Minimum income, insertion programmes, and labour market outcomes

Candidate Number: 4442L

Master of Philosophy in Economics
University of Cambridge

August, 2022

Word count: 9,868

Abstract

Utilising a rich set of data of recipients of the Portuguese minimum income scheme, this study investigates the labour market effects of participating in insertion programmes. This paper adds to the existing literature by addressing a complementary set of measures that do not specifically aim at activating individuals in the labour market. Making use of propensity score matching techniques, we identify a clear positive impact of job assistance measures on finding a new job. Medical assistance and training programmes, on the other hand, are found to delay entry into employment. We fail to find evidence of any significant effect of psychosocial support activities. The results are informative for future policy in Portugal and other countries.

Keywords: Minimum Income, Poverty, Labour Supply, Activation Programmes, Propensity Score Matching.

Contents

1 Introduction

In recent decades, the European welfare states have become the most comprehensive systems of social protection in the world (Bahle et al., 2011). Social insurance programmes, such as unemployment, sickness, and inability benefits, broadly protect citizens against income losses. Their eligibility is, nevertheless, time-restricted and they are normally linked to previous individual contributions into the system.

Despite this, poverty is still a harsh daily reality for many European citizens. In Portugal, before any social transfer, around 40% of the population is at risk of poverty.[1] Social assistance transfers, which are typically non-contributory, were therefore developed to protect households from poverty or compensate for higher expenditures (for instance, child benefits).

Means-tested, guaranteed minimum income (GMI) schemes were introduced in this context as the last safety net of any national social protection system. They generally aim at guaranteeing a politically-defined minimum income standard that only kicks in after all other social benefits are exhausted. The design of GMI schemes varies significantly across European countries in eligibility and coverage. The most common schemes, defined by Frazer et al. (2016) as "simple and comprehensive", cover all people with insufficient means to support themselves and are not confined to particular segments of the population.

Over the past twenty years, policymakers have defended a second pillar[2] to GMI schemes in an effort to mitigate the work disincentives associated with means-tested schemes while promoting social inclusion. In Portugal, this parallel function of the GMI was consummated with an activation programme that encourages integration into the labour market, community, and society through the provision of an insertion contract with several actions adapted to each beneficiary.

By exploiting these idiosyncratic characteristics of the Portuguese scheme, we aim to investigate the impact of the different insertion measures on the recipients' entry into employment. Non-random participation in the programmes implies that a mean difference of outcomes between treated and control group units cannot be used to infer causality in the corresponding treatment. We therefore implement a propensity score matching approach that re-establishes experimental conditions.

Our results indicate that, on average, job assistance programmes such as guidance

[1]Source: Statistics Portugal, 2020.

[2]See Commission Recommendation of 3 October 2008 on the active inclusion of people excluded from the labour market.

and monitoring have a strong positive impact on the exit from unemployment. Delays to entry in employment are found for participation in medical assistance and training programmes. These findings are consistent with the existing literature and are validated by sensitivity checks performed on the underlying assumptions and specification. The impact of psychosocial assistance strategies produces no significant results.

On the whole, we see the contribution of this paper to the literature as two-fold. First, the institutional framework of the Portuguese GMI scheme presents an ideal setting for the practical implementation of propensity score matching techniques. Unconfoundedness is highly plausible given that assignment to the insertion measures is driven by a set of household and individual characteristics observable to a caseworker.

Second, we fill a gap in the welfare literature by investigating the heterogeneity in insertion measures. Previous studies focused on the impacts of active labour market policies such as training programmes and guidance onto GMI recipients' labour market outcomes. We complement these by addressing a set of measures that aim at guaranteeing the basic pre-conditions of social participation.

The remainder of the paper proceeds as follows. Section 2 provides background into the Portuguese GMI scheme. Section 3 reviews the relevant literature within this topic. Section 4 describes the data and Section 5 outlines the methodology undertaken. Section 6 presents the empirical results and undertakes robustness checks. Section 7 discusses the strengths and weaknesses of our results. Finally, Section 8 concludes with a reference to avenues for further research.

2 Background

The Portuguese GMI is a non-contributory, means-tested social benefit that aims at reducing the intensity of poverty of the most deprived members of society. It is, therefore, an instrument of last resort that combines income support with promoting social integration and labour activation measures.

In 1996, the Portuguese Government introduced localized trials of a first minimum income scheme (*Rendimento Mínimo Garantido*) following the establishment of an European convergence strategy in social protection policies.[3] The programme was later expanded to the whole country in late 1997. In 2003, the GMI was reshaped into a "Social Integration Income" (*Rendimento Social de Inserção*) that placed greater

[3] European Council Recommendation 92/442/EEC of 27 July 1992.

emphasis on activation measures and restriction of entitlements. Since then, it has undergone several modifications.[4] The most significant reform was implemented in 2012 as part of the troika bailout programme which substantially decreased the generosity and accessibility of benefits.

2.1 Eligibility requirements

As most European GMI schemes, the Portuguese system is household-based. To be eligible to receive this social benefit, applicants must meet a plethora of requisites. First and foremost, there is an income constraint. Specifically, household income can't exceed the GMI reference value for each specific family unit. This value is computed following the modified-OECD equivalence scale that assigns a value of 1 to the household head, of 0.7 to each additional adult member and of 0.5 to each minor (<18 years old).[5] Table 1 presents the reference values for different household compositions based on the 2019 GMI value for a single adult household of €189.66.[6] Each minor adds €94.83 to the reference value while each additional adult increases the reference value by €132.76.

Table 1. GMI maximum values in 2019 by household composition.

Adults	**Minors**	**Reference Values**
One	-	€189.66
One	One	€284.49
One	Two	€379.32
Two	-	€322.42
Two	One	€417.25
Two	Two	€512.08
Two	Three	€609.91
Three	Five	€929.33

Household income is computed by adding 80% of all member's labour income after social security contributions and 100% of any income from other sources (for instance, rental income and pensions). For the first 12 months after returning to employment, a top-up exception reduces the labour income proportion to 50%. Furthermore, the

[4]See Pereirinha et al. (2020) for an overview.
[5]See OECD (2013) for an overview.
[6]As an example, a family consisting of two adults and two children will only be entitled to receive GMI as long as their combined income equals at most €189.66 $\times$ $(1 + 0.7 + 0.5 + 0.5) =$ €512.08.

applicant cannot hold financial assets (bank deposits, stocks, or bonds) that exceed 60 times the reference index for social support (€29,592).

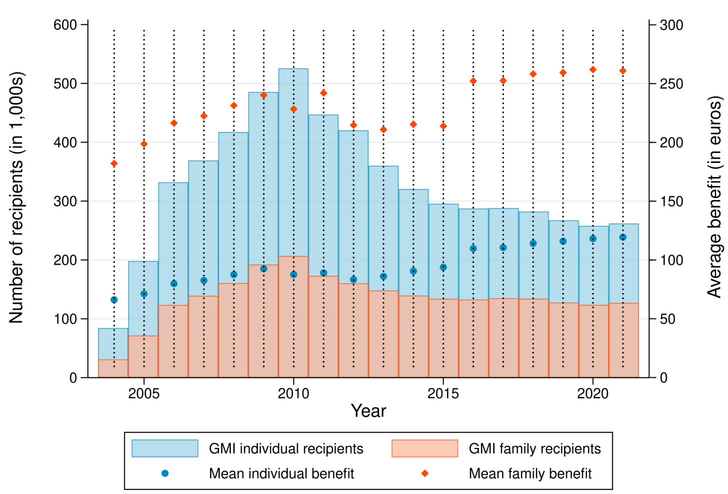
Secondly, the State requires all beneficiaries to sign and comply to an employment-insertion contract that defines a set of activation measures designed to promote social mobility and integration. All (able) unemployed recipients are also required to commit to being available to work by registering with the Employment and Vocational Training Institute (IEFP), the national employment registry. Failure to comply with the agreed activation measures or to accept convenient, socially valuable job offers leads to a temporary suspension of the GMI benefits that can last as long as 2 years.

Finally, applicants must reside in Portugal and be at least 18 years old.[7] The GMI is valid for twelve consecutive months and is automatically renewed for the same period if entitlement conditions are still met one month before the end of the contract.

