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MARGINAL EMPLOYMENT AS AN INCENTIVE TO FIND A REGULAR JOB? A META-REGRESSION ANALYSIS APPROACH

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Marginal employment as an incentive to find a regular job? A meta-regression analysis approach[1]

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Abstract

In this article, we focus on the device that allows job seekers to combine marginal employment (part-time or short-full time job) to partial unemployment insurance benefits. Such a public policy exists in many OECD countries. Theoretical conclusions on its efficiency are inconclusive and the worldwide literature on its impact reveals contrasting effects. This paper reviews quantitatively this literature and propose a multivariate meta-regression analysis. Our database is composed of a large set of both articles written in the period 1999–2021. Considered articles differ according to several features (data sources and kind of job seekers, explanatory variables, econometric strategy applied, and the type of publication). In spite of heterogeneity of studies and of potential publication bias, marginal employment increases the probability to find a regular job by about 20 to 40 percentage points. This study thus highlights the importance of this device in helping job seekers exit unemployment, which encourage such approach to activate passive expenditure.

JEL Codes: *C21, C29, J65, J68.*

Keywords: *meta-regression analysis, unemployment benefits, short full-time / part time jobs, public policy.*

1. Introduction

This article focuses on the particular device that allows JS to practice short-time employment, combining corresponding wage with benefiting from unemployment insurance benefits.

Since the mi-1980s, there has been an increase in flexibilization of labor markets, in France, as well as in most European countries. One of the major consequences has been the explosion of "special forms of employment", like fixed-term contracts (FTC), temporary part-time work… Besides, during the economic crisis that began in 2008, there was a serious economic contraction of 4.7% between the first quarter of 2008 and the second quarter of 2009. A plunge in global trade was another sign of this economic situation. Worldwide, the volume of trade in goods and services fell by 12% in 2009, according to the WTO. In France like in other OECD countries, the part of job seekers who practice a part-time job, get the corresponding wage, while still being been registered to the (un-)employment agency, has doubled since the mid-90s. Its rise is greater than 80% since the beginning of the French Economic depression in 2008. At the end of 2015, one third of French 5.4 million job seekers where concerned by this device.

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We focus on this particular kind of activity, *i.e.* the possibility of a job seeker to practice a short-time job while still receiving partial unemployment benefits. This is a particular form of employment. Job seekers have the opportunity to return to a part-time work / short full-time work and retain a part of their unemployment benefits. These jobs should allow job seekers to find more rapidly full-time (*i.e.* regular) jobs, irrespective of / whatever their quality. This employment program has been adopted for a long time in many OECD countries, like in Austria, Belgium, Denmark, Finland, France, Germany, Norway, Switzerland, among other. Following the country under consideration, this device gets different names: atypical jobs; marginal employment (ME hereafter); mini-jobs; part-time unemployment; reduced activities; subsidized temporary jobs.

Thus, since the beginning of the 1990s, a growing related literature has aimed at studying to what extent this device allows job seekers to find a regular job more rapidly. From a theoretical point of view, its impact if ambiguous. First, the device can lead to an enclosing effect in a precarious trajectory, known as the "locking-in effect" because it can reduce the time spent in job search decrease the probability of getting a stable employment (for instance, Fontaine and Rochut, 2014; Fremigacci and Terracol, 2013; Gerfin and Lechner, 2005; Houseman *et al.*, 2003; Neugart and Storrie, 2002). Second, the device may have a beneficial effect on the return to permanent contract, known a "stepping-stone effect" (Boockmann and Hagen, 2008; Loh, 1994; Mc Cormick, 1991; Mc Call, 1996; Nagypal, 2001). On the other hand, there is a large strand of literature focuses on empirics dealing with that matter. Indeed, a large set of articles aimed at evaluating the impact of this device on finding a regular (full-time) job, and in particular: Auray and Lepage-Saucier (2021); Böheim and Weber (2010), Caliendo *et al.* (2016); Cockx *et al.* (2013); Eppel and Mahringer (2019); Fontaine and Rochut (2014); Freier and Steiner (2008); Fremigacci and Terracol (2013); Gerfin *et al.* (2005); Gilles and Isshenane (2017); Godoy and Roed (2016); Granier and Joutard (1999); Kyyrä (2010); Kyyrä *et al.* (2013); Lalive *et al.* (2008); Lietzman *et al.* (2017). As for the theoretical literature, the effect of this device is ambiguous. Some of this research papers report mainly a positive impact (Auray and Lepage-Saucier, 2021; Cockx *et al.*, 2013; Fontaine and Rochut, 2014; Godøy and Røed, 2016; Fremigacci and Terracol, 2013; Lietzmann *et al.*, 2017), whereas other report an average zero (Eppel and Mahringer, 2019; Freier and Steiner, 2008; Gerfin *et al.*, 2005; Granier and Joutard, 1999; Lalive *et al.*, 2008) or even negative (Böheim and Weber, 2010; Caliendo *et al.*, 2016; Kyyrä *et al.*, 2013). Hence, it is difficult to conclude as to the efficiency of ME in terms of finding a regular job for JS.

Hence there is no consensus on the right effect of ME on finding a regular employment (and thus going out of unemployment). However, when evaluating the impact of this type of program, there are at least two types of issues. First, there is a wide heterogeneity in the characteristics of the studies that estimate its effects, such as: the institutional context (countries); the kind of survey under consideration (average year, kind of JS surveyed); the type of marginal employment (short-full time, part-time work; industry where it takes place); timeline (date of entry in ME, horizon considered after ME to evaluate); individual features of JS for whom the effect is estimated (age, gender, education, qualification) and their past experience on job market (employability, benefit from UI); econometric identification (matching / timing of events; estimand); publication features (year; scope). Second, among all these studies, there may have been some potential publication bias. Publication bias may be defined as 'the consequence of choosing research papers for the statistical significance of their findings' (Stanley, 2008). For instance, Begg and Berlin (1988) show evidence for publication bias in the case of medical studies where sometimes they report systematic positive results, although no or negative effects are found but stay unpublished. More generally, published results may overstate or understate the true effect (Stanley and Jarrell 1989; Ashenfelter *et al.*, 1999; Doucouliagos and Stanley 2009; Havranek and Irsova 2011).

To cope with both problems, this article considers meta-regression analysis (MRA). It allows us to synthesize the existing empirical literature that deal with evaluating the effect of ME on finding a regular job. As an alternative approach to empirical studies, it provides a « quantitative » review of literature, exploiting the set of all articles in a given area of research. It proposes an answer to the famous critics of Leamer (1983) on econometrics, notably relied to the fragility of data / the necessity to present a large number of econometric specifications (explanatory variables introduced or not) to ensure robustness of the main results. This method is considered many fields of research (management, psychology…) and already used by researchers in medicine for many years. In economics, it was introduced at the end of the 80's mainly by Stanley and Jarrell (1989). In our case, MRA is a complementary analysis to evaluate the causal impact of ME on finding a regular job, while controlling for all sources of heterogeneity that characterize empirical studies dealing with that matter, as well as disentangling potential publication bias that may arise from the reviewing process from the true effect. Using results and features provided by 16 articles, we show evidence for no publication bias, but a large genuine empirical effect of ME on the probability for JS in going back to regular employment.

We contribute to the literature on four levels. First, we show that previous empirical studies have given rise to a large range of values of the estimated effect of ME on finding a regular job due to differences in the population studied, the explanatory variables included, the econometric strategy, data sources, and characteristics of the publications. Second, we test for publication bias in this literature on the causal impact of ME on regular employment. Third, we provide evidence of a genuine empirical effect of ME on regular employment, net of potential publication bias and heterogeneity of the studies. Fourth, in spite of a large range of values for estimated effect of ME for each study included in the file drawer – 12 out of 16 of them report both negative and positive –, the effect of ME on regular employment is rather large. This conclusion is a support maintaining this type of approach to activate passive expenditure.

The article is organized as follows. Section 2 presents definition of ME, as well as their expected effects. Section 3 displays the dataset considered in our meta-regression analysis. and shows heterogeneity in considered empirical studies. Section 4 displays some evidence on publication bias. Section 5 considers the multivariate meta-regression analysis framework to provide new evidence for the causal effect of marginal employment on regular employment for job seekers. Section 6 concludes.

2. Using marginal employment to boost regular employment? Definition and expected effects

In this Section, we introduce this special device and display expected effects from a theoretical point of view.

2.1. Definition and evolution

2.1.1. A program designed to encourage the return to work…

For a job seeker (JS), it consists in practicing a part-time or short-full time job, getting the corresponding wage, while been still registered and receiving partial unemployment insurance (UI) benefits. Initial rationale behind the creation of this scheme was that the occupation of part-time or short-term jobs by the unemployed could be a "stepping stone" towards stable employment. The aim was to ensure that

jobseekers were not put off by such low-paid or very short-term jobs, by making or very short-term jobs, by making it possible to combine earnings from employment and unemployment benefit.

This type of program exists in many OECD countries, where it has more or less similar characteristics and thus goes by various names: atypical jobs; marginal employment (ME); mini-jobs; part-time unemployment; reduced activities (RA); subsidized temporary jobs (STJ). [In France, for instance, such a device corresponds to practicing a part-time salaried activity while being still unemployed. It also defines the total or partial accumulation of unemployment benefit and salary.]

Marginal employment, designed to limit the risk of job seekers drifting away from employment, was conceived as an incentive to return to work. The aim is to counterbalance the disincentive effects of unemployment insurance. First it should allow avoiding that, without this system, compensation policies can make it unattractive for jobseekers receiving compensation to return to work. Second, by granting jobseekers the possibility of partially combining replacement income and salaried income, ME aims to reduce the potential incentive problems posed by unemployment insurance, and combat the formation of an "unemployment trap" by making jobs offering lower wages than replacement income acceptable to jobseekers.

This scheme is part of an approach to activate passive expenditure, and is clearly designed to encourage the unemployed on benefit to accept job offers - usually low-paid, part-time or short-term - while continuing their search for work. However, this system does not always result in a combination of unemployment benefit and salary: on the one hand, when the JS is not or is no longer receiving UI, but continues to be registered at the unemployment agency while being employed (on a part-time job); and on the other hand, when wage from activity is too large in relation to the number of hours worked / is too high in relation to the "cumulation" criteria for both UI and wages provided by part-time job – that are usually low-paid – while continuing their search for work.

