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Employment effects of Reddito di cittadinanza, before and during the Covid-19 pandemic

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Abstract

Nel 2019 è stato introdotto in Italia per la prima volta un reddito minimo garantito, il cosiddetto Reddito di cittadinanza, un sostegno monetario alle famiglie povere accompagnato da percorsi di inclusione lavorativa e sociale per i beneficiari. Questo lavoro descrive una valutazione degli effetti occupazionali del Reddito di cittadinanza sui beneficiari inviati ai Centri per l'Impiego (Cpi) della Toscana per la stipula del Patto per il Lavoro. La valutazione è condotta impiegando una *differenza nelle differenze* con più tempi e coorti di ingresso al trattamento e facendo ricorso a fonti di dati amministrative. I beneficiari sono confrontati, mensilmente, con i non beneficiari, prima e dopo la politica, in termini di giornate lavorate. I risultati mostrano che, nei primi mesi dopo l'introduzione, il Reddito di cittadinanza provoca una riduzione delle giornate lavorative ma, successivamente, dopo un iniziale rodaggio dei Cpi e l'assunzione dei *navigator*, l'effetto occupazionale diventa positivo, con solo un leggero rallentamento durante l'emergenza dovuta al Covid-19. L'aumento delle giornate lavorative, maggiore per i giovani italiani con una precedente esperienza lavorativa nel commercio al dettaglio e nel turismo, è, però, molto contenuto (+3% in media) e, quindi, non in grado di far uscire i destinatari dalla condizione di povertà.

1. INTRODUCTION

Since 1992 European institutions called for the introduction of a guaranteed minimum income (GMI) in all Member States. In many countries a GMI was already in force, in others was quickly introduced. In Italy, after some experimentations, the so called *Reddito di cittadinanza* (*RDC*) was introduced only in 2019.

The implementation of GMI schemes across European countries, albeit heterogeneous, has some similarities. GMI is, typically, a non contributory income benefit subjected to means-testing and provided at certain citizenship/residence conditions. Income benefits are, usually, determined to top-up household disposable income to certain poverty thresholds. Last but not least, GMI is conditioned on various forms of labour market participation commitments (labour conditionality).

GMI differs with respect to universal basic income (UBI) for which the only pre-requirement is citizenship. Unlike contributory unemployment benefits (UB I), it is financed by fiscal general revenue and it is not intended to unemployed but to poor families, whose members may be employed or not. It has many similarities with non-contributory unemployment benefits (UB II), usually directed to unemployed, and subjected to family income means-testing[1]. Like contributory and non-contributory unemployment benefits, GMI is conditioned on labour market participation commitments.

The Italian GMI, introduced with the Law Decree n°4/2019, has been presented as an instrument to tackle poverty and, at the same time, as an active labour policy. In addition to the income support, the

[1] In Germany and UK the policy more similar to GMI is a non-contributory unemployment benefit (respectively *Arbeitlosengeld II* and Jobseeker's Allowance).

Italian government financed an extraordinary plan for the enhancement of public employment services (PES) and the hiring of a specific professional figure, called "navigator", specialised in taking in charge of *RDC* recipients, with the aim of avoiding any labour supply disincentive and of supporting them in finding a job.

Concerning GMI schemes and passive labour policies more in general, the most debated issue is, indeed, the possible disincentive effect on labour supply. Economic theory does not give a clear answer to this issue. The neoclassical model of labour supply, according to which individuals choose the combination of hours of work and leisure that maximizes their utility, under a time and a budget constraints, provides quite clear results, at least in its basic version. GMI increases the reservation wage, the level of wage beyond which individuals choose to work and below which they do not participate in the labour market, and, in this way, decreases working hours in favour of leisure (substitution effect).

However, other theories, such as the job search theory (e.g. see (McCall, 1970) (Mortensen, 1970)), developed to overcome the weaknesses of neoclassical labour supply models, firstly the fact that they do not contemplate unemployment, made the issue more complex. In these models, unemployed choose their reservation wage and their job search effort at that level and intensity that makes their utility of accepting a job offer greater than their utility of continuing the search and remaining unemployed. Reservation wages and search efforts depend on many factors such as the level of income benefits, the rate of arrival of jobs offers, the labour market demand and the costs of searching, so that the effect of GMI on labour supply may be ambiguous. E.g., high levels of income supports could make jobs with low wages less attractive but may positively affect the unemployed's intensity of search and his/her probability of getting a desirable job offer[2]. In addition, job search assistance and sanctions, on which often the income support is conditioned, could positively affect GMI recipients search efforts.

The empirical literature on the causal effect of GMI on labour market outcomes is still scant and not conclusive, both considering the impact of income support and that of active labour market policies assigned to recipients. Some studies show zero or little disincentive effects on labour supply while others find a decrease in employment rates for specific group of recipients, such as women living alone. Relating to specific labour market commitments, they find a positive impact of guidance and, mostly, training while a negative effect of start-up subsidies.

This paper aims at evaluating, with a counterfactual impact evaluation, the employment effects of *Reddito di cittadinanza* on recipients taken in charge by public employment services in the Region of Tuscany (Italy) in 2019. To the best of our knowledge, this is the first study that assesses the causal effects of *RDC* and of any other past Italian guaranteed minimum income experimentations on labour market outcomes. Further, our paper contributes to the scant literature on counterfactual impact evaluations of the employment effects of GMI schemes across European Countries. Our evaluation puts particular attention to the heterogeneous effects of the Italian GMI by type of recipients, identified by gender, age and citizenship.