2.2 Coverage

The value of the monetary allowance depends strictly on the number of people in the eligible household and their combined income as a unit. Specifically, the monthly GMI amounts to the difference between the successful applicant's household income and the appropriate GMI reference value.[8]

Figure 1. GMI recipients and average monthly benefit by type (SSI, 2022).[9]



[7]Non-EU citizens have a residence requisite of one year and the age restriction is relaxed under specific conditions, such as pregnancy and disabled care.

[8]If the same family's household income amounted to €300, they would receive on a monthly basis €512.08−€300 = €212.08 in the form of GMI.

Figure 1 shows the evolution of the caseload and average monthly allowances from 2004 on-wards. It has been marked by the three-fold influence of changes in macroeconomic conditions, institutional reforms, and changes introduced in unemployment benefits - the last safety net before GMI. During the sovereign debt crisis, important reforms were undertaken which resulted in a significant decline in the number of beneficiaries. Once these reforms were reverted in 2017, no change was made into the programme that significantly altered the number of beneficiaries or mean benefits. This is why we concentrate our analysis in the post-2017 period.

In 2019, the average nominal individual GMI benefit was €116.01, representing only 21.5% of the poverty line in the same year.[10] This low value reflects the large relative weight of underage individuals within GMI households - around 30%. Adding to the retirees and disabled, a substantial fraction of GMI recipients are not able to take up work. The programme shows gender-balanced participation levels and its incidence is more acute in the North and inland regions (Appendix A).

GMI benefits in Portugal are remarkably low in international comparison, reaching only 19% of the median income, below the OECD average of 23% (Coady et al., 2021). A no-income family unit moving from GMI benefits to a minimum wage increases monthly earnings by as much as 250%, which suggests that the disincentives for taking up work generated by the programme are arguably limited. On the other hand, since the Portuguese GMI is a differential allowance, recipients are subject to a 100% implicit tax rate on any additional income below the reference value, reducing any financial gain from taking up work. Similarly, around 35% of GMI recipients are entitled to other social security benefits, making the transition from welfare to work less likely given that it can have the effect of lowering the household's income. The compulsory insertion contract, the top-up scheme, and the strict sanctions were designed to limit these inactivity traps.

Overall, the Portuguese GMI provides very limited coverage of people vulnerable to poverty. In 2019, although 16.2% of the population was at risk of poverty, only around 2.5% were actually covered by the GMI. The strict eligibility conditions and low value of benefits partially explain this inadequacy, but the phenomenon of "non-take-up" seems to be equally important. Indeed, not all legally eligible individuals choose to sign up to the GMI benefit. The stigma attached to social welfare, participation costs and imperfect information on the benefit's availability are the main

[9] Monthly mean benefits calculated in December of every year.

[10] The poverty line is defined as 60% of median disposable income.

reasons for non-take-up of welfare benefits (Besley and Coate, 1992; Frazer et al., 2016). Simulations based on survey data suggest that around 28% of entitled families are not captured by the programme (Rodrigues, 2004). Crucially, non-take-up creates a self-selection process, since participation in the GMI programme is non-random. This endogeneity issue is, however, out of the scope of our analysis.

2.3 Insertion measures

Importantly, any GMI benefit receipt is conditional on the signature of an insertion contract (IC) between the prospective recipient and the welfare agency. The IC establishes a set of locally implemented actions that include, for instance, psychological support, short training courses, access to public housing, and family planning appointments. Its primary goal is to provide a path towards the recipients' self-sufficiency.

The process runs as follows. Once the GMI is approved by the public agency, a designated caseworker proposes a set of activation measures taking in consideration both individual and household-level characteristics. The content of the IC is therefore "tailor-made" to the recipients' observational needs. A short negotiation between the two sides then follows, which the caseworker generally dominates (SSI, 2019).

From a comparative standpoint, ICs under the Portuguese GMI scheme offer a wider range of activities that do not necessarily prescribe to the goals of the more common labour market activation policies. While getting recipients back to work is still the utmost goal of the insertion process, the programme also recognises that they may be deprived of basic rights such as access to education, health, and housing. This set of actions tries to deliver the basic pre-conditions of social integration.

Our study aims at exploring these two unique institutional features of the Portuguese GMI scheme. On the one hand, the content of the IC is arguably fully determined by the recipients' characteristics. On the other hand, the programme encompasses a wide-ranging scope of insertion measures that have not been properly addressed in the welfare programme impact literature.

[10] While selection into activation strategies is driven by the recipient's needs that are observed by the caseworker, the design of the IC is not fully deterministic. This is relevant in the context of our empirical strategy.

3 Literature review

The link between labour supply and welfare benefits is widely documented in the literature. Meghir and Phillips (2010) provide a broad review of empirical results concerning the sensitivity of labour supply to changes in work incentives. They find that the structure of the benefit system has heterogeneous effects across different categories of the population. Specifically, work disincentives are more likely to affect the work probability of low-skill men and secondary workers, particularly lone mothers and women with young children. For highly educated individuals, the sensitivity of hours of work to these disincentives is negligible. These findings have shed a light on the importance of studying social welfare programmes that specifically target the poor, including means-tested programmes.

The North American labour market has been the main focus of empirical studies on the effect of means-tested benefits on labour supply. Moffitt (2002) provides a survey on the impact of the Temporary Assistance for Needy Families (TANF, formerly AFDC) in the US, while Chan and Moffitt (2018) expand on this work by addressing the effects of recent reforms introduced in the programme. TANF recipients are shown to reduce hours of work by 10 to 50% relative to non-TANF levels. Expanding these impact evaluations to European income maintenance programmes is of particular interest, since they typically offer wider eligibility and more lenient time and work restrictions than the "new-world" counterparts. An example of these are the so-called guaranteed minimum income (GMI) schemes.[11]

The recent empirical literature has focused on quantifying the effects of GMI schemes on areas such as poverty reduction and labour market participation. Rodrigues (2004) took the first route by simulating the redistributive impacts of the Portuguese GMI scheme using the empirical framework of poverty reduction efficiency indices developed in Beckerman (1979). Similarly, Gorjón García and Villar (2019) reproduce the counterfactual household income of GMI recipients in the Basque Country, Spain, to measure such impacts. Overall, they find that each programme has a considerable effect on reducing the intensity and severity of poverty, albeit its incidence is only modestly attenuated.

On the effect of GMI benefits on labour market participation, however, there

[11] It is important to note that there is not a universal consensus around the definition of "guaranteed income". It is often characterized as a broad category of unconditional and unrestricted cash transfers that includes both targeted programmes and universal ones such as UBI. Other authors present guaranteed income as a means-tested, conditional transfer programme. In this paper, we follow the latter definition. See Berger Gonzalez and Bidadanure (2020) for a further discussion.

appears to be less consensus across the various studies. Clavet et al. (2013) provide an ex-ante assessment of a GMI proposal in Quebec and find strong negative impacts of the proposed scheme on labour participation rates, particularly among low-income earners. Terracol (2009) and Bargain and Doorley (2011) look into the French GMI programme (Revenu Minimum d'Insertion), which inspired most subsequent schemes in other countries, including Portugal. Terracol (2009) estimates a multivariate duration model and finds that the GMI programme adds 4 to 9 months to the recipient's unemployment duration. This sharp decrease is, however, limited to the first six months of receipt, with this deterrent effect quickly dropping to negligible levels afterwards. Bargain and Doorley (2011) take instead a regression discontinuity approach to their estimation of the benefit impact by exploiting the fact that individuals under the age of 25 are not eligible for the programme. They find significant evidence that GMI receipt reduces weekly hours of work at the threshold, a result that they attribute to the higher job search costs and lower earnings prospects of young, uneducated men. Conversely, Rica and Gorjón (2017) show that the Basque GMI scheme does not delay recipient's entry into employment. No ex-ante or ex-post assessment of the labour market outcomes of GMI recipients has ever been conducted in Portugal.