2.1.2. … present in many countries around the world

This employment program has been adopted for a long time in many OECD countries. For instance, the unemployment insurance systems of USA, Canada, Germany, Austria, Netherlands, Finland, Norway, Denmark provide for the possibility of accumulating income from part-time or limited employment and at least a fraction of the compensation unemployment insurance benefits (Kyyrä *et al.,* 2017).

For most countries, the accumulation of UB and earned income is also conditioned by criteria of duration of activity and /or an earnings threshold:

- A threshold of 15 hours per week in Germany (Caliendo *et al.*, 2016);
- Part-time work of less than 80% or full-time work of less than 2 weeks in Finland (Kyyrä *et al.*, 2017);
- Less than 50 pct of the number of hours worked in the reference job in Norway (Godøy and Røed, 2016).

In general, the cumulation of income is generally partial. The unemployment benefit paid is reduced in the event of paid activity, with the exception of Germany and Finland, for example, where cumulation is total for very low wage income (respectively less than 165 and 300 euros per month from paid activity – Caliendo *et al.*, 2016, Kyyrä *et al.*, 2017). As in France, the reduction in benefits is often equal to a percentage of the wage provided by the marginal employment.

2.1.2. Evolution in the use of ME

Like other OECD countries adopting this type of scheme, the use of ME has grown considerably in France since its introduction.

The number of jobseekers in reduced activity more than tripled between 1996 and 2017, rising from 621,000 in January 1996 (16.6% of jobseekers) to 2,140,000 in September 2017 (36.2% of jobseekers). This evolution has been marked by both the economic climate and the transformation of the labor market. Within this general upward trend, there was a drop in the number of jobseekers in reduced activity between August 2005 and December 2008. This phenomenon is linked to the favorable economic climate, which led to a sharp drop in the total number of unemployed (3.842 million in August 2005 versus 3.055 million in May 2008). However, the part of jobseekers in work out of all the unemployed continued to rise during this period of favorable economic conditions (+5 percentage points). With the economic crisis of 2008, the effects of which were most strongly in France in 2009 and 2010, there was a very sharp rise in the total number of unemployed people, which mechanically increased the number of jobseekers who practice ME, albeit more moderately.

As a result of the crisis and the associated drop in hiring, the part of jobseekers working only increased from March 2009 onwards, before levelling off between mid-2011 and 2014, when there was a further increase in the number of unemployed people working.

2.2. Theoretical effects. Stepping stone or lock-in effect?

The main question related to this device is the following: to what extent does it allow job seekers to find a regular job more rapidly than if they would stayed full-time job seekers?

2.2.1. Stepping stone effect?

The device can have a beneficial effect on the return to regular job, known a “stepping-stone effect” since it may help to find another full-time (permanent) job.

Besides, there are determining factors to find a regular job. According to job search models, the individual probability of exiting unemployment depends on two factors (REF??): the receipt of job offers; and the individual decision to accept any offers received.

Practice of a part-time job while looking for work favors the receipt of job offers, by keeping individuals still “registered” as unemployed in a favorable work environment. Maintaining professional ties may be all the more important for the long-term unemployed, since direct since direct contact with potential employers often declines as the employers as the duration of unemployment increases (Mc Cormick, 1991).

In addition, by increasing the gains associated with returning to work the ME scheme can lead to an increase in individual job-seeking effort individual job-seeking effort, and thus the rate of job offer receipt (McCall, 1996).

The probability of acceptance of offers received by jobseekers may also be influenced by the marginal employment scheme. The decision to accept a job offer is based on a comparison of the wage offered for the job and the individual reservation wage. However, marginal employment makes it possible to limit the “erosion” of human capital caused by unemployment spells, and even to accumulate additional human capital. It can also enable jobseekers to show their motivation and employability to future employers. All these factors will help to increase the wages offered and consequently increase the acceptance rate of offers received (Alibay and Lefranc, 2003).

At the same time, temporary or part-time employment for jobseekers can help in limiting the growing socialization deficit and discriminatory socialization deficit, as well as discriminatory practices by companies against the long-term unemployed.

Marginal employment may also be seen as a recruitment tool. It is indeed possible that some employers use part-time job combined to UI benefits as a recruitment tool, so to ensure that the person they hire fits with the position they are looking to fill (Gerfin and Lechner, 2002; Houseman *et al.*, 2003; Neugart and Storrie, 2002).

For all these reasons, the ME could have a "springboard effect", accelerating the return to stable employment by providing recent experience that the jobseeker can capitalize on during her or his search (Boockmann and Hagen, 2008; Loh, 1994; Nagypal, 2001).

2.2.2. Lock-in effect?

On the other hand, the considered device can have a detrimental effect on the return to permanent contract, known a "locking-in effect" because it leads to an "enclosing effect" in an "employment" precarious trajectory on the job market.

In fact, ME reduces the time spent in job search and thus decreases the probability of getting a stable employment. Indeed, job search is not a passive activity, and requires a significant investment from the applicant to prospect, write CVs and cover letters, and attend job interviews. Theoretical models of job search show that the intensity of job search is a determining parameter in the speed with which jobseekers exit unemployment. If ME significantly impinges on the time a jobseeker devotes to job search, she of he may be penalized by a "lock-in effect" and take longer to find a stable job.

This effect can be all the stronger as *(i)* the combination of UI benefit and income from ME tends to increase the reserve wage of jobseekers, who may then consider this option more than full-time employment, and as *(ii)* the days of ME into additional days of UB entitlement may have the effect of the recipient to extend his or her job search period.

Since part-time jobs are more likely to be precarious, they can lead to recurrent spells of unemployment, which can increase the probability of subsequent transitions to unemployment. Thus, a series of short and/or part-time contracts can form a spiral that weakens the job seeker's situation (Huyghues Despointes *et al.*, 2001). According to Fontaine and Rochut (2014), "by making socially acceptable a situation in which work is low-paid or low-skilled job, the practice of ME would slow down the return to a job more in line with the jobseeker's real qualifications of the jobseeker", and would encourage people to occupy / keep precarious jobs, with more frequent transitions between employment and unemployment.

Otherwise, *r*educing the time available for research activity can also have a negative impact on the quality of work, encouraging relatively inefficient matches.

Finally, ME can leave jobseekers with little time to find the position most the job best suited to their profile, and may also dissuade them from taking training.

2.3. Methodological difficulties associated with evaluating the causal effect of ME

2.3.1. Problems

Empirical studies on the effects for JS of part-time jobs combined with UB on finding a regular job have emerged since the late 1990s, and have had to resolve several methodological problems.

First, they have to deal with controlling for selection bias. When measuring the impact of ME, authors have to consider practicing ME is often related to self-selection in the device (Rubin, 1974). Indeed, people who engage in salaried activity during their unemployment spell have specific characteristics and rationally adapt their behavior to different financial incentive mechanisms. Thus a (simple) difference in the outcome variable – the probability to go from unemployment to regular employment – between JS who practice ME and other (full-time) JS does not in general allow recovering the causal effect of ME.

Second, there is the dynamic nature of the device. Empirical studies have to consider two things. On the one hand, the causal effect of ME, which can occur at any time during the unemployment spell, probably depends on the length of time the person has been unemployed. We can imagine that taking up a ME as soon as you become unemployed will not have the same effect as taking up a reduced activity after six or twelve months of unemployment. On the other hand, ME may have a delayed impact on the return to employment. The causal effect expected during reduced activity or in the very short term is a priori different in the longer term.

2.3.2. Identifying the impact of ME: econometric models

Consequently, two kinds of models are considered, based on key different identification assumptions. Dynamic matching and Timing of events models. Both assume that at each time, any job seeker does not know whether or not she / he will receive any job offer (ME or not); thus, such job offer cannot be expected by the job seeker.

On the one hand dynamic matching models (Lechner, 2008; Frederiksson and Johansson, 2008) are one way to overcome these two difficulties. They allow to identify true effect of ME assuming that finding a regular job is independent to practicing ME, but conditional to all observed features characterizing JS, that are correlated to both ME and finding a regular job. The main advantage of such method is that it does not rely on any parametric functional form between outcome, treatment and control variables, whereas its main drawback is that it requires a large set of observed variables and assumes that all covariates are observed.

On the other hand, duration models and Timing Of Events (Abbring and Van Den Berg, 2003) may also be considered. Time to ME and unemployment duration are modelled simultaneously and linked by unobservable components representing unobservable heterogeneity between JS. The main advantage of this model is that it tries to take account for selection based both on observed and unobserved variables, whereas its main drawbacks is to assume a particular parametric modelling of unobserved heterogeneity.

2.3.3. Further on timing on events

In those models, treatment dynamics are characterized by the time elapsed before reduced activity. Basically, the impact of reduced activity is measured on one dimension and thus by a single outcome variable: time spent before leaving unemployment and returning to employment (Kyyrä *et al.*, 2013) or, according to a stricter definition, regular employment (Cockx *et al.,* 2013; Kyyrä, 2010). The two processes (time to reduced activity and unemployment duration) are modelled simultaneously and linked by unobservable components representing unobservable heterogeneity between individuals, given that these two durations can be represented as competing durations.

On the other hand, highly-skilled individuals with high unemployment exit rates will not try to take on temporary jobs that their human capital or social network. This is why the treatment, *i.e.* the length of time before taking up a reduced activity, must be modelled in conjunction with the outcome variable under consideration: these simultaneous equation models of hazard functions - estimated by maximum likelihood - thus deal with part of the phenomenon of endogenous selection of ME linked to the joint influence of unobservable individual heterogeneity on the two durations.

Extensions are provided by adding additional equations to systems of simultaneous equations. On the one hand, some studies have considered two treatment variables in order to distinguish between part-time from short full-time ME (Kyyrä, 2010) or to distinguish between the duration of ME prior to entry and the duration of the ME as such (Fremigacci and Terracol, 2013). On the other hand, some evaluations have focused on several dimensions of the impact of ME, measured by several outcome variables. For example, Fremigacci and Terracol (2013) additionally introduce a process, conditional on having found a new job, representing the recurrence of unemployment (longer-term effect), *i.e.* the duration of employment before becoming unemployed again and starting a new job search period. Godøy and Røed (2016) model even more processes by distinguishing between exits from unemployment into good quality jobs or to bad quality jobs, and also characterizing participation in other active labor market integration programs. In addition, this type of model makes it possible to highlight the heterogeneity of the impact of ME between different profiles of jobseekers, introducing the product of the treatment dummy with some observed variables.