As evaluation approach, by following (Callaway & Sant'Anna, 2020), we apply a difference in difference with multiple time periods and staggered treatment timings. We consider as treated recipients from March 2019, the month of beginning of the new policy, until December 2019, while as controls non-recipients of *RDC* in the same period, both registered as unemployed at Tuscan PES at February 2019. We measure employment effects with monthly working days from March 2019 to December 2020.

Results show that, on average, *RDC* did not have any disincentive on labour supply. Labour activation policies somewhat very little increased working days, especially for young men previously employed in retail and tourism, with a slowdown during the lockdown against the spread of COVID-19 pandemic.

The remainder of the paper is organized as follows. Section 2 examines the existing empirical literature on employment effects of GMI schemes across European Countries. Section 3 briefly describes the *RDC* legal framework, putting particular attention on labour market participation commitments. Section 4 describes our evaluation approach, while section 5 illustrates our data, empirical strategy and results. Section 6 concludes.

[2] Generous income support is one of the pillars behind the so called flexicurity model, together with flexible labour markets and adequate active labour market policies. Indeed, a generous income support could increase the quality of job matches and, consequently, wages.

2. EMPIRICAL LITERATURE REVIEW

The empirical literature on causal effect of European GMI on employment, through is function of income support and/or via labour activation commitments assigned to recipients, is scant.

(Piketty, 1998) evaluates the effects of the French *Revenu minimum d'insertion (RMI)*[3] following a difference in difference approach. He compares employment rates of recipients, distinguished by sex and family conditions (living alone, in couples where the spouse is inactive/unemployed, in couples where the spouse is unemployed), before and after the introduction of the measure and finds a decrease for women living alone.

(Bargain & Doorley, 2011) evaluate the French GMI with a regression discontinuity approach by exploiting a feature of the scheme, namely that fact childless adults under age 25 are not eligible. Using the 1999 census data on single men without children, they estimate a regression model where the dependent variable is labour supply (measured with employment rates or weekly hours) and the treatment variable is 1 if the individual is aged 25 or above and zero otherwise. They find a significant labour disincentive effect, between 7.2% and 10.2%, on junior school drop-outs single man, while no significant effects for single men aged 20-35 with higher educational levels. For men in couples, the negative effect on labour is lower. (Bargain & Vicard, 2014) apply a similar approach to census data from 2004-2010 to evaluate both *Revenu minimum d'insertion* and the new measure that replaced it, called *Revenu de solidarité active (RSA)*, finding little disincentive effects for both the schemes.

A different evaluation is made in (Danzin & Simonnet, 2014) that aims at comparing the financial incentives to return to work, via the partial consideration of labour income in the means-test, provided for *RSA* and *RMI* in France. They apply a difference and difference, where the control group is composed by recipients who retained the same financial incentive to re-employment after the implementation of *RSA*, while the treatment group is made up of recipients for whom incentives increased. They find an increase in return to work for women with one/two children while they do not find effect for men.

(De La Rica & Gorjón, 2019) assesses GMI's effects for Basque Country in Spain[4]. The analysis is carried out on monthly job finding rates of the universe of unemployed registered at PES from February 2015 to January 2016. GMI's impact is evaluated, both as passive and as active policy, by applying inverse probability weighting and propensity score matching. In evaluating GMI's function of income support, they consider unemployed recipients in the current month as treated and unemployed who do not receive any benefit in the same month as control group. They find that, in general, the impact of GMI on monthly job finding is not significantly different from zero. For women, young (< 30) and less educated workers GMI delays job finding. Further, it has no effect on men and accelerates job finding for older workers and for unemployed with more than primary education. In evaluating GMI's active labour policies, they consider as treated recipients who receive activation measures-guidance and training-at least once in the last 6 months and as control group recipients who have not participated in any activation measure. They find a positive impact of guidance and a stronger effect of training.

(Rønsen & Torbjørn, 2009) evaluate an activation program in Norway targeted to four group of social assistance recipients-long-term recipients, single parents, youth and immigrants. They compare survival rates, used to estimate transitions to employment since entry into the programme, of the four target group and that of the other recipients, the control group, previously matched with a propensity score matching. They do not find significant difference between controls and treated, considered as a whole, while they find positive effect on transition to employment for long-term social security recipients and small negative effect for young recipients.

(Wolff & Nivorozhkin, 2012) evaluate, with a propensity score matching, the effect of a specific labour active policy, a start-up subsidy, assigned to German non-contributory unemployment benefits recipients unemployed in 2005. As control group they consider a random sample of unemployed

[3] In France, the *Revenu Minimum d'Insertion (RMI)* was introduced in 1989. From the very beginning the eventual labour disincentive effect was under question. In 1997, in order to incentivize labour participation, the government introduced a deduction of labour income from the means-test used for *RMI* that was extended for the new measure that replaced it in 2001, the *Revenu de solidarité active (RSA)*.

[4] In Spain, a national minimum income scheme, the *Ingreso Mínimo Vital* has been introduced only in 2020, but the country has a long experience of regional measures. The first region that introduced a GMI was País Vasco in 1989, followed by the other regions over the 1990s.

recipients in the same period who did not enter the self employment programme. They estimate average treatment effects for different group of participants -men and women, East and West Germany- and find that the treatment increases the share of recipients who are no longer registered as unemployed nor as job-seekers and that East Germans and women tend to benefit less from the policy. (Bernhard & Kruppe, 2012), (Wolff & Jozwiak, 2007), (Hohmeyer & Wolff, 2007) are similar evaluation made for German UB II recipients and different active labour policies, specifically vocational training, short-term training, One-Euro-Job.