Our work is also related to the growing literature about the impact of activation measures on the supply of labour. Crépon and van den Berg (2016) and Card et al. (2017) synthesise research findings on the effectiveness of active labour market policies (ALMP). Evidence on the employment effects of ALMPs is mixed, particularly because these policies are normally implemented as a package, which makes it difficult to disentangle the impact of specific components. Returns on individual programmes have been widely heterogeneous. Research to date points to private sector employment programmes as being generally effective, but only in the medium to long term, with insignificant effects in the short term. Job-search assistance programmes show similar but also immediate positive impacts on unemployment exit and reemployment rates, particularly in Europe. In contrast, subsidised public work schemes have been proved to be mostly ineffective. Evidence on training programmes is mixed, with young unemployed workers being the most likely to be negatively affected. This is corroborated by Centeno et al. (2009), who analyse the effects of participation in two different job search programmes in Portugal, one of which especially targeting the youth. Results indicate that this programme prolonged the unemployment spells of participants compared to non-participants.

Only three studies have addressed the impact of activation measures in the context of GMI programmes. Ayala and Rodríguez (2006, 2010) focus on Madrid's GMI scheme and use propensity score methods to find that recidivism rates are considerably lower in activated households. ALMPs are also shown to be more effective in promoting reintegration to work than general social services such as family mediation and life skills. Rica and Gorjón (2017) apply the same methods for the Basque case and find heterogeneous but throughout positive impacts of ALMPs on job-finding rates. While guidance services increase the probability of finding a new job by around 20%, training programmes double it.

Other recent studies relate GMI protection to health outcomes. Nelson and Fritzell (2014) explores cross-sectional data on benefit levels in 18 high-income countries. They use a fixed effects pooled time-series regression to find that the generosity of minimum income benefits was strongly associated with mortality rates and life expectancy. Specifically, countries that provided more comprehensive GMI benefits had better population health. Jutz (2021) builds on this result by investigating the extent to which more generous GMI benefits reduce health inequalities. This study finds that differences in health outcomes between income groups are not sensitive to higher benefit levels. To the best of our knowledge, no study in the existing literature examines the impact of health-related insertion measures on GMI recipients' labour market participation.

4 Data

Our data set consists of the administrative records of the full universe of household's heads of the Portuguese GMI programme from January 2018 to December 2019.[12] This information was provided by the Social Security Institute (*Instituto de Segurança Social*, SSI), which is the public agency responsible for the management of the system of welfare benefits in Portugal. Our paper presents the first empirical application of this data.

This rich database includes all the individual and household-level information provided by each potential recipient when applying for the GMI benefit, including standard demographic characteristics such as gender, age, educational attainment, nationality, place of residence, household size, and number of children. This set of

[12] In this study, we consider the term "GMI recipient" analogous to household head, although the latter is only a subgroup of the first.

characteristics is updated by the designated caseworker on a monthly basis.

Previous studies have captured the unemployment status using the unemployment benefit receipt. However, we argue that this strategy does not provide an accurate representation of those who are unemployed. In most countries, entitlement to unemployment benefits is constricted to a time period and to workers who meet the specific requirements (Bahle et al., 2011). Therefore, by circumscribing the unemployment status to whether individuals receive unemployment benefits, these studies do not account for those who can't or choose not to take up benefits while being engaged in a job search.

The Social Security Institute provides exact information on any change that occurs on the recipients' Social Security status. This means that we know when and for how long any given individual in our sample takes up Social Security benefits (including unemployment, disability, and GMI) while possibly contributing to the system as active workers. We therefore pin down unemployed workers as those who, for the majority of a given month, did not have any (disclosed) labour income. We also gather information on disability, parental leave, duration of GMI spells, and the average value of benefits from this registry.

The final set of characteristics was gathered from the insertion contracts that every household head has to sign in order to receive the GMI. This includes variables that have been highlighted in previous studies as fundamental for analysing welfare recipients (Goerge and Lee, 2002), including the existence of structural problems within the household such as drug and alcohol abuse, HIV diagnosis or mental disorders (hereafter referred to as "social problems"). From these contracts we also recover every insertion measure that GMI recipients have received in the past 2 years.

We began with a sample of 216,537 individual household heads. Since our aim is to study the impacts of the activation measures on exit rates to employment, we restrict our sample to individuals below the age of 60. This procedure eliminates approximately 45,000 individuals who we consider to be too old to take up work.

We then introduced a further truncation to our data. Educational attainment is a central independent variable, as it is likely to drive selection into treatment. For instance, we argue that individuals who have no qualification are more prone to participate in training programmes. Unfortunately, the SSI data set does not collect this information on a significant portion of recipients. From contacts with several caseworkers, we understood that this information is generally dismissed because any educational level needs to be verified, which entails going through a long, bureaucratic process. Consequently, any restriction would likely lead to an over-representation of either illiterate or unqualified people in our sample, who aren't subject to any verification. A technical report from the SSI (2019) dismisses this claim, since low educational attainment is present at similar rates in our sample and in the population.

The database resulting from the cleaning of administrative data provides information on 8,532 individuals. A descriptive analysis of our final sample is provided in Appendix B for December 2018. The data on gender show a prevalence of female household heads. Most individuals are middle-aged and, as expected, possess no to low education levels. As for the mean household benefit, almost 80% of the recipients receive a monthly GMI below €400, which does not cover the poverty line. The frequencies of the GMI spells' duration signal the persistence of poverty traps. As for household size, small households are more prevalent in our sample. More than 40% of the recipients are childless, although the number of single-parent households stands out (21.2%). Foreign nationals, the disabled and people with social problems are an almost negligible part of our sample.

Moreover, 98.9% of the households in our sample were activated by some kind of insertion measure. This is substantially more than what analogous programmes in Spain and Italy register (Ayala and Rodríguez, 2006; Brunori et al., 2010, around 40%). Table 2 characterizes and divides these insertion contract treatments in eight groups.[13] Four of these are disproportionately more participated than the others: job assistance, medical assistance, psychosocial support, and training programmes. We restrict our analysis to these groups of IC measures for sample size reasons.

Table 2. Frequencies of insertion contract treatments, 2018.

Insertion measures	**Percent**
Medical assistance | GP appointments, preventive health services, family planning	40.7
Psychosocial support | general counselling, management of daily routines	38.1
Socially useful activities | community projects in heritage and environmental protection	5.8
Schooling | literacy training, school tutoring, grants for adult learners	4.2
Job assistance | employment planning, self-employment support, other ALMPs	75.2
Training programmes | publicly-provided professional training courses, apprenticeships	20.6
Housing actions | access to public housing, support in home improvement and rehousing	8.7
Poverty alleviation | access to food banks, community centres, temporary housing	5.2

[13]Note that households can take part in more than one insertion measure - in fact, 56.3% of them do.

Regrettably, our data set does not contain information on previous work experience, occupational searches, and household income. This information would be particularly important to measure the impact of the GMI scheme itself on the probability of exiting unemployment. This initial plan could not be materialised because the IEFP, our planned complementary data source, did not provide this information in time for this study. In section 8, we address what future research may look into when this data becomes accessible.

5 Methodology

The aim of this paper is to assess the impact of different insertion contract programmes on the probability of exiting unemployment. The outcome variable Y_i takes the value of 1 if the unemployed GMI recipient finds a job in the next month and 0 otherwise. We take participation in insertion measures as the treatment effect. The binary independent variable of interest D_i takes the value of 1 if individual i has received a specific activation measure in the past year.[14] This follows from recent evidence in the literature that showed that the labour market effects of activation programmes are more significant 6 months after their full implementation.

5.1 Endogeneity issues

The main threat to internal validity in this paper is sample selection, which stems from the fact that the characteristics of the unemployed GMI recipients differ broadly across activation measures. This means that participation into treatment is non-random. GMI recipients who undertake a specific insertion strategy are systematically different from those who do not. Table A1 shows the distribution of the characteristics of the GMI recipients in the sample and compares it to the treated subsamples. There are notorious differences between the two groups, particularly in variables such as household size and number of children. Table 3 provides a further example of this. Looking at the "unmatched" panel, one can observe significant differences between participants in psychosocial support measures and non-participants in almost all covariates.