3. Dataset and descriptive statistics

In this section, we discuss the empirical framework and present the data set on which our MRA is based.[3]

3.1. File drawer

In a first step, we have to choose the way to select papers to be considered to build the MRA dataset. For this, we need to take several factors into account. have to estimate the impact of marginal employment while being still registered at the (un-)employment agency as a job seeker on finding regular job.

3.1.1. Problems

First, we must pay attention to the treatment variable, *i.e.* the variable of interest that is considered in papers that assess the effect of this type of policies. We focus only on articles that study the consequences of schemes allowing job seekers – while still been registered to the (un-)employment agency to take on part-time or short-term jobs while combining the associated salary with at least part of their unemployment benefits. Consistent with the corresponding empirical literature, we consider a set of keywords or expressions that allow us to locate all articles addressing the topic: marginal employment; mini-jobs; atypical jobs; subsidized jobs or employment; subsidized temporary jobs; reduced activities; subsidized irregular jobs; part-time unemployment; underemployed job seekers; partial UI benefits; part-time unemployed workers; working part-time and receiving supplementary benefits for part-time unemployment; partial unemployment insurance benefits (unemployed workers who are looking for a full-time job but take up a part-time).

[3] See for instance Stanley *et al.* (2013) for guidelines on this task.

Second, we must select studies based on the variable they focus on to assess the effect of the ME scheme. We retain articles that aim to evaluate the effect of the device on a consistent indicator, here the ability of JS to find a regular job. We keep only such kinds of articles, and get rid of papers that focus on the effect of ME on the transition from employment to unemployment, or on the quality of job (wages or type of labor contracts - fixed term or open-ended contracts).

Third, the unit of the estimated effect of ME is also a matter. We consider only studies where the coefficient of interest is exclusively expressed as a variation in the probability of the JS to find a regular job.

3.1.2. Building dataset

We perform searches on scholar databases and internet research engines between May 2022 and January 2023. First, we use Econlit databases (Cairn, JSTOR, Science Direct, Springer Link). Second, we extended the search to specialized research institution websites for working papers or research reports (IZA, NBER, SSRN). Third, we use Google as a search engine to identify work in progress and other non-published research. We ensure that no relevant work was overlooked by searching in the references in the selected papers. For each paper, we consider the published or most recent version available.

To build the *dataset from the file drawer,* we code a common set of features provided by considered articles: precision of ME effect (estimated standard error); the institutional context (countries); the kind of survey under consideration (average year, kind of JS surveyed); the type of marginal employment (short-full time, part-time work; industry); timeline (date of entry in ME, horizon considered after ME to evaluate); individual features of JS for whom the effect is estimated (age, gender, education, qualification); past experience of JS on job market (employability, benefit from UI benefits); econometric identification (matching / timing of events; estimand); publication features (year; scope of research). While coding information, we contacted the authors of certain selected papers to request clarifications on their work.

The final dataset was checked for coherence and for possible errors in the coding of the different variables.

3.2. Final dataset

This first version of the MRA dataset contains information on 16 articles and 495 estimates, published or written over 1999-2021. We impose further restrictions. In particular, we drop estimates for which no precision was available. We exclude estimates for which there are two few observations for some considered criteria.

The final dataset includes 468 estimated coefficients of interest (*effect sizes*). For each paper, there are about 29 effect sizes. The average estimated effect of ME amounts to 0.21.

The sample is also characterized by a large heterogeneity in estimated values of ME effects. The standard deviation amounts to 0.37, with some very large values (in absolute values) for some effect sizes.

Table 1. Studies included in the meta-regression analysis and descriptive statistics on impact of ME.

Authors of the article	Number of effect sizes	Average effect size	Standard deviation	Min	Max
Auray and Lepage-Saucier (2021)	41	0.743	0.393	-.293	2.013
Böheim and Weber (2010)	8	-0.013	0.020	-.042	.013
Cockx, Goebel, and Robin (2013)	27	0.93	0.764	-.165	2.561
Caliendo, Kühn, and Uhlendorff (2016)	49	0.17	0.976	-.465	5.89
Eppel and Mahringer (2019)	33	0.033	0.360	-.08	2.035
Fontaine and Rochut (2014)	48	0.094	0.031	.025	.184
Freier and Steiner (2008)	6	0.004	0.026	-.033	.037
Fremigacci and Terracol (2013)	10	0.804	1.615	-.764	4.732
Gilles and Issehnane (2017)	14	-0.044	0.022	-.077	-.008
Granier and Joutard (1999)	12	0.089	0.250	-.54	.431
Gerfin, Lechner, and Steiger (2005)	26	0.079	0.047	-.04	.15
Godøy and Røed (2016)	8	0.257	0.274	.015	.761
Kyyrä (2010)	76	0.35	0.517	-.374	1.883
Kyyrä, Parrotta, and Rosholm (2013)	40	-0.114	0.435	-.645	.467
Lietzmann, Schmelzer and Wiemers (2017)	48	0.097	0.048	.011	.197
Lalive, Van Ours, and Zweimüller (2008)	22	0.068	0.108	-.071	.398
Sample averages	468 / 29.2	0.206/0.222			

Source: Author's compilation and computations.
Note: see references for full information on related papers.

3.3. Descriptive statistics: heterogeneity of studies

Corresponding articles can be described by a lot of specific features that may explain why estimations of the ME effect might differ across the overall sample. Appendix Table A1 provides definitions and sample statistics (means and standard deviations) for all those variables. Distinguishing features of articles, we see a lot of differences in estimated values for effect of ME (Table 2).

First, as to data sources, there are larger values for old surveys, or surveys containing information only for female, for younger or old JS; conversely, smaller values are observed for surveys on Australia, Denmark, Germany or Swiss.

Second, regarding the type of ME, effects sizes are greater if ME for which effect is estimated refers to short full-time work, whereas estimated values are smaller for JS whose ME's experience is part-time work, begins early during the unemployment spell, happens in industries like transportation, finance, accommodation or scientific activity, and when the horizon of evaluation is short after the ME's experience.

Third, there are also differences in estimated values for ME's effect depending on several individual features of JS for whom ME effect is evaluated. In particular, larger values in effect size is detected for female or young JS, or for (blue-collar) workers. On the contrary, smaller values for individuals who not receive any UI benefits.

Fourth, as to control variables, there are larger values for estimated values ME's effect if the considered analysis controlled for duration dependence, number of months as unemployed, local features (including unemployment rate). On the other hand, smaller values are obtained for estimated ME's effect if gender, mother tongue, qualification, employment history is controlled for.

Fifth, larger values of ME's effect are estimated if timing of events as an identification strategy is considered, or if unobserved heterogeneity is controlled for, whereas smaller values are obtained for ME's effect if a matching estimator is used, or ATET rather than ATE is estimated.

Table 2. Differences in the mean effect size of marginal employment by type of characteristics of the study.

Variable	Difference[a]	Std. Error	Significance[b]
Data sources:			
Average year of the survey:			
1993-1998	0.131	0.07	0.065*
1999-2000	0.156	0.053	0.004***
2001	0.011	0.069	0.871
2002-2012	-0.214	0.056	0***
Country of the survey:			
Austria	-0.232	0.059	0***
Belgium	0.738	0.149	0***
Denmark	-0.383	0.074	0***
Finland	0.137	0.067	0.042**
France	0.153	0.062	0.015**
Germany	-0.14	0.072	0.055*
Norge	0.022	0.101	0.834
Swiss	-0.18	0.033	0***
Average age of the JS:			
20-32 years old	0.171	0.076	0.028**
34-37 years old	-0.143	0.08	0.075*
38 years old	*0.058*	*0.056*	*0.301*
39 years old	-0.336	0.057	0***
40-47 years old	0.169	0.053	0.002***
Gender of JS in data:			
All kinds	-0.209	0.107	0.053*
Women	0.738	0.149	0***
Men	-0.095	0.127	0.458
Kind of regular job: full-time equivalent	-0.173	0.029	0***
Type of Marginal Employment:			
Type of activity for ME:			
All kinds	-0.025	0.054	0.643
Part-time work	-0.114	0.053	0.033**
Short fulltime work	0.449	0.088	0***
Date for the start of the ME after beginning of the unemployment spell:			
All dates	0.061	0.05	0.219
0 to 6 months after	-0.193	0.042	0***
7 to 12 months after	0.102	0.065	0.118
13 to 36 months after	0.174	0.111	0.128
Time horizon considered for measuring ME's effect:			
All time horizons	0.272	0.057	0***
Short run	-0.307	0.053	0***
Long run	-0.078	0.05	0.12
Type of JS for whom ME's effect is estimated:			
Gender of JS:			
All genders	-0.121	0.052	0.022**
Men	-0.016	0.07	0.82
Women	0.192	0.066	0.004***
Age of JS:			
All ages	-0.333	0.109	0.004***
Junior (less than 30)	0.576	0.137	0***
Middle age	-0.165	0.15	0.298
Senior (more than 50)	-0.024	0.243	0.924
Socio-professional categories:			
All	-0.152	0.144	0.303
Workers	0.667	0.085	0.002***
Employees	0.106	0.309	0.754
Technicians	-0.095	0.41	0.839

Executives	0.126	0.37	0.757
Other qualification	-0.217	0.317	0.562
Education:			
All education	-0.293	0.212	0.185
Low education	0.48	0.374	0.236
High education	0.095	0.203	0.652
Past E/U history:			
Employability			
All unemployment experiences	0.226	0.036	0***
Low employability	-0.168	0.048	0.01***
High employability	-0.269	0.028	0***
Past earnings as employed:			
All earnings	0.137	0.032	0***
Low earnings	-0.113	0.03	0.001***
High earnings	-0.172	0.028	0***
JS with or without UB:			
All (receive benefits or not)	-0.087	0.153	0.574
Receive benefits	0.367	0.278	0.203
Not receive benefits	-0.216	0.065	0.003***
Industries for ME:			
All industries	0.302	0.074	0.001***
Metal industry	-0.224	0.421	0.689
Manufacturing	-0.288	0.237	0.346
Construction	-0.35	0.223	0.211
Trade	-0.257	0.278	0.452
Other industries	-0.316	0.039	0***
Transp./ Accommodation/ Finance / Scientific activity	-0.286	0.03	0***
Other services (red)	-0.304	0.036	0***
Considered control variables:			
Duration dependence	0.273	0.045	0***
Seasonal dummies	-0.048	0.064	0.456
Gender	-0.212	0.106	0.047**
Nationality	-0.043	0.053	0.418
Mother tongue	-0.18	0.033	0***
Marital status	*0.152*	*0.061*	*0.019***
Number of children	0.291	0.044	0***
Health	-0.146	0.091	0.112
Education	0.041	0.052	0.428
Qualification	-0.146	0.053	0.006***
Industry of last job	-0.406	0.065	0***
Employment history (hours worked)	-0.224	0.102	0.03**
Number of months as unemployed	0.11	0.055	0.045**
Being entitled to UI	-0.023	0.061	0.712
Local labor market	0.297	.044	0***
Local dummies	0.142	.056	.011
Local unemployment rate	0.175	0.052	0.001***
No local controls	-0.297	0.044	0***
Estimator:			
Controlling for unobserved	0.237	0.051	0***
Type of model:			
Matching	-0.291	0.046	0***
Timing of events	0.303	0.049	0***
Other estimators	-0.117	0.062	0.07*
Estimand:			
Average treatment effect	0.27	0.044	0***
Average treatment effect on the treated	-0.27	0.044	0***
Publication features:			
Year of publication			