Finally, some studies use causal multivariate duration model à la (Abbring & Van den Berg, 2003). (Terracol, 2009) apply a mixed proportional hazard duration model to study the transitions from unemployment to employment of French GMI recipients compared to non-recipients. Their analysis is based on monthly data from seven French waves of the European Community Household Panel between 1994 and 2000. According to their findings, *RMI* has a negative impact on the hazard out of unemployment, but this effect is limited to the first 6 months of receipt, and drops afterwards. Further, the negative effect is weaker in households with children.

The same econometric model is applied in (Heinesen, Husted, & Rosholm, 2013) to estimate the effects of active labour-market policies on the exit rate to regular employment for non-western immigrants who receive social assistance in Denmark in the period 1998-2004. They estimate the effects of different active labour market policies (ALMP)) –such as "employment with wage subsidy" and "direct employment programme"-on the hazard rate to employment, distinguishing by in-programme and post-programme effects and by the time when the program begins. They find both post-programme and in-programme positive effects, higher with "employment with wage subsidy". Both in-programme and post-programme effects show a larger impact when the program begins at least six months after the start of the income support.

A more general review of the empirical literature related to GMI passes through the debate on welfare dependency, on employment effects of contributory unemployment benefits, on the impact of active labour market policies and on universal basic income.

Concerning welfare dependency, somehow "the opposite side of the coin" when considering the effects of GMI on labour maker outcomes, many works that study the dynamics of GMI recipients in European Countries can be found in empirical literature. E.g. (Königs, 2018) compare GMI recipients of four European countries-Luxembourg, Netherlands, Norway, Sweden- from 2001 to 2008 in terms of benefit spell length, frequency of repeated spells, time until re-entry into benefits and total duration of benefit receipt. (Cappellari & Jenkins, 2014) model social assistance[5] entry and exit probabilities among working-age adults in Britain between 1991 and 2005. (Hohmeyer & Lietzmann, 2020) study the determinants of UB II duration in Germany between 2005 and 2014. (Ayala & Rodríguez, 2010) analyse the extent of welfare recidivism (re-entry) and the determinants of the duration of off-welfare spells for GMI recipients in the Madrid Government. (Hansen, 2009) investigates the duration of social assistance and the likelihood of re-entry in Norway from 1992 to 2002. (Bergmark & Bäckman, 2004) analyse patterns of exit and recidivism to long-term recipiency of social assistance recipients in Sweden from 1990 to 1999. Studies on welfare dependency, generally, show higher exit rates and lower recidivism for household with children and single parent while the opposite for immigrants. However, labour market and policy changes are often the major determinants.

Empirical literature on the impact of contributory unemployment benefits on exit to employment is wide and some systematic review on US, Canada and European countries can be found. These studies start from the observation of a "spike" in unemployment exit rates around the time of unemployment benefit exhaustion. (Filges, Geerdsen, Due Knudsen, Klint Jørgensen, & Kowalski, 2015) review studies that consider as primary outcome variable the exit rate out of unemployment and into employment prior to benefit exhaustion or shortly thereafter. Their review reveals a significant exhaustion effect in the month/week of benefit exhaustion, one month before exhaustion and two months before exhaustion, while no effects emerge more than two month before exhaustion or one month after benefits have expired. The review of (Card, Chetty, & Weber, 2007) show that the "spike" at the exhaustion of benefits or prior is large when spells are measured by duration of registered unemployment and on studies that focus on the duration of benefit receipt. In contrast, most studies that have focused on time to re-employment show relatively small changes in exit rates at or near benefit exhaustion.

In order to prevent the distorting effects of passive policies and welfare dependency generally GMI as well as contributory unemployment benefits are conditioned on active labour market policies. The

[5] They consider income support and Job Seekers Allowance and Unemployment Benefit (UB) for unemployed jobseekers.

empirical literature on the effects of ALPM on labour market outcome is broad and synthesized in the meta-analysis of (Kluve, 2010), (Card, Kluve, & Weber, 2018), (Vooren, Haelermans, Groot, & Maassen van den Brink, 2019) and in the review of (Filges, Smedslund, Due Knudsen, & Klint Jørgensen, 2015). Results of the meta-analysis are mixed even if few common finding emerge. Subsidized labour and public employment programs tend to have negative short-term impacts or lower positive effects. Job-search assistance, treats/sanctions and training programs have generally positive effects.
One last line of research related to employment effects of GMI concerns studies on the same effects measured after the implementation of a universal basic income. Indeed, in countries where active labour market participation commitments associated to GMI are very weak or almost non-existent, the effects on labour market outcomes could be similar to the ones observed for UBI recipients. A recent work reviews studies on the effect of UBI on employment (de Paz-Báñez, Asensio-Coto, Sánchez-López, & Aceytuno, 2020). As stated by the authors, it is difficult to evaluate empirically UBI due to the fact that a complete or long enough experience has never been implemented. Therefore, they consider some partial experiences implemented in Alaska, Iran and in the Cherokee tribe territory, some field experiments in developed countries and more in developing ones. They further review laboratory experiments and simulation methodologies. They do not find evidence that confirms that a UBI causes a reduction in the labour supply.