Our parameter of interest is the average treatment effect on the treated (ATT), which measures the impact of a given insertion programme on those individuals who

[14] We are unable to perform a duration analysis on the programmes' impacts since the end date of insertion measures of GMI recipients is hardly ever registered in the data.

participated. Using the potential outcomes framework, ATT can be expressed as

$$\tau_{ATT} = \mathbb{E}\left(Y_1 - Y_0 | D = 1\right) = \mathbb{E}\left(Y_1 | D = 1\right) - \mathbb{E}\left(Y_0 | D = 1\right). \tag{1}$$

The fundamental problem in this parameter is that we do not observe $\mathbb{E}\left(Y_0 | D = 1\right)$, that is, the average employment outcome of treated individuals had they not participated in the insertion programme. The observed $\mathbb{E}\left(Y_0 | D = 0\right)$ is not a good counterfactual of $\mathbb{E}\left(Y_0 | D = 1\right)$ since participants are not randomly assigned into treatment. Thus, running a simple regression model will lead to a biased estimate of the ATT.[15]

Table B1 shows the results of a pooled probit regression model with month and region fixed-effects for the probability of finding a job. It includes participation in the insertion measures of interest among the covariates potentially associated with higher unemployment exit rates. Participation in medical assistance and psychosocial support activities is found to be negatively correlated with job-finding probability. Perhaps surprisingly, the same result is found for training programmes. The only activation measure that is associated with higher unemployment exit rates is job assistance. The effects of other covariates are well defined and show the expected signs, with the exception of age.

These effects, however, cannot be interpreted as causal. In the absence of an experimental design, we must employ an empirical strategy that accounts for differences between the treated and control groups in order to properly estimate the impact of each insertion contract programme.

5.2 Assumptions

In past decades, welfare programme evaluations have extensively used matching econometric estimators to address this endogeneity issue. A literature based on experimental and non-experimental studies has shown the success of these matching methods in producing valid estimates of treatment effects, while recognizing some important shortcomings (Dehejia and Wahba, 2002).

The fundamental idea behind matching is straightforward. To avoid the selection bias, matching techniques find a control unit that is similar to a treated unit, allowing an estimate of the treatment's impact as the difference between a participant and their corresponding matched case. These methods rely, however, on an important assumption known as conditional independence (Rubin, 1977, also referred to

[15] $\Delta = \mathbb{E}\left(Y_1 | D = 1\right) - \mathbb{E}\left(Y_0 | D = 0\right) = \mathbb{E}\left(Y_1 - Y_0 | D = 1\right) - \mathbb{E}\left(Y_1 - Y_0 | D = 0\right) = \tau_{ATT} + SB.$

as unconfoundedness or selection on observables). For each unit, there is a vector of observable covariates X_i such that, after controlling for them, the potential outcomes are independent of the treatment status. In other words, once covariates are accounted for, the assignment of treatment is "as good as random".

$$(Y_{i1}, Y_{i0}) \perp D_i | X_i, \quad \forall_i \tag{2}$$

One limitation of matching methods is that they require a sufficiently rich comparison group. As the number of covariates increases, so does the difficulty in finding exact matches. Rosenbaum and Rubin (1983) suggested the use of the propensity score - the probability of receiving treatment conditional on covariates - to reduce this dimensionality problem.[16] Propensity score matching relies on the common support condition (also known as overlap), which requires

$$0 < p(X_i) < 1, \quad \text{with } p(X_i) \equiv \Pr(D = 1 | X_i = x), \tag{3}$$

that is, the probability of receiving treatment for each X_i lies between 0 and 1. When both (2) and (3) are satisfied, the treatment assignment is said to be strongly ignorable, and the ATT can be estimated as

$$\tau_{ATT} = \mathbb{E}\Big(\mathbb{E}\left(Y_1 | D = 1, p(X_i)\right) - \mathbb{E}\left(Y_0 | D = 0, p(X_i) | D = 1\right)\Big). \tag{4}$$

Therefore, within subpopulations with the same propensity score, covariates are independent of the treatment variable and cannot lead to biases. When all relevant differences between treated and untreated individuals are captured in the observed covariates, PSM can yield a consistent estimate of the treatment effect.

The validity of these assumptions, however, is often controversial. Violations of the common support assumption are testable and easily detectable. The unconfoundness assumption, however, cannot be tested empirically. We address the plausibility of these assumptions in the context of our analysis in Section 6.1.

5.3 Empirical strategy

We use propensity score matching methods to estimate the labour market effects on GMI recipients of four insertion measures: medical assistance, psychosocial support,

[16] Note that PSM allows for matching to be performed on a single variable - the propensity score $p(Xi)$ - instead of an entire set of covariates.

job assistance, and training programmes. We closely follow Caliendo and Kopeinig (2008) in the implementation of this empirical strategy.

We begin by estimating the propensity scores. We run a probit model with a specification that includes 13 covariates: the household head's age, educational attainment, GMI spell, household size, number of children, average household benefit, and a set of binary variables that include whether the recipient is a single-parent, foreigner or disabled, gender, and whether the household has social problems or the recipient was on parental leave in the past 6 months. Crucially, these variables are used by caseworkers to decide the set of insertion measures that each recipient is subject to as part of their insertion contract. Hence, they effectively determine participation, without being affected by the treatment.[17]

These predicted probabilities are then ranked from lowest to highest to define a valid comparison group for the treated population. This counterfactual group is constructed through the trimming of the extreme observations that find no substantial match in terms of propensity scores to treated units. Hence, some observations may be lost, so as to find a common support (or overlap) region in which covariate balance requirements are satisfied.

After creating a balanced score, the next step is choosing a matching algorithm that assigns treated units to untreated ones on the basis of the propensity score. These algorithms differ not only in the way neighbourhood matches and the common support region are defined, but also how the weights that balance the sample are constructed. This choice entails a trade-off between efficiency and bias.[18] When treated individuals are matched to a unique individual in the comparison group that is closest in terms of propensity score (known as nearest neighbour), the match is always best and will generally lead to the least biased estimates. However, this also potentially reduces the number of nonparticipants used in matching, which then increases the variance of the estimator. Based on these trade-offs, we implemented five different estimators to ascertain the robustness of our estimated treatment effects.

Once participants are adequately matched to non-participants, the average treatment effect on the treated can be calculated. PSM defines the ATT as the mean difference in outcomes over the common support region where the comparison units are weighted by the distribution of propensity scores of the treated ones. Explicitly,

[17] See Imbens (2015) for a further discussion on covariate selection, including the threat of overspecification.

[18] Huber et al. (2013) provide a thorough evaluation of different matching techniques.

the treatment effect can be written as follows (see A. Smith and E. Todd, 2005):

$$\tau_{ATT} = \frac{1}{N_T}\left[\sum_{i\in T} Y_j^T - \sum_{k\in C(j)} w_{jk} Y_k^C\right], \tag{5}$$

where N_T is the number of treated individuals j, $C(j)$ is a set of control units matched to treated unit j, and w_{jk} is the weight that aggregates outcomes for the matched control-group individuals k. The nearest-neighbour matching estimator defines $C_j = \min_j \left\| p(X_j) - p(X_k) \right\|$. In an attempt of avoiding "bad" matches (the minimization may still yield matched units with substantially different propensity scores), Caliper matching deviates from the previous method by requiring $\left\| p(X_j) - p(X_k) \right\| < \varepsilon$, where ε is a specific level of tolerance for the bias. Finally, non-parametric estimators such as Kernel matching use a kernel-weighted average over several units in the comparison group to construct a match for each participant in the programme.

6 Empirical findings

6.1 Preliminary checks

As discussed above, the validity of PSM relies on two crucial assumptions. Conditional independence implies that only observed factors affect the assignment of treatment. Common support, on the other hand, ensures that there is sufficient overlap in propensity scores across treated and untreated individuals.

Conditional independence is a strong assumption and its validity cannot be tested directly. If unobserved factors affect assignment into treatment, then this assumption is violated and the researcher should rely in different identification strategies such as difference-in-differences and instrumental variables estimators. Although statistical tests are not available, one should be able to make a rhetorical case for why this condition is plausible. In the context of our study, we argue that this condition holds because a separate decision-maker with limited information on participants decides on treatment outcome.[19] Specifically, we know that selection is determined by caseworkers who, based on a set of characteristics, decide which insertion programmes

[19] An early example of this is provided by Angrist (1998), who studied the effect of voluntary military service on future earnings. He argues that by observing the main characteristics of the candidates that the military uses to screen and select individuals (age, schooling, test scores), any remaining differences between veterans and non-veterans are ignorable.

that family will take part in. Hence, this decision strictly depends on the variables available in our rich data.