1999-2008	-0.193	0.035	0***
2010	0.098	0.063	0.124
2013	0.163	0.109	0.14
2014-2016	-0.121	0.072	0.094*
2017-2021	0.037	0.051	0.468
Field of research:			
Labor area	-0.015	0.06	0.795
General area	0.015	0.06	0.795

Source: Author's compilation (Table 1 and references) and computations.
Scope: 468 estimates provided by 16 articles that aim at evaluating the effect of a ME device, excluding observations for which some information is not usable and for which standard error of the effect size is unavailable.
Notes: [a]Ratio of estimated effect of ME to its standard error. [b]Standard error of estimated effect size.

4. Publication bias. Funnel plots and asymmetry testing

4.1. Publication bias.

As reported for instance in Stanley (2008, p. 104), publication bias is a 'the consequence of choosing research papers for the statistical significance of their findings', which may result from behaviors of researchers, reviewers and/or editors. For instance, in the case of medical studies, papers that provide positive results (*i.e.* indicating a positive effect of the 'treatment') are more likely to be published (Begg and Berlin, 1988).

More generally, and particularly in economics, these different features entail that published results can overstate or understate the true effect such that the estimated effects of ME might be correlated with sampling errors (Stanley and Jarrell, 1989). If these effects are correlated with other variables, then the conclusions about the determinants of finding a regular job may be seriously biased. The existence of such bias is due to the natural workings of a scientific process designed to discover important new results (Ashenfelter *et al.*, 1999).

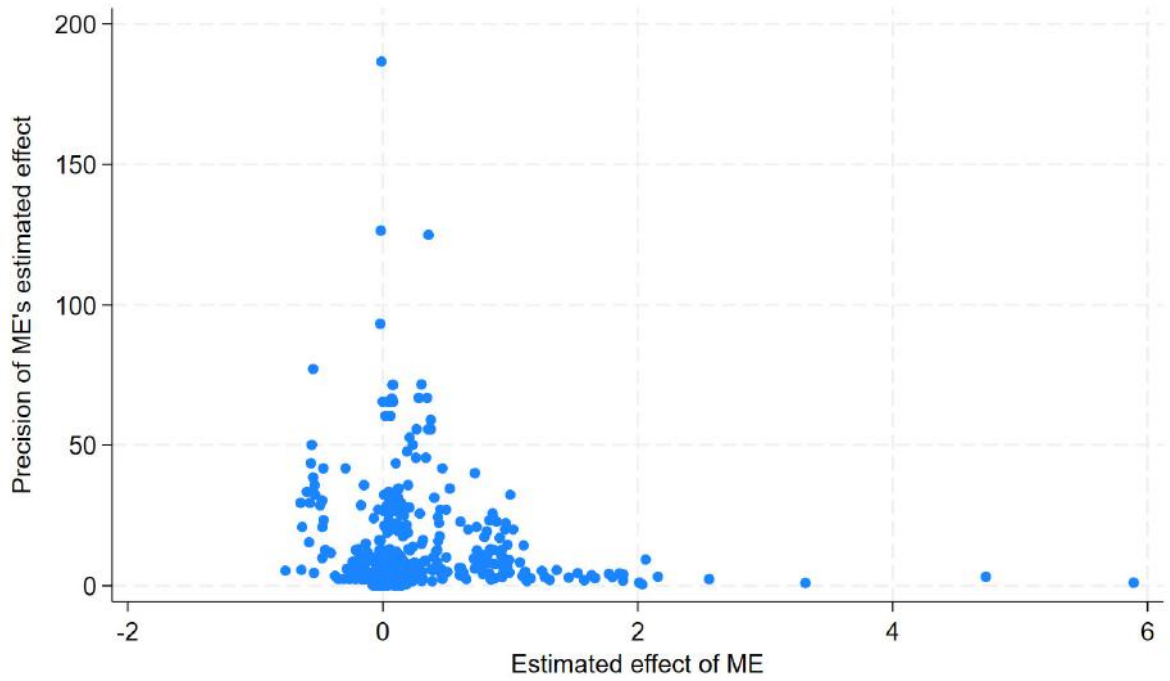
4.2. Graphical approach: Funnel / Galbraith plots

4.2.1. Funnel plots

A first approach to detect publication bias is funnel plot. As defined in Sutton *et al.* (2000b), the graph represents the relation between the estimated coefficient (or effect) of interest and its precision, which is often measured by the inverse of the standard error of the estimated coefficient (or effect) of interest.

Its interpretation is the following. In absence of any publication bias, estimates will vary randomly, hence symmetrically around the true effect (Stanley (2008), p. 107). Thus, these plots are referred to as 'funnel plots' because they should be shaped like a funnel if no publication bias is present. This particular shape is expected because trials of smaller size (which are more numerous) have increasingly large variation in the estimates of their effect size as random variation becomes increasingly influential. However, since very frequently, smaller or non-significant studies are less likely to be published, trials in the bottom left hand corner (when a desirable outcome is being considered) of the plot are often omitted, creating a degree of asymmetry in the funnel. [Because small-sample studies with typically larger standard errors and hence less precision are at the bottom of the graph, the plot will be more spread out at the bottom than it is at the top.]

Figure 1-a. Funnel plot: scatter diagram of precision versus non-standardized effect of Marginal Employment.



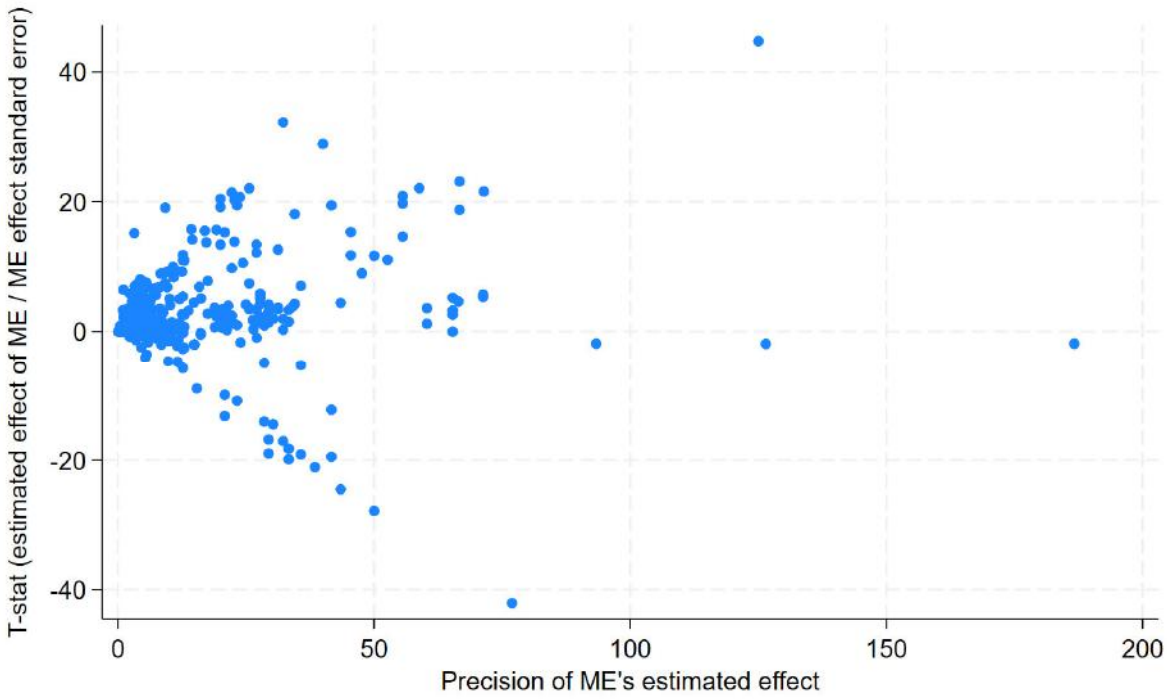
Source: Author's compilation (Table 1 and references) and computations.
Scope: all articles included in the meta-regression analysis, excluding observations with non-usable information of moderators or missing SE of estimated effects of marginal employment.
Notes: x-axis: effect size (estimated effect of marginal effect); y-axis: precision of estimated effect of marginal employment (inverted standard error for estimated effect of marginal employment). size (estimated effect of marginal effect).

The distribution does not need to contain both positive and negative correlations; a funnel plot can be symmetrical with all positive (or negative) valued observations as it is sometimes the case (Abdullah *et al.*, 2015). Hence, Sutton et al. (2000b) refer to an overweighted plot on one side the left or another on the right around what would be the true effect of parental education could be a sign of the existence of publication selection, respectively negative (under-estimation of the effect) or positive (over-estimation of the effect). In the context of our study, funnel plot displayed in Figure 1 shows an overweighting on the right side, even if it is not so clear-cut (only bias in top graph on the left).

4.2.2. Galbraith plots

A second approach to detect publication bias is provided by the Galbraith plot (Galbraith, 1988). Indeed, heterogeneity of true effect and misspecification biases may also be seen as 'type II' publication selection (*i.e.*, excess variation). Type II arises from the selection of statistically significant findings, irrespective of their direction. Type II selection will cause excess variation. Large *t-values* (in magnitude) will be overreported.