3. THE REDDITO DI CITTADINANZA'S LEGAL FRAMEWORK

The *Reddito di Cittadinanza* was introduced with the Law decree 4/2019, subsequently converted into the law 26/2019. Applicants were able to apply starting from March 2019.
The measure, despite the name, is not a universal basic income but a typical GMI. Households in which the householder owns the Italian or of a EU country citizenship, living in Italy for at least 10 years, with disposable income lower than a poverty line and real estate and movable assets under certain thresholds may apply and access the measure. The income benefit is paid monthly to a maximum of 18 months with the possibility of a renewal in the case of maintenance of requirements. The amount[6] is determined to top-up household disposable income to the poverty line, differentiated on the basis of the family size. In household disposable income all income sources are considered, included the entire labour income of each family member.
The income support is conditioned on specific paths of work and social inclusion. More specifically, each recipient -not employed, not in education and professional training, not retired, under 65 years of age and without disabilities- is obliged to register as unemployed at PES[7]. Among recipients obliged to the registration, a sub set, those closer to the labour market[8], must be summoned by PES within 30 days from the recognition of the income benefit for a first interview and for the sign of the so called "Agreement for work". Family members obliged to register at PES not closer to the labour market but living with members in this condition must be summoned by PES in the same times.
Households without recipients obliged to register at PES or where every member is higher than 67 years of age receive only the income support and are not subjected to any activation measure. Households where recipients close to the labour market are not in must, instead, be summoned by municipal social services for an evaluation of all their needs, in addition to work ones, in terms of health, education, housing, family and proximity networks, and for the sign of the so called "Agreement for social inclusion". Even if belonging to households taken in charge by municipal social services, members under the age of 29 must be summoned by PES within 30 days from the recognition of the income benefit.
Recipients closer to the labour market, at their first meeting with PES workers, are subjected to a statistical and qualitative profiling to assess their personal and career characteristics and to measure their employability level to better identify the most suitable labour activation programmes. The "Agreement for work" that must be signed by recipients includes the following obligations: i) registration

[6] The amount is increased for families living in a rented accommodation.
[7] Family caregivers of children under 3 years or of severely disabled members may be exempted from the registration.
[8] The law consider closer to the labour market recipients not-employed from less than two years, who benefited of contributory unemployment benefits for no more than two years, already involved in labour activation measures according to their unemployment status (see Legislative Decree 150/2015), not involved in social activation measures (see Legislative Decree 147/2017).

in a specific digital platform, ii) actively seeking for a work, iii) participation into training or retraining courses or projects for self-entrepreneurship eventually proposed, vi) participation to psycho-aptitude interviews and any selection tests eventually proposed, v) acceptation of at least one of three "suitable job offers"[9].

Recipients who do not fulfil their obligations may occur in sanctions that consist in a monthly cut of the income benefit at the first infringement to a complete revocation in case of repeated infringements. Sanctions are applied in case of non-presentation at the convocation at PES for the first interview, in case of non-participation in training courses or selection tests and in case of non-acceptance of at least one of three suitable job offers.

For a sub set[10] of *RDC* recipients the Law decree 4/2019 introduced a specific active labour policy called "recollocation allow" that could be spent at PES or at private labour agency to get a professional and intensive job search support service. Further, a tax credit for firms that hire *RDC* recipients and for training agencies which guarantee training or professional course aimed at hiring them has been financed[11].

In order to make PES more effective and ready to apply their new functions following the introduction of *RDC*, the government financed an extraordinary plan to strengthen personnel and infrastructural equipment. Further, the government financed the hiring of a specific professional figure called "navigator" to support current PES workers in defining the contents of the "Agreement for work", in connecting with private labour agencies, in finding the vacancies expressed by firms and the opportunities from the education and training system. For bureaucratic reasons, "navigators" actually started working no earlier than October 2019.

4. EVALUATION APPROACH

To evaluate the causal effects of *RDC* on labour market outcome we rely on a difference in difference (DiD) approach. The traditional version of DiD considers two time periods and two groups. In the first period, no one is treated. In the second, a group is treated and the other is not (control group). Assuming that, in absence of the treatment, the outcome of the treated and the one of the control group would have followed the same path over time (parallel trends assumption), the average treatment effect (ATT) can be estimated by comparing the average variation in the outcome, from the first to the second period, observed by the treated group to the one experienced by the control group.

Difference in difference (DiD) is a suitable approach to evaluate employment effects of *RDC*, given that recipients are in severe economic condition and fragile and vulnerable working conditions and, therefore, not really comparable with other subjects if not with themselves over time. However, our, as several other empirical applications, do not well fit with the traditional version of DiD with two time periods and two groups, given that the context of implementation of the policy is very complex.

RDC was introduced in March 2019 for the first time and, as other new policies, required a long implementation time. At the beginning PES, despite the government had heavily funded them investing in personnel, skills and resources, presumably needed time to adapt and to manage a large amount of additional users and with characteristics different from traditional ones. In addition, "navigators" actually started doing their job only from the beginning of autumn 2019. Moreover, the law that introduced *RDC* gave to PES at least one month to call recipients for the sign of the so called "Agreement for work". To conclude, because of the lockdown against Covid-19 pandemic, from March 2020 to April 2020, PES workers suspended the application of labour conditionality for *RDC* recipients. In this very complex context, potential recipients could have applied to the new policy immediately in March 2019 or in the following months, being faced with a PES system in very different conditions.