Furthermore, possible logical unobserved confounders (motivation to take up training programmes, for instance) are highly correlated with the covariates in our model (educational attainment) and are, therefore, indirectly controlled for. Further analyisis on post-match balance across covariates also supports the plausibility of this condition (see subsection 6.1). To the extent, as we argue, that the principle remaining sources of bias in participants/non-participants comparisons are differences in the variables used by Social Security caseworkers to assign individuals into different insertion measures, the assumption that any programme participation is ignorable conditional on predetermined covariates is justified.

Verifying the common support condition is an easier task. Several ways are suggested in the literature, including checking whether the distributions of the propensity scores for the treated and untreated groups overlap before any matching occurs.[20] Figure C1 enables this exact assessment. On the left side of panel C1a, the propensity scores for medical assistance are plotted according to their density before any matching is performed. Besides overlapping extensively, the density plot shows that the propensity scores have a similar distribution in the treatment and control groups. The propensity scores of the remaining insertion programmes also show this feature.

Imbens (2004) also suggests testing whether the mean propensity score is equivalent in the treatment and control groups within each of five quintiles. If it is not, quintiles can be split into smaller blocks. If this equivalence is not eventually found in smaller blocks, then either the functional form or the set of covariates need to change. In the medical assistance case, balance is achieved after 7 splits, leaving us with a total of 12 blocks. A minima and maxima comparison[21] was also carried out but no significant differences were found between the two groups. Hence, there seems to be sufficient overlap in the characteristics of treated and untreated individuals to find appropriate matches and define a wide region of common support.

Since we condition on propensity scores (instead of actual covariates), we also need to check whether the matching procedure was successful in balancing the independent variables is both the treatment and control group. Estimates for the effects of the insertion measures on the recipients' labour market outcomes originated through

[20] Lechner (2008) argues that the bullet-proof nature of this visual analysis nullifies the need for alternative complicated estimators. Caliendo and Kopeinig (2008) present however two alternative methods that aim to determine the region of common support with greater precision.

[21] See Caliendo and Kopeinig (2008) for an overview.

PSM are only reliable to the extent that the matching process produces a credible comparison group. Figure C1 provides an initial visual assessment of this by comparing the distribution of propensity scores pre and post-matching. The solid and dashed lines largely coincide after observations are matched to each other, suggesting that the matching algorithm succeeded in making the distributions of the treated and untreated groups more identical.

Several other procedures are suggested in the literature. The following balancing tests were performed considering a one-to-one nearest neighbour estimator. One suitable indicator of balance is the standardized difference of means, which is defined as the difference of covariate means in the treated and matched control samples as a percentage of the pooled standard deviation of the two groups. Table 3 shows the standardized bias across covariates before and after matching for the insertion measure "psychosocial support". The standardized difference in the means of the gender indicator descreased from -31.5% in the unmatched sample to -0.5% in the matched sample, becoming statistically insignificant. All remaining covariates show a standardized bias below 5% after matching, a level that is seen as sufficient in most empirical studies (Rosenbaum and Rubin, 1985). Figure C2 provides a plot of this balance diagnostic for the remaining insertion measures pre and post-matching as well as the variance ratio of each covariate. The ratio of treatment and matched control variances should be near 1 if the two groups are to be balanced (Rubin, 2001). As expected, the matching procedure was extremely successful in reducing the systematic differences in means and variances of participants and non-participants across all insertion measures. A subsequent comparison of the pseudo-R^2s before and after matching was also supportive of post-matching balance. The low values of the pseudo-R^2s after matching indicated that the covariates no longer explained the probability of participation (see Sianesi, 2004), as balance was achieved.

Table 3. Covariate balance of means across treatment and control groups before and after matching - psychosocial support.

	Unmatched			Matched		
Variable	Treated	Control	SD (%)	Treated	Control	SD (%)
Female	0.624	0.767	-31.5***	0.624	0.626	-0.5
Foreign nationals	0.018	0.013	4.2*	0.018	0.015	3.5
Parental leave	0.080	0.111	-10.7***	0.080	0.081	-0.2
Disabled person	0.033	0.017	10.0***	0.033	0.026	4.9
Social problems	0.021	0.005	13.8***	0.021	0.021	0.5
Single parent	0.146	0.252	-26.6***	0.146	0.146	0.0
Age						
<30	0.056	0.072	-6.5***	0.056	0.052	1.5
30-39	0.177	0.267	-21.6***	0.177	0.178	-0.1
40-49	0.297	0.329	-7.0***	0.297	0.291	1.3
50-60	0.470	0.332	28.3***	0.470	0.479	-2.0
Educational attainment						
Illiterate	0.091	0.010	-2.9	0.091	0.092	-0.1
No qualifications	0.037	0.045	-4.1*	0.037	0.092	1.6
Primary school	0.332	0.027	-14.3***	0.008	0.007	0.4
Middle school	0.086	0.074	4.4**	0.086	0.077	3.4
Secondary school	0.431	0.454	-4.6**	0.431	0.448	-3.4
University	0.023	0.017	4.2*	0.023	0.021	0.4
Mean household benefit						
<€200	0.653	0.313	72.4***	0.653	0.651	0.4
€200-€400	0.209	0.423	-47.4***	0.209	0.210	-0.2
€400-€600	0.102	0.203	-28.5***	0.102	0.101	0.1
€600+	0.037	0.061	-11.3***	0.037	0.038	-0.6
GMI spell						
<2	0.339	0.298	8.8***	0.339	0.333	1.1
2-4 years	0.230	0.239	-2.1	0.230	0.222	2.0
4-6 years	0.145	0.151	-1.6	0.145	0.149	-1.2
6+ years	0.286	0.313	-5.7*	0.286	0.300	-1.2
Household size						
1	0.550	0.134	97.6***	0.550	0.550	0.0
2-3	0.227	0.424	-43.1***	0.227	0.229	-0.5
4-5	0.153	0.326	-41.6***	0.153	0.150	0.7
6+	0.071	0.115	-15.5***	0.070	0.071	-0.1
Number of children						
0	0.622	0.275	74.5***	0.622	0.624	-0.5
1	0.130	0.256	-32.2***	0.130	0.127	0.8
2-3	0.179	0.369	-43.7***	0.179	0.180	0.3
4+	0.069	0.100	-11.2***	0.069	0.068	0.1
N	(3,246)	(5,286)		(3,246)	(5,286)	

SD stands for standardized difference, which may be significant at * 10%, ** 5%, or *** 1%.

6.2 Treatment effects

The regression estimates in Table B1 provide a benchmark against which we can assess the five sets of matching estimates given in Table 4. The matching is performed using different algorithms to infer the robustness of our results to different specifications. Columns (1), (2), and (3) were obtained using the nearest neighbour method with a different number of matches. In Column (2), estimates were obtained without replacement of units, meaning that each unique comparison group individual was only matched once. Column (4) applied a tolerance of 0.001 to the maximum propensity score distance. Finally, column (5) was calculated using a non-parametric Kernel estimator that uses a weighted average of all nonparticipants to find an adequate counterfactual match.[22]

Table 4 reports the estimated average treatment effect on the treated as the difference between the treated units' outcomes and what these would have been had they not been subject to treatment. It also presents the number of treated and control group individuals used in the matching procedure that result from the restrictions imposed on the common support regions by the different estimators.

Overall, the matching estimates are consistent with the tendencies identified in the regression results. These simple comparisons of labour market outcomes, however, seem to overestimate the absolute value of the impact of the activation measures on employment. Still, the differences observed in job-finding rates between participants and nonparticipants in the insertion measures are not fully eliminated once the differences between the two groups are accounted for (selection bias), suggesting that at least a part of these differences stem from the policies.

The estimates are found to be highly significant across the different matching estimators and insertion measures, with the notable exception of psychosocial support. They do not appear to depend crucially on the methodology chosen, since they remain very similar as different parameters and algorithms were applied.

Job assistance is the only factor with a positive impact on the unemployment exit of GMI recipients. The probability of finding a job twelve months after participating in job assistance activities increases in a range of 4.7 to 6.2 percentage points, depending on the algorithm used. The evidence therefore suggests that ALMPs that

[22] The estimation method of the standard errors differs across matching algorithms. Bootstrap estimation is performed only in column (5) using 50 replications. For nearest neighbour matching with or without a caliper, it has been shown that the bootstrap method yields inconsistent results (Abadie and Imbens, 2008). Instead, we provide robust Abadie-Imbens standard errors as developed in Abadie and Imbens (2016).

involve employment planning and guidance made low-income households more likely to participate in the labour market as active employees.