Figure 1-b. Galbraith plot. Scatter diagram of standardized effect (often a t-value) versus precision



Source: Author's compilation (Table 1 and references) and computations.
Scope: all articles included in the meta-regression analysis, excluding observations with non-usable information of moderators or missing SE of estimated effects of marginal employment.
Notes: x-axis: precision of estimated effect of marginal employment (inverted standard error of effect of marginal employment); y-axis: t-statistic (estimated effect of marginal employment related to its estimated standard error).

Assuming that there was no genuine effect of Marginal Employment (*T-stat = 0*), only 5% of the studies should report t-statistics exceeding roughly 2. However, we find that 187 of 468 estimates report t-statistics greater, in magnitude, than the associated critical value for the 0.05 significance level. Hence, there is suspicion for publication bias.

4.2. Funnel asymmetry testing (FAT)

However, funnel plots are only graphs that represent the empirical relationship between an estimate and its precision. The presence of asymmetry in the shape of the funnel would indicate possible publication bias. This graph asymmetry can be formally tested through the funnel asymmetry test (FAT, Stanley 2005).

4.2.1. Presentation

Funnel asymmetry testing was suggested by Egger *et al.* (1997) through the equation:

$$b_j = \beta_1 + \beta_0 SE_j + u_j \qquad (1)$$

Where b_j denotes the estimated effect of ME on regular employment. It is reported in the j^{th} study in our final dataset ($j = 1, 2, \ldots N$). SE_j is the standard error of b_j, and u_j is a random error term. If there is no publication bias, the estimated effects should vary randomly around the true value β_1 of ME's effect on finding a regular job.

FAT consists in a two-tailed *t-test* performed on the intercept: if β_0 is different from zero, there is evidence for funnel symmetry and thus for publication bias (Sutton *et al.* 2000a). The sign of the estimate of β_0 indicates the direction of this bias.

Otherwise, testing H_0: $\beta_1 = 0$ becomes a test for the existence of empirical effect of ME on regular employment (Precision Effect Testing, PET, see Stanley (2005)).

When estimating equation (1), two issues arise that may be important to address. The OLS estimator using this equation is heteroskedastic. Considering robust standard errors is a solution. However, OLS may remain still inefficient (Cipollina and Salvatici, 2010). Estimates displayed in considered studies included in filedrawer may be dependent. To take account for this, 'robust with cluster' procedure is adopted, adjusting standard errors for intra-study correlation (Sterne et al., 2000; Macaskill et al., 2001).

4.2.2. Results

As mentioned in Egger *et al.* (1997), FAT is characterized by a low power. To take account for the fact we do not have a necessarily large sample of effect sizes at hand, we thus proceed to FAT at a 10 percent level.

First, as to FAT, estimates in column one of Table 3a report a negative estimated β_1 that indicates under estimation of ME's effect (at a 10 percent significance level). Second, concerning PET, estimated β_0 shows evidence for a positive impact of ME because estimated β_0 is 0.28 and is significant at a 5 percent level.

Table 3a. Marginal employment effect and publication bias. Funnel asymmetry and precision effect testing.

Estimated parameter	(1)	(2)	(3)	(4)	(5)
Standard Error of estimated effect of ME	-0.0443670* (0.062)	-0.0427752* (0.068)	-0.0427152* (0.061)	-0.0425464** (0.044)	-0.0411015** (0.040)
Intercept	0.2840413** (0.011)	0.2647515** (0.018)	0.2568246** (0.019)	0.2402048** (0.014)	0.2252780** (0.012)
Observations	468	464	460	447	422
R-squared	0.020	0.028	0.032	0.044	0.068

Source: Author's compilation (Table 1 and references) and computations.
Scope: all articles included in the meta-regression analysis, excluding at least observations with non-usable information of moderators or missing SE of estimated effects of marginal employment.
Notes: effect size is the explained variable. Estimated coefficient for standard error of estimated effect of marginal employment is considered to test for funnel asymmetry; estimated intercept is used to test for genuine empirical effect. Robust p-value within parentheses, using standard errors clustered at the article level. *** (resp. ** or *) stands for significance at a 1% (resp. 5% or 10%) level. Detailed results: results for col (1) hold when excluding observations with non-usable information and missing values for SE of estimated effect of ME, for col (2) as well as with 0.5 pct of lowest / highest values of estimated effect of marginal employment, for col (3) with 1 pct of lowest / highest values of estimated effect of marginal employment, or for col (4) with 2.5 pct of lowest / highest values of estimated effect of marginal employment, or for col (5) with 5 pct of lowest / highest values of estimated effect of marginal employment.

Since of low statistical power for these tests, we consider robustness checks estimating parameters of the same equation, considering restricted samples: for column (2) (resp. column (3); column (4); column (5)), we also exclude from main sample observations with 0.5 (resp. 1 percent; 2.5 or 5 percent) of lowest / highest values of estimated effect of marginal employment. Results remain the same.

4.2.3. Alternative specification

As an alternative estimation to (1), weighted least squares (WLS) may also be applied to obtain efficient estimates (Maddala, 1977). Dividing (1) by SE_j, the following equation is thus estimated:

$$t_j = \beta_0 + \beta_1(1 / SE_j) + \varepsilon_j \quad (2)$$

where t_j is the conventional *t-value* for b_j. Note that the intercept and slope coefficients are reversed, and the independent variable becomes the inverse of its previous incarnation.

Table 3b. Marginal employment effect and publication bias. Funnel asymmetry and precision effect testing. V2-Estimation of equation with standardized variables.

Estimated parameter	(1)	(2)	(3)	(4)	(5)
Inverted standard error of estimated effect of marginal employment	0.0634033 (0.118)	0.0664310 (0.117)	0.0673777 (0.117)	0.0903101 (0.134)	0.1317005 (0.179)
Intercept	1.1172471 (0.239)	1.0961375 (0.248)	1.1141026 (0.235)	1.0458698 (0.266)	0.9585318 (0.347)
Observations	468	464	460	447	422
R-squared	0.030	0.034	0.035	0.073	0.201

Source: Author's compilation (Table 1 and references) and computations.
Scope: all articles included in the meta-regression analysis, excluding at least observations with non-usable information of moderators or missing SE of estimated effects of marginal employment.
Notes: ratio of size to its standard error is the explained variable. Estimated coefficient for inverted standard error of estimated effect of marginal employment is considered to test for existence of genuine empirical effect; estimated intercept is used to test for publication bias. Robust p-value within parentheses, using standard errors clustered at the article level. *** (resp. ** or *) stands for significance at a 1% (resp. 5% or 10%) level. Detailed results: results for col (1) hold when excluding observations with non-usable information and missing values for SE of estimated effect of ME, for col (2) as well as with 0.5 pct of lowest / highest values of estimated effect of marginal employment, for col (3) with 1 pct of lowest / highest values of estimated effect of marginal employment, or for col (4) with 2.5 pct of lowest / highest values of estimated effect of marginal employment, or for col (5) with 5 pct of lowest / highest values of estimated effect of marginal employment.

Corresponding results are reported in Table 3b. We do not find any evidence both for publication bias (estimated β_0 is positive but not significant) and for genuine empirical effect (estimated β_1 is still positive but NS). The power of these test remains low. Besides, we still do not consider further heterogeneity.

5. Multivariate MRA. Estimated equation and results.

Through FAT and PET, we find evidence for both publication bias and genuine empirical effect for the effect of ME on regular employment. So far, we haven't considered heterogeneity among the studies. Table 2 shows that effect sizes seem to differ according to several features of articles included in the dataset.

5.1. Identification

The aim of this section is to take account for heterogeneity of studies. The multivariate MRA generalizes the FAT-PET approach, adding "moderators", *ie. K* (often dummy) variables Z_{jk} coding and characterizing for features of articles *j* (e_j is the new - meta-regression - error term):

$$b_j = \beta_1 + \beta_0 SE_j + \sum_{k=1}^{K} \alpha_k Z_{jk} + e_j \quad (3)$$

In this equation, coefficients of moderators refer *ceteris paribus* to difference in terms of estimated effect of ME between studies that are characterized by the given criterion ($Z_k = 1$) and the reference group. In this case, β_0 represents the 'true' value of the ME causal effect, once heterogeneity of studies is considered and corrected for publication bias (represented by β_1) that is measured for the reference group ($Z_k = 0$).

5.2. Findings

While still considering clustered standard errors at the study level, we use OLS to estimate parameters of (3). We consider several specifications. Since there are a lot of qualitative features characterizing articles, some multicollinearity problems may be account for. We thus include / exclude some set of variables from some estimated equations. Table 4 reports these results.

First, the empirical effect sizes (estimated effects of ME) are largely explained by the heterogeneity of studies. Indeed, a large of characteristics of considered articles included in our dataset are *ceteris paribus* significantly correlated to estimated ME's effects. This is the case for data sources (larger values when the year of the survey is 2001, or where JS in the sample is rather old – 40-47 years old), for the type ME that is experienced (smaller values for part-time, but larger for short full-time ME experience), for individual features of JS (larger values for blue collar workers), for control variables considered while estimating ME effect (larger values if duration dependence or entitlement to UI benefits is account for, if seasonal dummies are included; smaller values if qualification of JS as well as number of months as unemployed is controlled for); for the econometric estimator under consideration (smaller values are obtained through matching). Overall, R-squared of corresponding regressions amount to more than 0.3 (compared to 0.03 for FAT-PET).

Second, focusing on the existence of publication bias, we see that estimated intercept is not statistically significantly different from zero. It indicates no publication bias for the reference group.

Third, we look at genuine empirical effect. Estimated β_0 is positive and significant. ME increases probability of finding a regular job from by about 17 percentage points, ranging from 0.14 to 0.45, irrespective of publication selection and heterogeneity of studies.

Table 4. Multivariate Meta-Regression Analysis of the effect of marginal employment on finding a regular job.