For all these reasons, we apply, instead of a simpler two groups-two periods approach, a DiD with multiple time periods and staggered treatment timings by following (Callaway & Sant'Anna, 2020). In their framework, there are $i = 1, \dots, n$ units and $t = 1, \dots, \tau$ time periods. G_g is a binary variable equal to 1 if a unit is first treated in time period g (treated) while C equal to 1 for units that never entering in the

[9] The adequacy of a job offer is determined on the basis of the distance from the workplace to the recipient's residence, taking into account the consistency with previous professional experiences and the adequacy of remuneration.

[10] The measure was not general for financial reasons.

[11] However, the number of tax credits actually used by firms is very limited.

treatment (control group). By assumption, once a unit becomes treated that unit will remain treated in the following periods (*irreversibility of treatment*). Causal effects are indentified and estimated by using a generalisation of the average treatment effect on the treated of the canonical DiD called *group-time average tratment effect* and denoted by $ATT(g,t) = E\left[Y_{it}(g) - Y_{it}(0)|G_g = 1\right]$. $Y_{it}(0)$ is unit *i*'s untreated potential outcome at time t if that unit remain untreated, while $Y_{it}(g)$ is unit *i*'s potential outcome at time *t* if the unit first becomes treated in time period *g*.
To identify the $ATT(g,t)$ (Callaway & Sant'Anna, 2020) impose two assumptions. The first is the *limit treatment anticipation*, that restricts the anticipation of the treatment, denoted by δ, for all "eventually treated" groups. The second, is the *conditional parallel trends assumption*, that can be based on "never-treated" or "not-yet treated"; in the first case, the one that we consider for our evaluation, it implies that the average outcomes for the group first treated in period g and for the "never-treated" group would have followed parallel paths in the absence of treatment, conditioning on covariates X [1].

[1] $$E\left[Y_{it}(0) - Y_{it-1}(0)|X, G_g = 1\right] = E[Y_{it}(0) - Y_{it-1}(0)|X, C = 1]$$

Under these assumptions, $ATT(g,t)$ are non-parametrically point-identified and can be recovered with three different approaches, identical from an identification standpoint but not for the estimation. The "outcome regression" approach, that models the conditional expectation of the outcome evolution for the comparison groups, the "inverse probability weighting" approach that models the conditional probability of being in group g, and the "doubly robust" approach that exploits both OR and IPW components[12].
The appropriate reference time period is $t = g - \delta - 1$, that is the most recent time period when untreated potential outcomes are observed for units in group *g*. When pre-treatment covariates do not have a role in identification, the average $ATT(g,t)$ is identified by subtracting from the change in outcomes actually experienced by the group *g*-between the most recent period before they were treated and the current period- the change in outcomes experienced by the comparison group (see [2]).

[2] $$ATT(g,t) = E\left[Y_{it}(g) - Y_{ig-\delta-1}(0)|G_g = 1\right] - E\left[Y_{it}(g) - Y_{ig-\delta-1}(0)|C = 1\right]$$

As (Callaway & Sant'Anna, 2020) underline, the $ATT(g,t)$ can be the ultimate causal parameters or can be averaged to evaluate more aggregate effects. For example, they can be aggregated across calendar times *t*, across different groups *g* or by length of treatment exposure.

5. THE EMPLOYMENT EFFECTS OF *REDDITO DI CITTADINANZA*

5.1 Data and descriptive statistics

Our evaluation is based on data referred on one of the Italian regions, Tuscany, for which we have different sources of administrative data.
Data on Tuscan *RDC* recipients of 2019 are collected by the National Social Security Institute (INPS) and include information on the month of application and the duration of the benefit. The information on whether recipients must summoned by Tuscan PES derives from data collected by the National Agency for Active Labour Policies (ANPAL). The Tuscan Labour Informative System (SIL), owned by the regional government of the Region of Tuscany, collects data on individuals (recipients of *RDC* and not) who register as unemployed at Tuscan PES and on the daily flow of hirings and firings of workers by Tuscany companies and entities, with detailed information on demographic characteristics (gender, age, education level, nationality) as well as labour market information (previous employment experience, sector of activity).
According to the database obtained by matching the different administrative data sources, Tuscan recipients of *RDC* are about 91thousand in 2019. We select those who must summoned by Tuscan PES and registered as unemployed before the introduction of the measure (at the end of February 2019)

[12] The DR approach shows additional robustness against model misspecications when compared to the OR and IPW approaches.

and compare them to PES users registered as unemployed on the same date who are "never-treated" in our observation period (from the March 2019 to December 2020)[13].

As table 1 shows, recipients of *RDC* registered as unemployed at Tuscan PES at the end of February 2019, so before the introduction of the measure, are 16,220. Half of the recipients, 8,795 units, apply for *RDC* in March 2019, the first month of introduction of the new policy, 2,652 apply in April while 1,654 in May. The distribution by demographic characteristics of controls and treated clearly demonstrates how the latter, to a greater extent than the former, have those features associated with a lower probability of participating in the labour market and of finding a job. Indeed, among treated there are, on average, more foreigners and female and more people higher than 50 years old than among controls. Not surprisingly, in the two years prior to the introduction of *RDC*, the average number of days worked in a month by the future recipients was 10, while that of controls was 13 and the percentage of people who works was respectfully 52% and 61%.

