weighting. The common support has been restricted to the range .05–.95 of the Propensity Score

	Non-local job	Local job	OLF
DAE	1.912**	0.927	0.876
	(1.967)	(−1.176)	(−0.935)
Nr. of observations	1623		
EA	0.953	1.052	1.054
	(−0.194)	(0.724)	(0.330)
Nr. of observations	1635		
SOCNET	0.959	1.313***	0.947
	(−0.174)	(3.847)	(−0.388)
Nr. of observations	1645		
ADS	0.978	1.260**	0.943
	(−0.062)	(2.417)	(−0.324)
Nr. of observations	1489		

Notes: * significant 10%, ** significant 5%, *** significant 1%. Reported coefficients are hazard ratios. t-statistics in parenthesis have been calculated by bootstrapping standard errors with 2000 replications. Estimates have been performed by a weighted Cox proportional hazard model on the treatment indicator, where weights have been computed with a propensity score-based IPTW algorithm. Separate models have been estimated for each search method on specific common support samples. See Table 3 for estimates of treatment selection equations

Table 10 Effect of search methods on cause-specific hazards. Propensity Score Matching Estimates with Nearest Neighbour Matching (radius algorithm on caliper 0.05)

	Non-local job	Local job	OLF
DAE	2.508**	0.917	0.946
	(2.537)	(−1.297)	(−0.342)
Nr. of observations	1623		
EA	0.922	1.060	1.108
	(−0.309)	(0.797)	(0.582)
Nr. of observations	1650		
SOCNET	1.022	1.302***	0.937
	(0.088)	(3.481)	(−0.443)
Nr. of observations	1645		
ADS	0.931	1.256**	0.981
	(−0.215)	(2.203)	(−0.095)
Nr. of observations	1582		

Notes: * significant 10%, ** significant 5%, *** significant 1%. Reported coefficients are hazard ratios. t-statistics in parenthesis have been calculated by bootstrapping standard errors with 2000 replications. Estimates have been performed by a weighted Cox proportional hazard model on the treatment indicator, where weights have been computed with a nearest neighbour matching algorithm. Separate models have been estimated for each search method on specific common support samples. See Table 3 for estimates of treatment selection equations

Table 11 Effect of search methods on cause-specific hazards. Propensity Score Matching Estimates with Inverse Probability of Treatment Weighting. Time window for moves is between 6 months before and 12 months after exit

	Non-local job	Local job	OLF
DAE	1.781**	0.927	0.876
	(1.973)	(−1.110)	(−0.916)
Nr. of observations	1623		
EA	0.976	1.064	1.084
	(−0.108)	(0.862)	(0.489)
Nr. of observations	1650		
SOCNET	0.964	1.324***	0.947
	(−0.164)	(3.850)	(−0.384)
Nr. of observations	1645		
ADS	0.996	1.252*	0.993
	(−0.011)	(1.954)	(−0.032)
Nr. of observations	1582		

Notes: * significant 10%, ** significant 5%, *** significant 1%. Reported coefficients are hazard ratios. t-statistics in parenthesis have been calculated by bootstrapping standard errors with 2000 replications. Estimates have been performed by a weighted Cox proportional hazard model on the treatment indicator, where weights have been computed with a propensity score-based IPTW algorithm. Separate models have been estimated for each search method on specific common support samples. See Table 3 for estimates of treatment selection equations

Table 12 Effect of search methods on cause-specific hazards. Propensity Score Matching Estimates with Inverse Probability of Treatment Weighting. Time window for moves is between 9 months before and 12 months after exit

	Non-local job	Local job	OLF
DAE	1.703*	0.918	0.876
	(1.959)	(−1.246)	(−0.916)
Nr. of observations	1623		
EA	0.988	1.074	1.084
	(−0.054)	(0.962)	(0.489)
Nr. of observations	1650		
SOCNET	0.921	1.338***	0.947
	(−0.393)	(3.926)	(−0.384)
Nr. of observations	1645		
ADS	1.023	1.259**	0.993
	(0.073)	(1.977)	(−0.032)
Nr. of observations	1582		