Explanatory variable / Specification	(1)	(3)	(4)	(5)	(6)	(7)
Intercept	0.3202497***	0.2665007**	0.4599339**	0.3825761***	0.1676723	0.1921848*
	(0.001)	(0.036)	(0.013)	(0.000)	(0.108)	(0.093)
Inverted precision of impact of ME						
Squared error of impact of ME	-0.0168037	0.0017758	0.0006788	0.0211969	0.0238905	0.0211969
	(0.632)	(0.970)	(0.985)	(0.651)	(0.540)	(0.651)
Data sources:						
Average year of the survey:						
1993-1998	-0.4151138**		0.2186798	0.1868434	0.6095018***	0.4741091**
	(0.016)		(0.311)	(0.173)	(0.000)	(0.018)
1999-2000	-0.2623435			0.4113292	0.1099156	0.1281229
	(0.143)			(0.185)	(0.632)	(0.634)
2001	-0.5439909**	-0.8119419***	-0.8126776***	-0.8029910***	-0.8533536***	-0.8029910***
	(0.038)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
2002-2012	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*
Country of the survey:						
Austria	-	-	-0.2665546	-0.6220563	-0.4263643	-0.3872871
			(0.276)	(0.136)	(0.280)	(0.418)
Belgium	1.3500295***	-	-	-	-	-
	(0.003)					
Denmark	0.1825318	-	-	-	-	-
	(0.646)					
Finland	0.9032360*	-	-	-	-	-
	(0.090)					
France	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*
Germany	0.2435198	-	-	-	-	-

	(0.422)					
Norge	0.7060602	-	-	-	-	-
	(0.122)					
Swiss	0.4735410***	-	-	-	-	-0.0384262
	(0.006)				-	(0.902)
Average age of the JS:						
20-32 years old	0.2764843		-0.1644459	-0.1118453	-0.0129895	0.0522978
	(0.351)		(0.433)	(0.406)	(0.956)	(0.838)
34-37 years old	-0.0538394	-0.3021159*	-0.2075257**	-	-	-
	(0.784)	(0.065)	(0.019)			
38 years old	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*
39 years old	-	-0.0540572	-	0.0413900	0.0582703	0.0087017
		(0.838)		(0.561)	(0.482)	(0.897)
40-47 years old	0.5347721**	0.6429050**	0.5424848***	0.5156469***	0.7551680**	0.7370478**
	(0.037)	(0.017)	(0.009)	(0.000)	(0.025)	(0.043)
Kind of regular job: full-time equivalent	-0.0729865	-0.0615102	-0.0789047	-0.0664068	-0.0258454	-0.0664068
	(0.241)	(0.162)	(0.229)	(0.268)	(0.129)	(0.268)
<u>Type of Marginal Employment:</u>						
Type of activity for ME:						
All kinds	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*
Part-time work	-0.7011265	-0.1119162	-0.3462544**	-0.0383290	-0.4774897***	-0.4236593***
	(0.108)	(0.146)	(0.047)	(0.797)	(0.000)	(0.001)
Short fulltime work	-0.1209852	0.4616011***	0.2292478	0.5367464***	0.0963844	0.1514161
	(0.774)	(0.000)	(0.183)	(0.002)	(0.156)	(0.147)
Date for the start of the ME after beginning of the unemployment spell:						
All dates	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*
0 to 6 months after	-0.2426410	-0.1514583	-0.1498810	-0.1582831	-	-0.1582831
	(0.131)	(0.294)	(0.201)	(0.272)		(0.272)
7 to 12 months after	-0.0058697	0.0848072	0.0850629	0.0654378	-	0.0654378
	(0.973)	(0.612)	(0.523)	(0.568)		(0.568)

13 to 36 months after	0.0355297 (0.835)	0.0327271 (0.834)	0.0440407 (0.646)	0.0072732 (0.953)	-	0.0072732 (0.953)
Time horizon considered for measuring ME's effect:						
All time horizons	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*
Short run	-0.2444908 (0.298)	-0.1581561 (0.467)	-0.1673743 (0.452)	-	-	-
Long run	0.1299071 (0.600)	0.2260529 (0.334)	0.2170916 (0.359)	-	-	-
Type of JS for whom ME's effect is estimated:						
Gender of JS:						
All genders	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*
Men	0.0230602 (0.846)	-0.0297371 (0.818)	-	-	-	-
Women	-0.0083327 (0.944)	-0.0506659 (0.699)	-	-	-	-
Age of JS:						
All ages	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*
Junior (less than 30)	-0.0307341 (0.847)	-0.0395341 (0.806)	-	-0.0344093 (0.784)	-0.0304406 (0.765)	-0.0344093 (0.784)
Middle age	-0.0868374 (0.643)	-0.0908262 (0.628)	-	-0.0858845 (0.609)	-0.0890685 (0.540)	-0.0858845 (0.609)
Senior (more than 50)	0.0868711 (0.740)	0.0805565 (0.766)	-	0.0705193 (0.788)	0.0726583 (0.755)	0.0705193 (0.788)
Socio-professional categories:						
All SPC	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*
Workers	0.1491633 (0.242)	0.1591120 (0.192)	0.1678416** (0.029)	0.1648354* (0.055)	0.1687593*** (0.001)	0.1648354* (0.055)
Employees	0.0167473 (0.905)	0.0131567 (0.931)	0.0262462 (0.645)	0.0186280 (0.891)	0.0261513 (0.798)	0.0186280 (0.891)
Technicians	-0.0415044	-0.0496546	-0.0351091	-0.0443159	-0.0355995	-0.0443159

	(0.790)	(0.770)	(0.618)	(0.772)	(0.770)	(0.772)
Executives	0.1197492	-0.0090039	0.0300516	-0.0035811	0.0051470	-0.0035811
	(0.570)	(0.962)	(0.823)	(0.984)	(0.972)	(0.984)
Other qualification	-0.1633882	-0.1712782	-0.1567481*	-0.1656676*	-0.1569135**	-0.1656676*
	(0.259)	(0.218)	(0.078)	(0.065)	(0.032)	(0.065)
Education:						
All education	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*
Low education	0.2989806	0.3013382	0.3084489	0.3170034	-	0.3170034
	(0.582)	(0.581)	(0.548)	(0.547)		(0.547)
High education	-0.0599195	-0.0415533	-0.0325812	-0.0304918	-	-0.0304918
	(0.745)	(0.820)	(0.824)	(0.842)		(0.842)
Past E/U history:						
Employability						
All unemployment experiences	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*
Low employability	0.0122596	0.0002400	0.0171643	-0.0338893	-	-0.0338893
	(0.944)	(0.999)	(0.882)	(0.785)		(0.785)
High employability	-0.0552682		-0.0359658	-0.1088976	-	-0.1088976
	(0.634)		(0.722)	(0.184)		(0.184)
Past earnings as employed:						
All earnings	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*
Low earnings	0.0886826	0.0495926	0.0728038	-0.0090339	-	-0.0090339
	(0.685)	(0.828)	(0.612)	(0.959)		(0.959)
High earnings	-0.1444612	-0.1315255	-0.1116646	-0.0042812	-	-0.0042812
	(0.321)	(0.347)	(0.205)	(0.954)		(0.954)
JS with or without UB:						
All (receive benefits or not)	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*
Receive benefits	0.2469239	0.2283999	0.2086094	0.2169902	-	0.2169902
	(0.379)	(0.351)	(0.371)	(0.364)		(0.364)
Not receive benefits	-0.2810376	-0.3118013	-0.3326456	-0.3352621	-	-0.3352621
	(0.412)	(0.401)	(0.393)	(0.378)		(0.378)
Industries for ME:						

All industries	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*
Metal industry	0.1130257	0.0951620	0.1126546***	0.0996140	0.1106303	0.0996140
	(0.434)	(0.529)	(0.000)	(0.357)	(0.188)	(0.357)
Manufacturing	0.0197873	-0.0186842	-0.0047379	-0.0544699	-0.0496566	-0.0544699
	(0.913)	(0.925)	(0.963)	(0.760)	(0.728)	(0.760)
Construction	-0.1063319	-0.1253181	-0.1133375	-0.1387154	-0.1394256	-0.1387154
	(0.601)	(0.540)	(0.396)	(0.432)	(0.365)	(0.432)
Trade	0.0263500	0.0147200	0.0270815	0.0069917	0.0156964	0.0069917
	(0.863)	(0.924)	(0.743)	(0.957)	(0.883)	(0.957)
Transp./ Accomod/ Finance./Scientif Act	-0.0726513	-0.1143418	-0.1097315	-0.1908848	-0.1929691	-0.1908848
	(0.704)	(0.603)	(0.498)	(0.366)	(0.198)	(0.366)
Other services (red)	-0.1387229	-0.1519471	-0.1474513	-0.1873659	-0.1920639	-0.1873659
	(0.416)	(0.387)	(0.270)	(0.228)	(0.105)	(0.228)
Considered control variables:						
Duration dependence	-	0.5586712***	0.6140340***	0.6816094***	0.5794746***	0.5496662***
		(0.004)	(0.002)	(0.000)	(0.000)	(0.000)
Seasonal dummies	-	0.2655901	0.1840702		0.5739962***	0.5587823**
		(0.342)	(0.170)		(0.000)	(0.018)
Nationality	-	0.0430254	0.2051481	0.3275842***	-	-
		(0.716)	(0.215)	(0.009)		
Mother tongue	-	-	-	-	-0.1050801	-
					(0.707)	
Number of children	-	0.4886229**	0.3109414***	-0.0253402	0.1322434	0.1025437
		(0.023)	(0.000)	(0.828)	(0.300)	(0.423)
Health	-	-	-0.1678526	0.2889443	-	-
			(0.465)	(0.327)		
Qualification	-	-0.5014136***	-0.7441568***	-0.7140849***	-0.8277687***	-0.7847109***
		(0.000)	(0.001)	(0.000)	(0.000)	(0.000)
Employment history (hours worked) _	-	-0.2836508*		-0.0281369	0.0880345	0.0909264
		(0.085)		(0.656)	(0.276)	(0.456)
Number of months as unemployed	-	-0.1531744*	-0.2191177	-0.2010159	-0.4248161**	-0.3785273**

		(0.092)	(0.224)	(0.198)	(0.012)	(0.024)
Being entitled to UI	-	-	-	0.0805208	0.5699144***	0.5412385***
				(0.313)	(0.000)	(0.000)
Local labor market	-	0.4377671	-	-	-	-
		(0.181)				
Local unemployment rate	-	-0.6157660**	-0.1619930	-0.0804457	-0.0174064	-0.0057603
		(0.036)	(0.441)	(0.453)	(0.917)	(0.977)
No control	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*
Estimator						
Controlling for unobserved	-0.0803835	-0.0866862	-0.0845178	-0.0882702	-0.0933690	-0.0882702
	(0.313)	(0.393)	(0.275)	(0.227)	(0.185)	(0.227)
Type of model:						
Matching	0.4577303	0.0182101	-0.1036131	-0.2042129**	-0.2226379**	-0.2042129**
	(0.238)	(0.911)	(0.212)	(0.020)	(0.021)	(0.020)
Timing of events	0.2370962	-0.0829060	-0.0981995	-0.1053703	-0.1002881	-0.1053703
	(0.119)	(0.662)	(0.117)	(0.155)	(0.156)	(0.155)
Other estimator	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*
Estimand						
Average treatment effect	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*
Average treatment effect on the treated	-0.6384690*	-0.0934857	-	0.0377556	0.0562945	0.0377556
	(0.082)	(0.494)		(0.642)	(0.531)	(0.642)
Publication features						
Year of publication						
1999-2008	-	0.3397593*	-	-	-	-
		(0.054)				
2010	-	-0.2911290	-	-	-	-
		(0.335)				
2013	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*
2014-2016	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*
2017-2021	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*

Field of research:						
Labor area	-	0.0238284	-0.0954618	-0.4263987	-0.2127436	-0.2488873
		(0.823)	(0.477)	(0.103)	(0.244)	(0.268)
General area	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*	*Ref.*
Observations	468	468	468	468	468	468
R-squared	0.360	0.370	0.369	0.336	0.302	0.336

Source: Author's compilation (Table 1 and references) and computations.
Scope: all articles included in the meta-regression analysis, excluding at least observations with non-usable information of moderators or missing SE of estimated effects of marginal employment.
Notes: effect size is the explained variable. Estimated coefficient for standard error of estimated effect of marginal employment is considered to test for funnel asymmetry; estimated intercept is used to test for genuine empirical effect. Robust p-value within parentheses, using standard errors clustered at the article level. *** (resp. ** or *) stands for significance at a 1% (resp. 5% or 10%) level.