Table 1.
DISTRIBUTION OF CONTROLS AND TREATED, BY MONTH OF APPLICATION AND DEMOGRAPHIC CHARACTERISTICS - ABSOLUTE AND RAW PERCENTAGE FREQUENCIES AT THE END OF FEBRUARY 2019

	Controls	Treated	Mar-19	Apr-19	Mag-19	Giu-19	Lug-19	Ago-19	Set-19	Ott-19	Nov-19
Overall	32,557	16,220	8,795	2,652	1,654	771	650	286	428	584	400
Italian	74%	65%	61%	61%	59%	62%	70%	71%	66%	69%	71%
Foreigner	26%	35%	39%	39%	41%	38%	30%	29%	34%	32%	29%
under 29	38%	25%	29%	31%	30%	30%	30%	29%	33%	26%	34%
30-39	21%	22%	22%	21%	21%	20%	19%	22%	21%	26%	21%
40-48	21%	27%	28%	27%	26%	28%	28%	26%	24%	25%	23%
50+	20%	26%	22%	21%	23%	22%	23%	23%	22%	24%	21%
Female	51%	52%	52%	51%	53%	54%	52%	54%	54%	59%	51%
Male	49%	48%	48%	49%	47%	46%	48%	46%	46%	41%	49%

Relevant differences can be seen also by comparing treated between them. More specifically, by analysing their work experience, prevailing sector of activity and education level for 16 different profiles (see Table 2) we identify some peculiar and distinctive types of *RDC* recipients, on which we expect different effects from the "Agreement for work" signed at PES.

A first type is composed of foreigner women who, especially if higher than 30 years of age, work, more than others treated, in cleaning services or as care-takers. Foreigner women higher than 50 years presumably work most of all as assistants to the elderly, perhaps taking advantage of a high qualification in health disciplines obtained in their country of origin. They are recipients who do have a job, that is, however, underpaid or completely/partially illegal. For this reasons, we do not expect a huge investment in their professional relocation when taken in charge by PES.

A different type is composed of young recipients, under 30 years of age whose professional experience have been concentrated in sectors exposed to seasonal and contractual precariousness, such as retail and tourism. Among them, men tend to have worked more than women, while foreigner men more than Italian men. Young foreigner women have a relevant work experience in cleaning services or as care-takers too. Their education tends to be higher than that of the older treated. Young recipients are probably easier to relocate but they risk of finding a job in sectors that do not allow them to get out of poverty.

A third type involves men, over 50, especially Italians, with a lower number of monthly working days and an lower probability, with respect to others treated, of having worked before *RDC.* They presumably are people now on the margins of the labour market, certainly more difficult to relocate and that probably require retraining and professional training courses.

The other recipients not included in these cases are women, between 30 and 50 years of age, especially Italian, who often never worked before *Rdc*, perhaps even due to personal or family reasons, and men, in the same age classes, who have frequently worked and, if foreigner, to a great extent, in the building sector. For the former we do not expect a significant activating ability of PES, for the latter their relevant work experience could increase their likelihood of finding a new job.

[13] We extracted a random sample of controls that is two times the number of treated.

Table 2.
WORK EXPERIENCE, PREVAILING SECTOR OF ACTIVITY AND EDUCATION LEVEL OF TREATED BY DEMOGRAPHIC CHARACTERISTICS AT THE END OF FEBRUARY 2019

	Average working days per month	Percentage who worked	Agriculture	Industry	Building	Retail + tourism	Services	Cleaning + caretaker	Percentage with compulsory school
Female under 29 0	8	44%	2%	7%	1%	40%	12%	38%	49%
Female under 29 1	9	50%	1%	7%	1%	60%	19%	12%	47%
Female 30-39 0	11	52%	4%	5%	0%	30%	7%	54%	54%
Female 30-39 1	9	52%	3%	7%	0%	37%	23%	30%	61%
Female 40-48 0	13	54%	2%	2%	0%	19%	6%	72%	60%
Female 40-49 1	9	48%	2%	5%	1%	39%	14%	39%	64%
Female 50+ 0	12	54%	2%	0%	1%	7%	2%	88%	48%
Female 50+ 1	8	40%	2%	5%	0%	30%	13%	49%	64%
Male under 29 0	14	67%	9%	18%	11%	32%	22%	9%	66%
Male under 29 1	10	56%	3%	19%	9%	47%	19%	3%	55%
Male 30-39 0	14	67%	11%	16%	23%	22%	15%	13%	82%
Male 30-39 1	11	61%	5%	20%	15%	27%	26%	7%	72%
Male 40-48 0	12	62%	15%	14%	27%	18%	13%	13%	85%
Male 40-49 1	8	51%	6%	16%	21%	26%	23%	7%	75%
Male 50+ 0	8	49%	21%	17%	17%	13%	13%	20%	79%
Male 50+ 1	6	36%	9%	15%	19%	23%	23%	11%	71%
Overall	10	52%	5%	11%	9%	33%	17%	24%	63%

5.2 Evaluation strategy and assessment of parallel trends

In our evaluation of the employment effects of *RDC* we consider as outcome variable the average monthly days worked by treated and controls, calculated by using data of the Tuscan Labour Informative System on daily flows of hirings and firings of workers by Tuscany companies and entities. We consider months from January 2017 to February 2019 as pre-treatment period, while post-treatments months go from March 2019 to December 2020. Given that *RDC* started in March 2019, we do not admit anticipation in the treatment (δ is equal to zero). Indeed, we assume that *RDC* recipients did not changed their behaviour on the labour market before the start of the policy, because the policy was completely new and promised by some political parties for many years before its actual introduction. Altogether, we have 48 monthly time points from 2017 to 2020, while the first entry in *RDC* happens at t=27, in March 2019.