Notes: * significant 10%, ** significant 5%, *** significant 1%. Reported coefficients are hazard ratios. t-statistics in parenthesis have been calculated by bootstrapping standard errors with 2000 replications. Estimates have been performed by a weighted Cox proportional hazard model on the treatment indicator, where weights have been computed with a propensity score-based IPTW algorithm. Separate models have been estimated for each search method on specific common support samples. See Table 3 for estimates of treatment selection equations

Table 13 Effect of search methods on cause-specific hazards. Propensity Score Matching Estimates with Inverse Probability of Treatment Weighting. Local Labour Markets are defined by Local Authority Districts

	Non-local job	Local job	OLF
DAE	2.004**	0.913	0.876
	(2.302)	(−1.387)	(−0.916)
Nr. of observations	1623		
EA	1.082	1.047	1.084
	(0.330)	(0.634)	(0.489)
Nr. of observations	1650		
SOCNET	1.134	1.289***	0.947
	(0.511)	(3.481)	(−0.384)
Nr. of observations	1645		
ADS	0.716	1.332***	0.993
	(−1.068)	(2.578)	(−0.032)
Nr. of observations	1582		

Notes: * significant 10%, ** significant 5%, *** significant 1%. Reported coefficients are hazard ratios. t-statistics in parenthesis have been calculated by bootstrapping standard errors with 2000 replications. Estimates have been performed by a weighted Cox proportional hazard model on the treatment indicator, where weights have been computed with a propensity score-based IPTW algorithm. Separate models have been estimated for each search method on specific common support samples. See Table 3 for estimates of treatment selection equations

Table 14 Effect of search methods on cause-specific hazards. Propensity Score Matching Estimates with Inverse Probability of Treatment Weighting. An additional weighting factor was used to take into account unobserved spells

	Non-local job	Local job	OLF
DAE	2.063*	0.964	0.931
	(1.778)	(−0.535)	(−0.486)
Nr. of observations	1623		
EA	1.025	0.997	1.084
	(0.090)	(−0.028)	(0.475)
Nr. of observations	1650		
SOCNET	1.029	1.236**	0.911
	(0.103)	(2.534)	(−0.662)
Nr. of observations	1645		
ADS	0.761	1.273**	0.967
	(−0.782)	(1.976)	(−0.159)
Nr. of observations	1582		

Notes: * significant 10%, ** significant 5%, *** significant 1%. Reported coefficients are hazard ratios. t-statistics in parenthesis have been calculated by bootstrapping standard errors with 2000 replications. Estimates have been performed by a weighted Cox proportional hazard model on the treatment indicator, where weights have been computed with a propensity score-based IPTW algorithm. Separate models have been estimated for each search method on specific common support samples. See Table 3 for estimates of treatment selection equations