5.3. Robustness

As for FAT/PET in section 4, there is an alternative to account further for heteroscedasticity. Indeed, WLS may be used. To proceed, we have to divide all the variables of previous equation by the effect size standard error (SE_j) and consider the following equation (Stanley *et al.*, 2008):

$$t_j = \beta_0 + \beta_1 (1 / SE_j) + \sum_{k=1}^{K} \alpha_k \left(Z_{jk} / SE_j \right) + v_j \qquad (4)$$

This time, β_1 represents the 'true' value of the impact of ME on finding regular job, once heterogeneity of studies is taken into account for publication bias (represented by β_0) that is measured for the reference group (Havranek and Irsova, 2011). v_j is the meta-regression disturbance term (unobserved heterogeneity).

We consider several sets of moderators, the same for both kinds of equations, (3) and (4). Three kinds of samples are also considered: (a) with initial restrictions, (b,c,d) dropping 0.5 percent (respectively 1 percent, respectively 2.5 percent) of smallest / largest values of estimated effect sizes / estimated effect of ME.

Our results are the following. First, heterogeneity of studies still helps in explaining effect sizes. Whatever the kind of regression, R-squared is quite large, slightly greater in standardized regressions than in regression using variables in levels. Plenty of features explain the size of estimated ME's effect. Second, there is still no evidence for publication bias for the individual of reference. Third, overall, while being larger when considering standardized regressions, ME finally increases the probability to find a regular job on average by 32 to 40 percentage points, considering either regression with variables in level, or with standardized variables.

5.4. Discussion

The FAT-MRA multiple regressions give evidence for a causal effect for JS of marginal employment on their probability to find a regular job. The estimated effect of ME is around 0.30. This is of significant magnitude and in the MRA literature corresponds to the 'true' or 'genuine' empirical effect of the interest variable (Stanley, 2005). Hence, ME increases for JS the probability to find a regular job on average by 30 percent.

On the other hand, the 0.30 estimate is slightly larger than that from the average effect size provided by the articles included in the file drawer under consideration for this evaluation (*i.e.* 0.21, see Table 1), but not significantly different because its values range from 0.14 to 0.45. Moreover, Table 1 reports large ranges of values for estimated impacts in every papers of the file drawer: 12 out of the 16 studies report both negative and positive estimated effect for ME on regular employment. This conclusion is a support maintaining this type of approach to activate passive expenditure.

Besides, descriptive statistics in Table 2 show that a large number of moderators are significantly related to the estimated coefficient of effect of ME. In particular, when controlling for moderators, R-squared jumped from 0.03 to 0.35 or even twice its value, following the considered econometric specification and estimation method. Hence, the heterogeneity of studies explains a large part of the variation in the coefficient of parental transmission of education in related empirical studies. Moreover, there is hardly any evidence for publication bias.

Finally our results show it was important to consider meta-regression analysis to provide new evidence on the causal effect of ME on the probability for JS to find a regular job.

Table 5a. Multivariate Meta-Regression Analysis of the effect of marginal employment on finding a regular job. Part 1. *Using level of variables.*

Explanatory variables / Specifications	(1)	(2)	(3)	(4)	(5)
Intercept	0.2366692**	0.3541572***	0.4599339**	0.2301570***	0.2256612***
	(0.020)	(0.000)	(0.013)	(0.006)	(0.009)
Inverted precision of impact of ME					
Squared error of impact of ME	0.0019949	0.0225677	0.0006788	-0.0220398	-0.0250440
	(0.966)	(0.626)	(0.985)	(0.170)	(0.157)
Control variables	Yes	Yes	Yes	Yes	Yes
Observations	476	476	468	447	447
R-squared	0.381	0.348	0.369	0.596	0.596

Source: Author's compilation (Table 1 and references) and computations.
Scope: all articles included in the meta-regression analysis, excluding at least observations with non-usable information of moderators or missing SE of estimated effects of marginal employment.
Notes: effect size is the explained variable. Estimated coefficient for standard error of estimated effect of marginal employment is considered to test for funnel asymmetry; estimated intercept is used to test for genuine empirical effect. Robust p-value within parentheses, using standard errors clustered at the article level. *** (resp. ** or *) stands for significance at a 1% (resp. 5% or 10%) level. Precision concerning the sample considered: results for col (1) and (2) hold when excluding observations with missing values for SE of estimated effect of ME, for col (3) as well as with 0.5 pct of lowest / highest values of estimated effect of marginal employment, for col (4) and (5) with 1 pct of lowest / highest values of estimated effect of marginal employment. Set of control variables includes: institutional context (countries); the kind of survey under consideration (average year, kind of JS surveyed); the type of marginal employment (short-full time, part-time work; industry); timeline (date of entry in ME, horizon considered after ME to evaluate); individual features of JS for whom the effect is estimated (age, gender, education, qualification); past experience of JS on job market (employability, benefit from UI); econometric identification (matching / timing of events; estimand); publication features (year; scope of research).

Table 5b. Multivariate Meta-Regression Analysis of the effect of marginal employment on finding a regular job. Part 2. *Weighted regressions (standardized variables).*

Explanatory variables / Specifications	(1)	(2)	(5)	(7)	(9)
Intercept	-0.6282967	-0.9625682	-0.4515558	-0.5175518	-0.5297714
	(0.232)	(0.626)	(0.399)	(0.393)	(0.393)
Precision of estimated impact of ME					
Inverted squared error of impact of ME	0.4516983	0.5425354***	0.5248477**	0.4627899**	0.4631274*
	(0.113)	(0.000)	(0.013)	(0.048)	(0.053)
Control variables:	Yes	Yes	Yes	Yes	Yes
Observations	476	476	468	447	447
R-squared	0.742	0.352	0.708	0.701	0.701

Source: Author's compilation (Table 1 and references) and computations.
Scope: all articles included in the meta-regression analysis, excluding at least observations with non-usable information of moderators or missing SE of estimated effects of marginal employment.
Notes: effect size is the explained variable. Estimated intercept is considered to test for funnel asymmetry; estimated coefficient for inverted standard error of estimated effect of marginal employment is used to test for genuine empirical effect. Robust p-value within parentheses, using standard errors clustered at the article level. *** (resp. ** or *) stands for significance at a 1% (resp. 5% or 10%) level. Precision concerning the sample considered: results for col (1) and (2) hold when excluding observations with missing values for SE of estimated effect of ME, for col (3) to (6) as well as with 0.5 pct of lowest / highest values of estimated effect of marginal employment, for col (7) to (9) with 1 pct of lowest / highest values of estimated effect of marginal employment. Set of control variables includes: institutional context (countries); the kind of survey under consideration (average year, kind of JS surveyed); the type of marginal employment (short-full time, part-time work; industry); timeline (date of entry in ME, horizon considered after ME to evaluate); individual features of JS for whom the effect is estimated (age, gender, education, qualification); past experience of JS on job market (employability, benefit from UI); econometric identification (matching / timing of events; estimand); publication features (year; scope of research).

6. Conclusion

In this article we consider policy that aim at bringing job seekers back to regular work. We focus on marginal employment, *i.e.* the possibility for job seekers to practice part-time or short fulltime employment. This device is considered as a mean to activate passive expenditure, while representing a way for JS to go back to regular full-time employment.

Indeed, at the end of the 2000s, there was a serious economic contraction, notably over the years 2008 and 2009. At the same time, there was a huge increase in full-time unemployment, as well as in the number of JS who practice marginal employment. A plunge in global trade was another sign of this economic situation. Worldwide, the volume of trade in goods and services fell by 12% in 2009, according to the WTO. One of the major consequences of flexibilization of labor markets since the 1980s has been the expansion of "special forms of employment", like fixed-term contracts (FTC), or temporary part-time work.

Marginal employment is a worldwide device that expands at the same time. In this context, an economic literature had developed to deal with the efficiency of marginal employment. From a theoretical point of view, several conditions to be met for the device to be successful. Empirical evidence shows also ambiguous findings.

The originality of our paper is to be complementary to existing empirical paper dealing with this matter. Exploiting heterogeneity of articles to run a meta-regression analysis using worldwide articles that aim at evaluating the effect of such a device on finding a regular job is used, we show that, in spite of heterogeneity of studies and of potential publication bias, this particular form of employment seems to enhance the probability of job seekers to go out of unemployment and find a regular job by about 30 percentage points.

Several studies evaluating the effects of reduced activity on the return to work of unemployed individuals also examine its effects on working conditions, particularly salary or the type of employment contract (fixed-term or permanent) characterizing the jobs of individuals leaving unemployment. In future research, it would be interesting to conduct an analysis in this area.

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Appendices.

Table A1. Summary statistics of dependent and explanatory variables of the meta-regression model.