Due to the strong heterogeneity in the characteristics of *RDC* recipients, shown in paragraph 6.1, for both identification and estimation reasons our evaluation is made on 16 different recipient profiles $p_{1,\dots,}p_{16}$, combination of sex, citizenship and four age classes (29-; 30-39; 40-49; 50+). From the point of view of identification, we believe that the change in outcomes after *RDC* is more likely to be the same looking between profiles of treated and controls rather than in general. Further, as explained in the previous paragraph, we expect heterogeneous effects of *RDC* on employment, precisely due to their different characteristics and given the peculiarities of the Italian labour market and the ability of PES in relocating users.

Accordingly to our empirical strategy, we reformulate the framework of (Callaway & Sant'Anna, 2020), as represented in [1] e [2], respectively in [3] and [4]. In each profile *p* there are 10 different entry timings: C equal to 1 (=no entry), g=27 (=entry at t=27, March 2019), …, g=35 (=entry in November 2019). We impose the parallel trend assumption for each profile *p* [3] and, under no anticipation, we estimate, with the "doubly robust" approach, a profile-group-time $ATT(p,g,t)$ for each entry group g belonging to each profile *p* and for each *t* >=27, by means of the estimator [4].

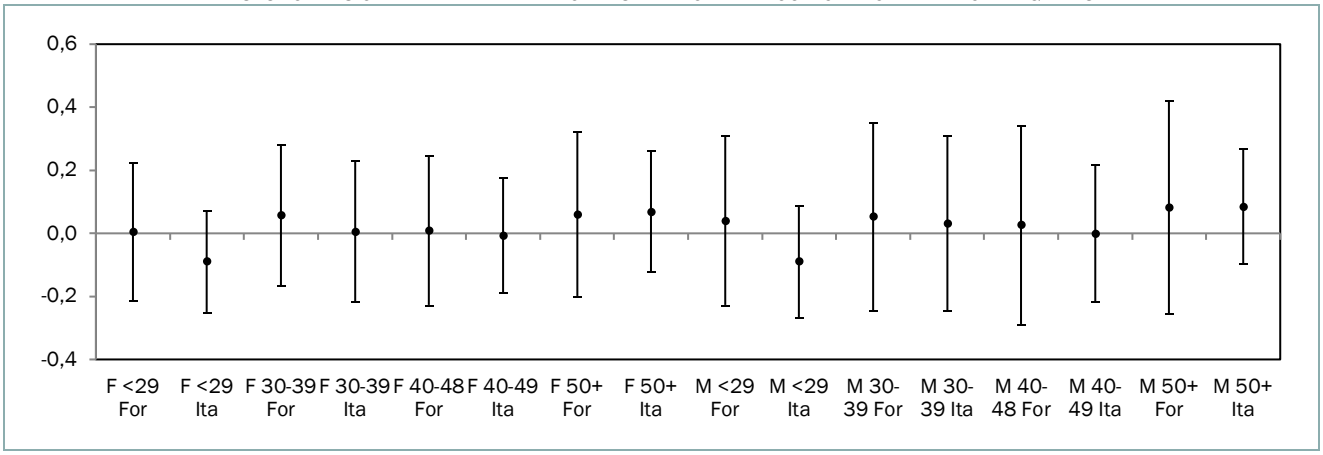
[3] $$E\left[Y_{it}(0) - Y_{it-1}(0) |\, G_g = 1, \mathrm{p}\right] = E[Y_{it}(0) - Y_{it-1}(0) | C = 1, \mathrm{p}]$$

[4] $$ATT(p,g,t) = E\left[Y_{it}(g) - Y_{ig-1}(0) | G_g = 1, \mathrm{p}\right] - E\left[Y_{it}(g) - Y_{ig-1}(0) | C = 1, \mathrm{p}\right]$$

Then, we average the profile-group-time average treatment effects $ATT(p,g,t)$'s in meaningful ways to obtain more aggregate causal quantities. To evaluate the heterogeneous effects by profile, we aggregate the estimated $ATT(p,g,t)$s by applying, first, a simple average across times (from March

2019 to December 2020) in each group and, then, a weighted average across groups (g=27,...g=35). To obtain *ATT*s by demographic characteristics and an overall result, we make respectively a weighted average by demographic characteristics and an overall weighted average of the $ATT(p)$s (see paragraph 6.3 for the results). We, finally, aggregate the $ATT(p,g,t)$s by calender month with the aim to better explain the different phases of the implementation of the new policy (see paragraph 6.4). The *ATT* by calendar month are obtained by first avareging the $ATT(p,g,t)$s across groups within profiles and then by avareging across profiles by calendar month. To simplify the discussion, from now on, we call the different averages of $ATT(p,g,t)$ simply as *ATT*.
Since all our estimates are based on the assumption of parallel trends, we assessed its plausibility by estimating the pseudo $ATT(p,g,t)s$ for the pre-treatment period and by averaging them, first, across pre-entry times and, then, across groups by profile. As figure 1 shows, each pseudo-ATT is significant different from zero demonstrating that, before *RDC,* the change in outcomes from one month to the following have been the same for treated and controls.