Table 4. Effects of insertion measures on the probability of finding a job.

	(1) NN	(2) NNwor	(3) NN5	(4) Caliper	(5) Kernel
			A. Medical assistance		
ATT	-0.0830***	-0.0841***	-0.0882***	-0.0842***	-0.0890***
	(0.0119)	(0098)	(0.0112)	(0.0112)	(0.0095)
Treatment	3471	3471	3471	3110	3425
Control	3601	3471	4360	4209	5003
			B. Training programmes		
ATT	-0.0593***	-0.0512***	-0.0474***	-0.0479***	-0.0522***
	(0.0127)	(0137)	(0.0113)	(0.0114)	(0.0121)
Treatment	1757	1757	1757	1737	1739
Control	4635	1757	5550	5454	6354
			B. Job assistance		
ATT	0.0470***	0.0624***	0.0464***	0.0473***	0.0521***
	(0.0139)	(0.0121)	(0.0125)	(0.0125)	(0.0106)
Treatment	6410	2116	6410	5981	6264
Control	1941	2116	2070	1942	2018
			B. Psychosocial support		
ATT	-0.0127	-0.0154	-0.0168	-0.0164	-0.0229*
	(0.0131)	(0.0102)	(0.0126)	(0.120)	(0.0119)
Treatment	3246	3246	3246	3073	3204
Control	3555	3246	4467	4361	5239
N	8532	8532	8532	8532	8532

Each column reports matching estimates with a different algorithm: (1) one-to-one nearest neighbour with replacement, (2) one-to-one nearest neighbour without replacement, (3) one-to-five nearest neighbours with replacement, (4) caliper of 0.001, (5) Kernel matching using Gaussian density function. Robust standard errors in parentheses, bootstrapped in (5). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Conversely, individuals who use training programmes decrease their likelihood of finding a job by around 5 percentage points. Participants in medical assistance activities are also found to be less likely to exit unemployment after completing the programme, delaying their job entry by around 9 percentage points. The negative impacts of psychosocial support measures are less pronounced, although they are mostly insignificant.

6.3 Sensitivity analysis

As discussed in Section 5, if there are unobserved factors that simultaneously affect selection into treatment and the outcome, a "hidden bias" arises to which our matching estimates are not robust. To investigate the sensitivity of our results to minor violations of this unconfoundedness assumption, we take a bounding approach as suggested by Rosenbaum et al. (2010).[23] It is important to note that this sensitivity check does not test the conditional independence assumption itself, but only the extent to which our results are sensitive to it.

If a study is free of hidden bias, as we argued, then individuals with the same set of covariates are perfectly matched and should have the same probability of receiving treatment. This is not the case under unmeasured confounding: because of unobserved variables, these individuals are not comparable. We test the sensitivity of the three impact estimates found to be significant using Rosenbaums bounds. This method relies on individuals with the same set of covariates that, for some immeasurable reason, differ on their odds of receiving the treatment.

Table 5 presents the p-value results for gamma values between 1 and 2, in steps of 0.1, after a 5th nearest neighbour matching was implemented. The indicator Γ measures how much the odds of GMI recipients participating in an activation measure would need to change due to unobservables for our matching results to change. We look at the upper significance level in the case of job assistance because it is the only activation measure with a positive estimated impact on unemployment exit.

If the assumption of no hidden bias ($\Gamma = 1$) is to be believed, then the results are highly significant throughout. As conditional independence is relaxed($\Gamma > 1$), our estimates become less and less statistically significant. The study of the impact of the training programmes becomes sensitive at about $\Gamma = 1.9$, while the study of the effect of job assistance measures becomes sensitive at $\Gamma = 1.7$. Our estimated result is

[23] See Nannicini (2007) for an alternative sensitivity check.

insensitive to a bias that would double the odds of participation in medical assistance activities but sensitive to a bias that would triple these odds. A smaller hidden bias could explain away the effects of job assistance, but only a larger bias could explain away the effects of medical assistance. Overall, our results present some degree of robustness to unmeasured confounding.

Table 5. Rosenbaum bounds' significance values.

	p-values		
Γ	Medical assistance	Training programmes	Job assitance
1.0	<0.0001	<0.0001	<0.0001
1.1	<0.0001	<0.0001	<0.0001
1.2	<0.0001	<0.0001	<0.0001
1.3	<0.0001	<0.0001	<0.0001
1.4	<0.0001	<0.0001	0.0003
1.5	<0.0001	<0.0001	0.0016
1.6	<0.0001	0.0001	0.0486
1.7	<0.0001	0.0037	0.2175
1.8	<0.0001	0.0438	0.3243
1.9	0.0001	0.2086	0.4474
2.0	0.0050	0.5197	0.6312

7 Discussion

Our results find some support in the literature. The comparison with the results of Rica and Gorjón (2017) for job assistance is particularly instructive. Comfort is gained from similar findings of a substantial positive impact on employment entry of GMI recipients. In other contexts, the existing literature is overwhelmingly supportive of the positive effects of job assistance programmes (Caliendo and Schmidl, 2016). However, there are some differences in our results, such as a decrease in probability of finding a job after undertaking a training programme.

This effect in particular needs to be reconciled in future research. Evaluations of active labour market policies have shown mixed results with respect to training programmes (Martin and Grubb, 2001). For the youth, results are predominantly negative. This hypothesis lead us to investigate the possible heterogeneous effects across age groups. Indeed, the youth is overrepresented in this subsample (around

12%, compared to the sample's 6.6%). Although not presented, we found that young individuals who used training programmes decrease their likelihood of finding a job by around 8 percentage points, almost doubling our estimate for the whole population. As expected, this effect dissipates as we move up in age groups, until it becomes insignificant in the last group. Further research should assess these impacts on different demographic groups, possibly across gender and educational attainment.

With regards to the results on medical assistance or psychosocial support, we were not able to draw any comparison in the literature. This study therefore provides a benchmark for these estimates that future studies might compare to.

The use of propensity score matching presents some natural merits (few constraints imposed on the functional form), but also some caveats. As in any estimation method, our results are valid to the extent that our assumptions hold. We argued that selection bias from unobserved factors is likely to be negligible in our context. Nevertheless, this is still a strong, untestable assumption. While in Section 6.3 we found that our results are quite robust to deviations from conditional independence, the usual caveats associated with propensity score matching should be noted.

It is also important to remember that we extract the activation measures from the household-defined insertion contracts. As a consequence, some individuals might be linked to insertion measures that they did not receive, but only family members. Similarly, our identification strategy for the unemployed (by linking unemployment status to non-employment), while having the advantage of accounting for those out-of-benefits, may incorrectly identify persons outside the labour force as unemployed, even though they are not actively looking for a job. A more thorough assessment would rely on public employment registries. Unfortunately, getting hold of such data proved very difficult.

Finally, although the data set used in this analysis provides extremely rich, never-before-used information on GMI recipients that avoids the biases associated with surveys, it suffers from lack of precision in one fundamental variable: educational level. IEFP records, if made available to us in time, would have solved this issue. A larger sample would also enable the analysis of a greater level of heterogeneity in the insertion measures groups, not only those defined in Table 2 that were not considered in our study, but particularly within those that were analysed.

8 Conclusion

Guaranteed minimum schemes in Europe have progressively adopted a two-fold approach to welfare provision that reconciles transfers of a basic level of income and measures aimed to improve the labour market participation of the low-income recipients. In this paper, we analyse the extent to which these insertion measures fulfil their goal in the context of the Portuguese minimum income scheme.

The idiosyncrasies of the Portuguese GMI programme provide a classic setting to perform a propensity score matching analysis. Our results suggest that job assistance activities are very effective in increasing the recipients' labour market participation. We show that exits from unemployment are 4.7 to 6.2 p.p. more likely after receiving a job assistance measure. Conversely, medical assistance and training programmes are found to decrease the likelihood of finding a job by around 9 and 5 percentage points, respectively. These findings are robust to different matching estimators and to the relaxation of the conditional independence assumption. No significant results are found for the effect of psychosocial assistance. To the best of our knowledge, this is the first study that carries out an analysis of non-ALMPs on the entry to employment.