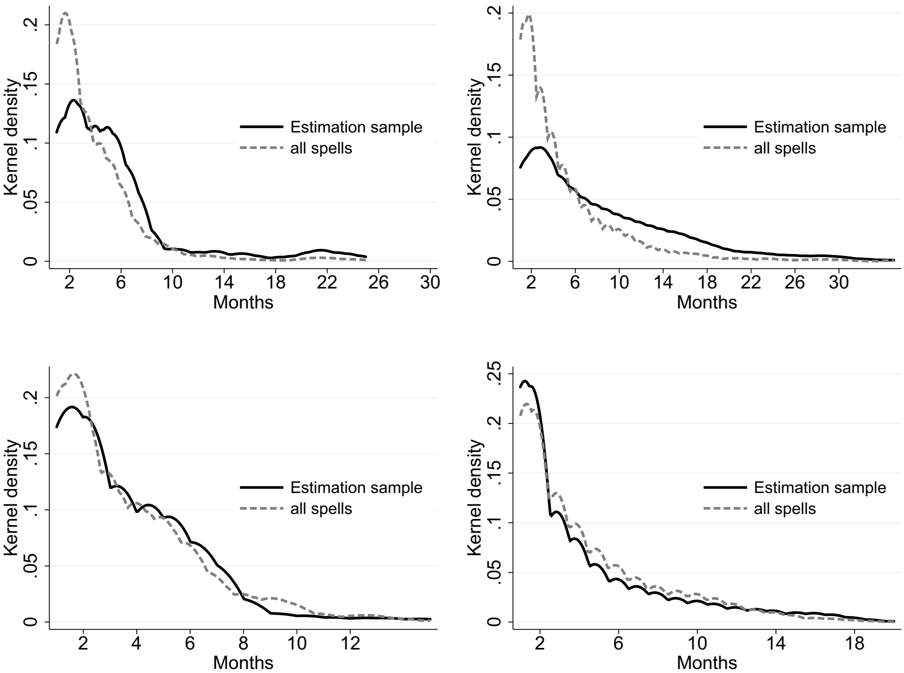

Fig. 5 Unemployment duration distribution. Comparison between estimation sample (cross-wave spells) and all spells (cross-wave and in between-waves spells)

Table 15 Effect of search methods on cause-specific hazards with time-varying treatment. Propensity Score Matching Estimates with Inverse Probability of Treatment Weighting

	Non-local job	Local job	OLF
DAE	2.140**	1.060	0.994
	(2.003)	(0.898)	(−0.041)
Nr. of observations	2038		
EA	1.112	1.049	1.235
	(0.344)	(0.648)	(0.934)
Nr. of observations	2042		
SOCNET	0.831	1.336***	0.999
	(−0.643)	(4.024)	(−0.006)
Nr. of observations	2059		
ADS	0.652	1.268**	1.284
	(−1.007)	(2.130)	(1.111)
Nr. of observations	1979		

Notes: * significant 10%, ** significant 5%, *** significant 1%. Reported coefficients are hazard ratios. t-statistics in parenthesis have been calculated by bootstrapping standard errors with 2000 replications. Estimates have been performed by a weighted Cox proportional hazard model on the treatment indicator, where weights have been computed with a propensity score-based IPTW algorithm. Separate models have been estimated for each search method on specific common support samples. The propensity score was estimated as the probability of using the given method any time during the spell conditional on initial values of covariates (Ali et al. 2013)

Table 16 Effect of search methods on cause-specific hazards with time-varying treatment and covariates. Marginal Structural Model estimates with Inverse Probability of Treatment Weighting

	Non-local job	Local job	OLF
DAE	2.292*	1.136	0.953
	(1.925)	(1.303)	(−0.215)
Nr. of observations	11346		
EA	1.130	0.980	0.957
	(0.214)	(−0.126)	(−0.104)
Nr. of observations	7844		
SOCNET	1.054	1.333***	1.141
	(0.167)	(2.735)	(0.601)
Nr. of observations	11247		
ADS	0.990	1.323**	1.331
	(−0.023)	(2.239)	(0.861)
Nr. of observations	8339		

Notes: * significant 10%, ** significant 5%, *** significant 1%. Reported coefficients are hazard ratios. t-statistics in parenthesis have been clculated with robust standard errors clustering at the individual level. Estimates have been performed by a weighted pooled logistic regression on data expanded in person-month form, where weights have been computed by cumulating IPTW weights over time (Hernán et al. 2000). Separate models have been estimated for each search method on specific common support samples

Table 17 Effect of search methods on cause-specific hazards with time-varying treatment and covariates. Unobserved heterogeneity is allowed for. Propensity Score Matching Estimates with Inverse Probability of Treatment Weighting

	Non-local job	Local job	OLF
DAE	2.142*	1.024	0.994
	(1.844)	(0.261)	(−0.039)
Nr. of observations	2038		
EA	1.114	1.077	1.235
	(0.388)	(0.829)	(1.187)
Nr. of observations	2042		
SOCNET	1.054	1.354***	0.998
	(0.226)	(4.773)	(−0.014)
Nr. of observations	2059		
ADS	0.759	1.267**	1.197
	(−0.712)	(2.281)	(0.650)
Nr. of observations	1979		

Notes: * significant 10%, ** significant 5%, *** significant 1%. Reported coefficients are hazard ratios. t-statistics in parenthesis have been calculated by bootstrapping standard errors with 2000 replications. Estimates have been performed by a weighted Cox proportional hazard model on the treatment indicator, where weights have been computed with a propensity score-based IPTW algorithm. Separate models have been estimated for each search method on specific common support samples. The propensity score was estimated as the probability of using the given method any time during the spell conditional on initial values of covariates (Ali et al. 2013)

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