Variable	Variable Description	Mean	Std. Dev.
Meta-dependent variable			
Estimated impact of ME	= Estimate of the effect of the marginal employment.	0.236	0.592
T-statics of estimated effect[a]	= Student t-statistic associated to the effect size.	1.935	7.011
Meta-independent variables			
Estimate's accuracy[b]	= Estimated standard error of effect of the marginal employment	1.092	1.88
Inverse of SE of ME's effect	= Inverted standard error (effect size precision).	12.897	19.152
Data sources:			
Average year of the survey:			
1993-1998	= 1, if the year of the survey is betw. 1993-1998.	0.186	0.389
1999-2000	= 1, if the year of the survey is betw. 1999-2000.	0.207	0.406
2001	= 1, if the year of the survey 2001.	0.274	0.446
2002-2012	= 1, if the year of the survey is 2002-2012.	0.333	0.472
Country of the survey:			
Austria	= 1, if the country of the survey is Austria.	0.088	0.283
Belgium	= 1, if the country of the survey is Belgium.	0.058	0.233
Denmark	= 1, if the country of the survey is Denmark.	0.085	0.28
Finland	= 1, if the country of the survey is Finland.	0.162	0.369
France	= 1, if the country of the survey is France.	0.267	0.443
Germany	= 1, if the country of the survey is Germany.	0.22	0.415
Norge	= 1, if the country of the survey is Norge.	0.017	0.13
Swiss	= 1, if the country of the survey is Austria.	0.103	0.304
Average age of the JS:		36.656	5.755
20-32 years old	= 1, if the average year of the JS in the survey is 20-32	0.224	0.418
34-37 years old	= 1, if the average year of the JS in the survey is 34-37	0.199	0.399
38 years old	*= 1, if the average year of the JS in the survey is 38*	*0.218*	*0.413*
39 years old	= 1, if the average year of the JS in the survey is 39	.156	.363
40-47 years old	= 1, if the average year of the JS in the survey is 40-47	0.203	0.403
Gender of JS in data:			
All kinds	= 1, if the survey covers mean and women	0.825	0.381
Women	= 1, if the survey covers only women	0.058	0.233
Men	= 1, if the survey covers only men	0.118	0.322
Kind of regular job: full-time equivalent	= 1, if the regular job is full-time equivalent	0.026	0.158
Type of Marginal Employment:			
Type of activity for ME:			
All kinds	= 1, if the ME refers to all non atypical jobs	0.519	0.5
Part-time work	= 1, if the ME refers to part-time work	0.4	0.49
Short fulltime work	= 1, if the ME refers to all non atypical jobs	0.081	0.273
Date for the start of the ME after beginning of the unemployment spell:			
All dates	= 1, if the ME begins at any date after beginning of US	0.598	0.491
0 to 6 months after	= 1, if the ME begins 0 to 6 months after beginning of US	0.231	0.422
7 to 12 months after	= 1, if the ME begins 7 to 12 months after beginning of US	0.115	0.32
13 to 36 months after	= 1, if the ME begins 13 to 36 months after beginning of US	0.056	0.229
Time horizon considered for measuring ME's effect:			
Short run	= 1, if the effect of ME is evaluated on the short run	0.201	0.401
Long run	= 1, if the effect of ME is evaluated on the long run	0.35	0.478
All time horizons	= 1, if the horizon at the effect of ME is evaluated is undefined	0.449	0.498
Type of JS for whom ME's effect is estimated:			
Gender of JS:			
All genders	= 1, if the effect of ME is evaluated for all gender	0.464	0.499
Men	= 1, if the effect of ME is evaluated for male JS	0.31	0.463
Women	= 1, if the effect of ME is evaluated for female JS	0.226	0.419

Age of JS:			
All ages	= 1, if the effect of ME is evaluated for JS of all ages	0.882	0.322
Junior (less than 30)	= 1, if the effect of ME is evaluated for JS younger than 30	0.073	0.260
Middle age	= 1, if the effect of ME is evaluated for JS aged 30 to 50	0.024	0.152
Senior (more than 50)	= 1, if the effect of ME is evaluated for JS older than 50	0.021	0.145
Socio-professional categories:			
All	= 1, if the effect of ME holds for JS from all SPC	0.962	0.193
Workers	= 1, if the effect of ME holds for workers only	0.009	0.092
Employees	= 1, if the effect of ME holds for employees only	0.009	0.092
Technicians	= 1, if the effect of ME holds for technicians only	0.006	0.08
Executives	= 1, if the effect of ME holds for executives only	0.009	0.092
Other qualification	= 1, if the effect of ME holds for other qualification	0.006	0.08
Education:			
Low education	= 1, if the effect of ME holds for JS with low education	0.019	0.137
High education	= 1, if the effect of ME holds for JS with high education	0.019	0.137
All education	= 1, if the effect of ME holds for JS from all education levels	0.962	0.193
Past E/U history:			
Employability			
Low employability	= 1, if the effect of ME holds for JS with large unemp. experience	0.009	0.092
High employability	= 1, if the effect of ME holds for JS with a short unemp. experience	0.011	0.103
All unemployment experiences	= 1, if the effect of ME holds for JS with all kinds of unemp. experience	0.981	0.137
Past earnings as employed:			
Low earnings	= 1, if the effect of ME holds for people with low earnings (past emp. exp.)	0.006	0.08
High earnings	= 1, if the effect of ME holds for people with high earnings (past emp. exp.)	0.004	0.065
All earnings	= 1, if the effect of ME holds for people with all earnings (past emp. exp.)	0.989	0.103
JS with or without UB:			
Receive benefits	= 1, if the effect of ME holds for JS with unemployment benefits	0.038	0.193
Not receive benefits	= 1, if the effect of ME holds for JS without unemployment benefits	0.036	0.187
All (receive benefits or not)	= 1, if the effect of ME holds for JS, indep. of receiving or not UB	0.925	0.263
Industry for ME:			
All industries	= 1, if the effect of ME holds for JS, indep. of the industry where he is looking for job	0.957	0.202
Metal industry	= 1, if the effect of ME holds for JS, who are looking for a job in metal Ind.	0.004	0.065
Manufacturing	= 1, if the effect of ME holds for JS, who are looking for a job in manuf. Ind.	0.006	0.08
Construction	= 1, if the effect of ME holds for JS, who are looking for a job in construction Ind.	0.009	0.092
Trade	= 1, if the effect of ME holds for JS, who are looking for a job in trade Ind.	0.006	0.08
Transportation	= 1, if the effect of ME holds for JS, who are looking for a job in transp. Ind.	0.002	0.046
Accommodation	= 1, if the effect of ME holds for JS, who are looking for a job in accomod. Ind.	0.002	0.046
Finance	= 1, if the effect of ME holds for JS, who are looking for a job in finance Ind.	0.002	0.046
Scientific and technical activities	= 1, if the effect of ME holds for JS, who are looking for a job in scientific and technical Ind.	0.002	0.046
Public services	= 1, if the effect of ME holds for JS, who are looking for a job in scientific and technical Ind.	0.002	0.046
Other industries	= 1, if the effect of ME holds for JS, who are looking for a job in other Ind.	0.006	0.008
Other services	= 1, if the effect of ME holds for JS, who are looking for a job in other Serv.	0.017	0.130
Transp./ Accommodation/ Finance / Scientific Activity	= 1, if the effect of ME holds for JS, who are looking for a job in transp., accomod., finance or scientific Ind.	0.009	0.092
Other services (red)	= 1, if the effect of ME holds for JS, who are looking for a job in other services ind.	0.009	0.092
Considered control variables:			
Duration dependence	= 1, if the considered article controls for duration dependence	0.605	0.489
Seasonal dummies	= 1, if the considered article includes as controls seasonal dummies	0.35	0.478
Gender	= 1, if the considered article controls for gender of JS	0.823	0.382
Nationality	= 1, if the considered article controls for nationality of JS	0.746	0.436
Mother tongue	= 1, if the considered article controls for mother tongue of JS	0.103	0.304
Number of children	= 1, if the considered article controls for number of children of JS	0.697	0.46
Health	= 1, if the considered article controls for health of JS	0.175	0.381
Education	= 1, if the considered article controls for education of JS	0.679	0.467
Qualification	= 1, if the considered article controls for qualification of JS	0.476	0.5
Industry of last job	= 1, if the considered article controls for industry of JS	0.714	0.453
Employment history (hours worked)	= 1, if the considered article controls for employment hist. of JS	0.784	0.412

Number of months as unemployed	= 1, if the considered article controls for number of months as unemployed	0.483	0.5
Being entitled to UI	= 1, if the considered article controls for JS being entitled to UI	0.647	0.478
Local labor market	= 1, if the considered article controls for local labour market	0.726	0.446
Local dummies	= 1, if the considered article includes local dummies	0.365	0.482
Local unemployment rate	= 1, if the considered article controls for local unmployment rates	0.545	0.499
No local controls	= 1, if the article did not consider local control variables	0.274	0.446
Estimator:			
Controlling for unobserved	= 1, if the article tries to control for unobserved heterogeneity	0.536	0.499
Type of model:			
Matching	= 1, if the article makes use of a matching estimator	0.419	0.494
Timing of events	= 1, if the article makes use of a timing of events strategy	0.545	0.499
Other estimators	= 1, if the article considers another kind of estimators	0.036	0.187
Estimand:		0.635	0.482
Average treatment effect	= 1, if the article aims at evaluating ATE		
Average treatment effect on the treated	= 1, if the article aims at evaluating ATET	0.365	0.482
Publication features:			
Year of publication			
1999-2010	= 1, if the year of publication of article is included in 1999-2010	0.321	0.467
2013-2016	= 1, if the year of publication of article is included in 2013-2016	0.389	0.488
2017-2021	= 1, if the year of publication of article is included in 2017-2021	0.291	0.455
1999-2008	= 1, if the year of publication of article is included in 1999-2008	0.141	0.348
2010	= 1, if the year of publication of article is 2010	0.179	0.384
2013	= 1, if the year of publication of article is 2013	0.165	0.371
2014-2016	= 1, if the year of publication of article is included in 2014-2016	0.224	0.418
2017-2021	= 1, if the year of publication of article is included in 2017-2021	0.291	0.455
Academic		*0.957*	*0.202*
Field of research:			
Labour area	= 1, if the research area is labour	0.363	0.481
General area	= 1, if the research area is a general branch	0.636	0.481

Source: Author's compilation (Table 1 and references) and computations.
Scope: 468 estimates provided by 16 articles that aim at evaluating the effect of a ME device, excluding observations for which some information is not usable and for which standard error of the effect size is unavailable.
Notes: [a]Ratio of estimated effect of ME to its standard error. [b]Standard error of estimated effect size.

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