These findings offer some important policy recommendations. As a policy device, job assistance programmes such as employment planning should be expanded in the GMI population, as they significantly help recipients to exit unemployment and put them on the path of welfare independence. The medical assistance and training programmes should be reassessed, as they are found to worsen labour market outcomes.

Our analysis opens up a number of potentially important contributions to the impact evaluation of insertion measures. A duration analysis would be able to account for the intensity of the compliance to the activation measures in the measurement of their effect. Another line of research could provide insights on whether the insertion measures affect the outcomes of non-participants via spillover effects. Finally, a key question that this paper leaves unsettled is whether the GMI programme itself delays entry into employment. The existence of a household-size-specific income threshold may present an opportunity to identify the effect of GMI receipt due to a regression discontinuity.

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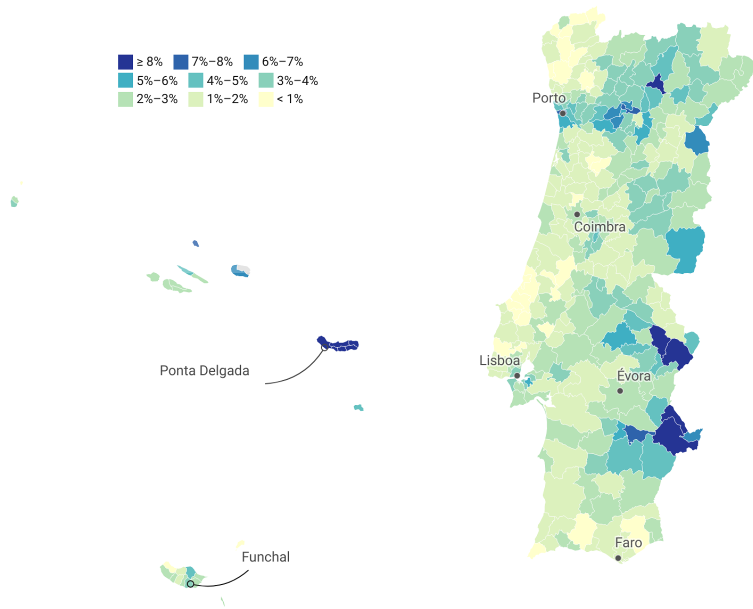
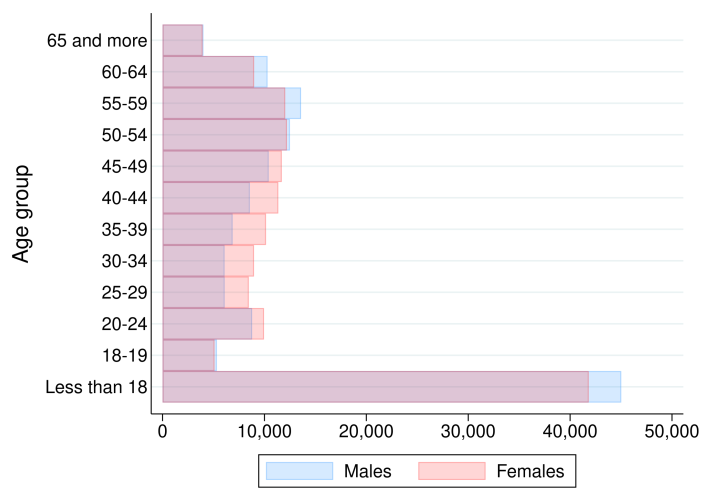
A Characterization of GMI recipients

Appendix Figure A1. Density of GMI recipients across municipalities, 2018 (SSI).



This figure shows a relative concentration of GMI recipients in the Azores archipelago and the North and inland Alentejo regions (the latter located in Southern Portugal).

Appendix Figure A2. Distribution of GMI recipients across age groups and gender, 2018 (SSI).



This bar plot shows that women and men access the GMI programme at similar rates and that a significant portion of GMI recipients are underage.

Appendix Table A1. Socio-economic characteristics of GMI recipients in the whole sample and selected treated subsamples.

Characteristics		Percent			
		Sample	Medical	Job	Training
Gender	Female	71.3	68.2	72.2	70.7
	Male	28.7	31.8	27.8	29.3
Age	<30	6.6	4.2	7.1	11.9
	30-39	23.3	15.1	25.7	24.6
	40-49	31.7	30.3	33.0	31.6
	50+	38.4	50.4	34.2	31.9
Educational attainment	Does not read or write	9.6	8.6	9.5	7.5
	No qualifications	4.2	4.5	4.0	3.9
	Primary school	31.9	35.3	30.1	33.2
	Middle school	44.5	43.2	46.0	49.9
	Secondary school	7.9	7.1	8.4	7.0
	University	1.9	1.3	1.9	1.4
Mean household benefit	<€200	44.2	55.9	41.4	41.3
	€200-€400	34.2	29.9	35.3	34.8
	€400-€600	16.4	11.5	17.6	18.6
	€600+	5.2	2.8	5.7	5.3
GMI spell	<2	31.3	30.7	32.2	32.7
	2-4 years	23.6	23.5	23.7	22.8
	4-6 years	14.9	15.5	15.1	14.7
	6+ years	30.2	30.3	29.0	29.8
Household size	1	44.2	42.5	26.2	28.7
	2-3	34.2	32.2	35.1	34.7
	4-5	16.4	19.0	27.9	27.1
	6+	5.2	6.3	10.8	9.5
Number of children	0	40.7	54.9	36.3	39.5
	1	20.8	19.0	21.4	20.3
	2-3	29.7	20.6	32.6	30.6
	4+	8.8	5.5	9.7	9.6
Other	Activated households	98.9	NA	NA	NA
	Foreign nationals	1.4	1.3	1.6	0.9
	Parental leave	10.0	8.7	10.6	9.8
	Disabled persons	2.3	4.7	1.0	0.8
	Social problems	1.2	2.0	1.0	1.3
	Single parent	21.2	16.5	23.0	22.4
N		(8,532)	(3,480)	(6,416)	(1,757)

B Probit estimates

Appendix Table B1. Probit model on probability of unemployment exit.

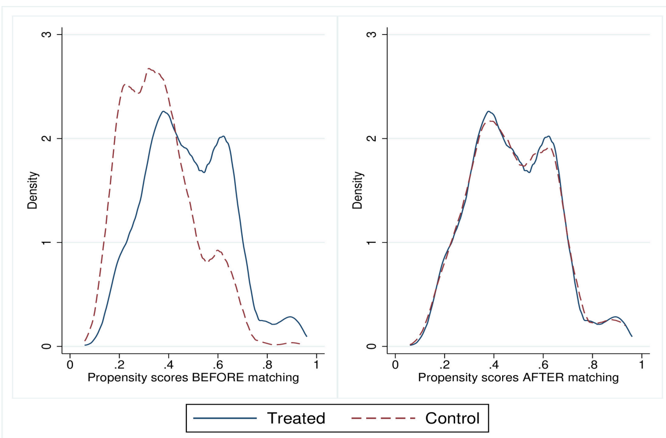
		$\hat{\beta}$	SE
Insertion measures	Medical assistance	-0.2723***	0.03069
	Training programmes	-0.2086***	0.04107
	Job assistance	0.1448***	0.04203
	Psicosocial support	-0.0676*	0.03751
Dummies	Female	0.0023	0.04041
	Foreign nationals	0.1642	0.12208
	Parental leave	-0.7068***	0.07834
	Disabled person	-0.5919***	0.13615
	Social problems	-0.2443	0.16418
Age	<30	-0.4062***	0.08189
	30-39	-0.1411***	0.05007
	40-49	0.0533	0.04039
Educational attainment	No qualifications	-0.8492***	0.08117
	Primary school	-0.2179***	0.03811
	Middle school	-0.5388***	0.09742
	Secondary school	0.1851***	0.05592
	University	0.4928***	0.10232
Mean household benefit	<€200	0.8950***	0.12203
	€200-€400	0.5949***	0.11718
	€400-€600	0.1209	0.11662
GMI spell	<2	0.0777*	0.04330
	2-4 years	0.0466	0.04572
	4-6 years	0.0749	0.05224
Household size	1	-0.1722	0.13603
	2-3	-0.1085	0.11973
	4-5	-0.1165	0.09878
Number of children	1	0.2499***	0.06673
	2-3	0.3070***	0.08444
	4+	0.3236**	0.13970
N		(8,532)	

Robust standard errors. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Baseline: men, native, no parental leave or social problems, over 50 years old, illiterate, household size over 6, GMI benefit over €600, childless, welfare spell over 6 years.

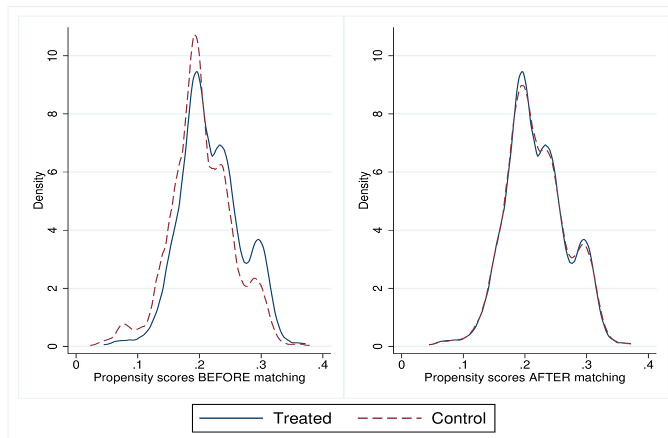
C Matching analysis

Appendix Figure C1. Density plots of propensity scores.

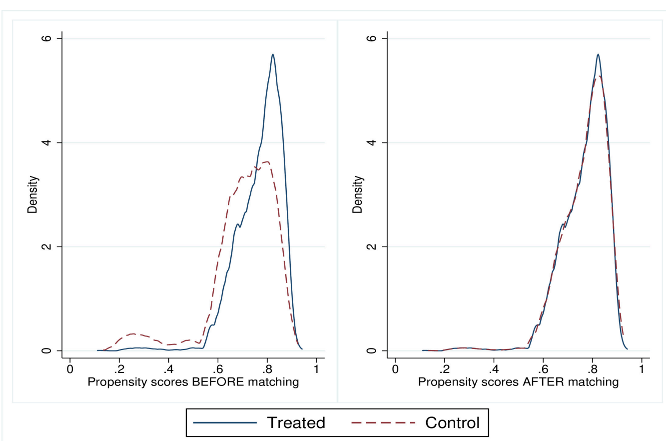
(a) Medical assistance



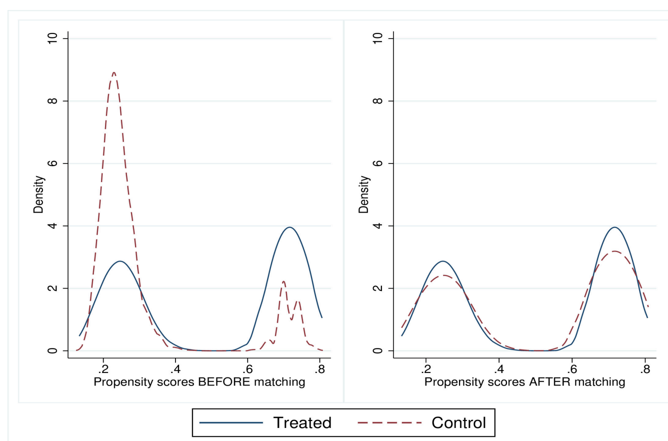
(b) Training programmes



(c) Job assistance

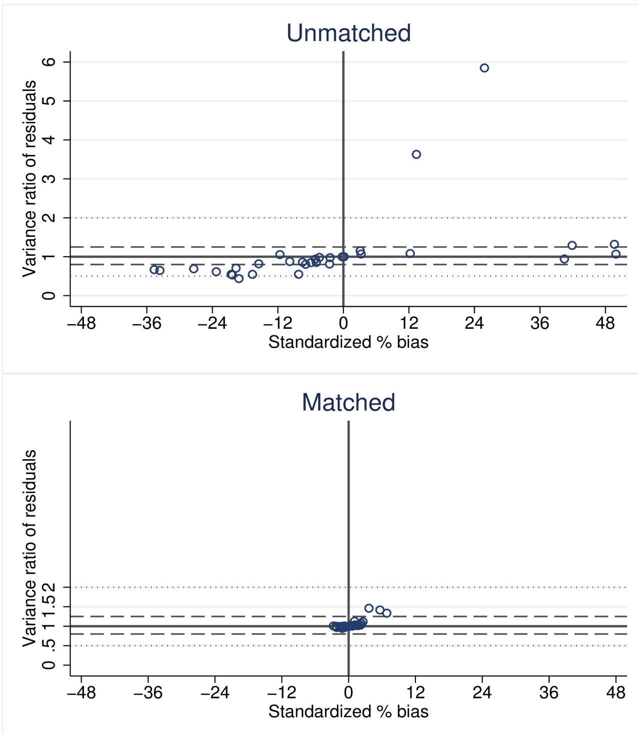


(d) Psychosocial support

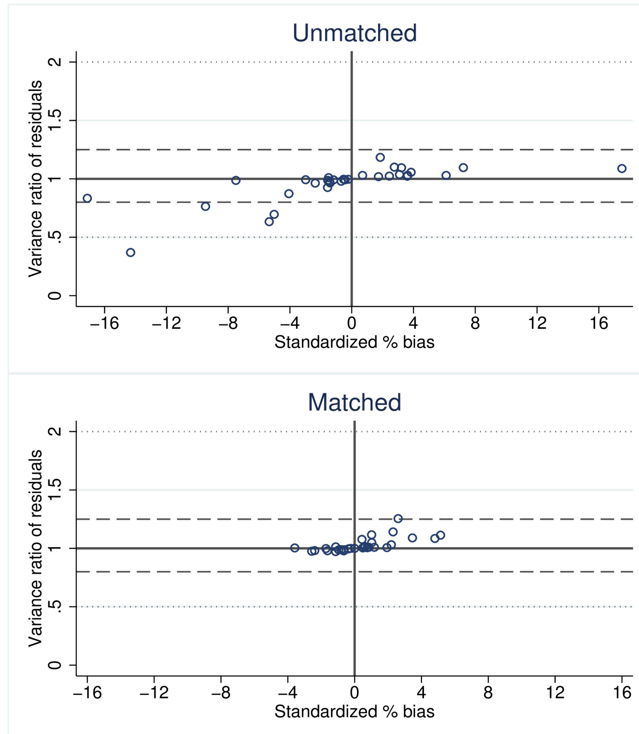


Appendix Figure C2. Visual inspection of standardized differences and ratio of residual variances before and after matching.

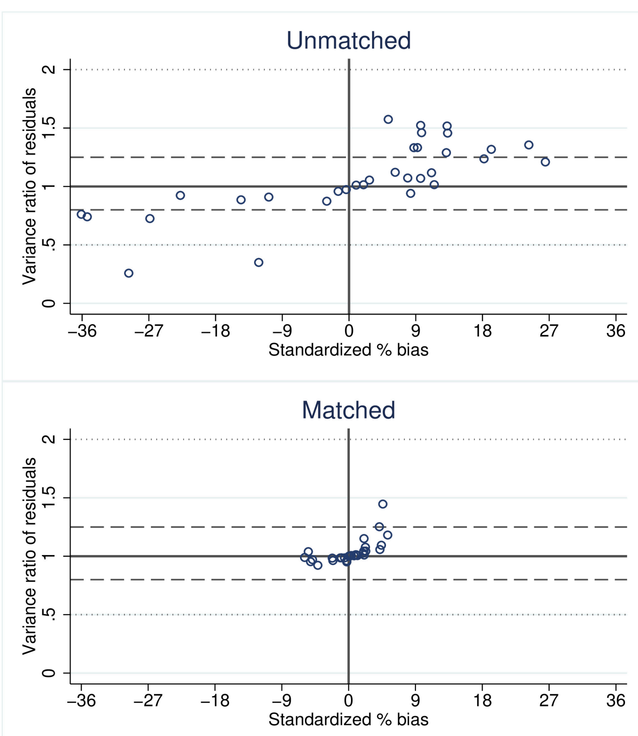
(a) Medical assistance



(b) Training programmes



(c) Job assistance



(d) Psychosocial support