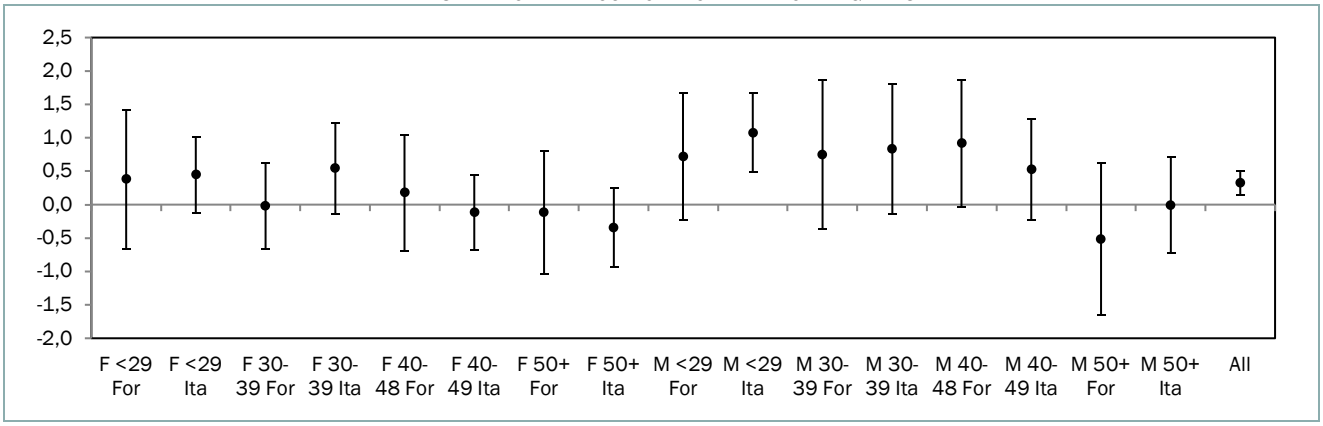
Figure 1.
PSEUDO *ATT*S ON PRE-TREATMENT MONTHS BY PROFILE *P* - 95% CI - MONTHLY WORKING DAYS



5.3 Heterogeneous effects by type of recipients

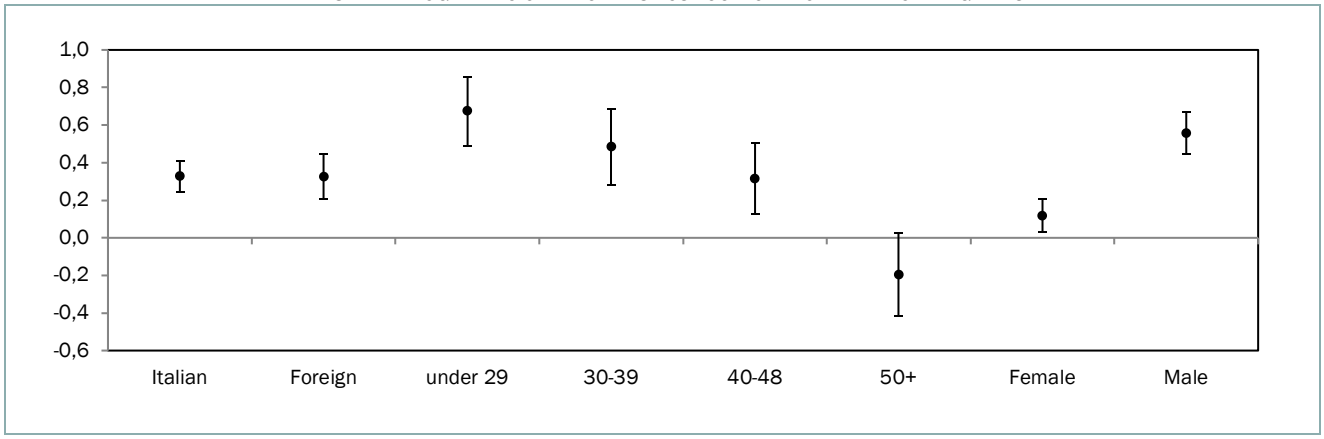
Considering the different profiles of recipients as a whole, we find a positive and significantly different from zero but very limited average effect of *RDC* on treated, equal to 0.3 monthly working days (see figure 2). This means, that *RDC* increases the monthly working days of treated by the 3%.
Looking at the different profile of recipients, as expected, *RDC* shows heterogeneous effects. The profile for which we find the higher effect (and significantly different from zero) is the one composed of Italian men under 29 years old for which *RDC* increases by 1.1 the number of monthly working days. The effect is positive and higher than the other treated for the rest of men, excluded those with more than 50 years of age, for whom, on the contrary, we found a negative effect. Among women, we find a slight positive effect for young women, Italian e foreigner, and for Italian women between 30 and 39 years old. For women higher than 50 years of age, the effect is even negative, although not significant different from zero.

Figure 2.
*ATT*S BY PROFILE *P* - 95% CI - MONTHLY WORKING DAYS



Further aggregating the *ATT*s by demographic characteristics, we find some clear evidence (see Figure 3). *RDC* has a positive and significant effect on monthly working days that decreases with age until becoming negative for recipients over 50 years of age. The *ATT* is 0.6 working days for recipients under 29 years, 0.5 between 30 and 39 years and 0.3 for recipients between 40 and 49. Further, the employment effect of *RDC* is equal to 0.6 working days for men and only 0.1 for women, while there is not a heterogeneous effect among Italian and foreigner recipients.

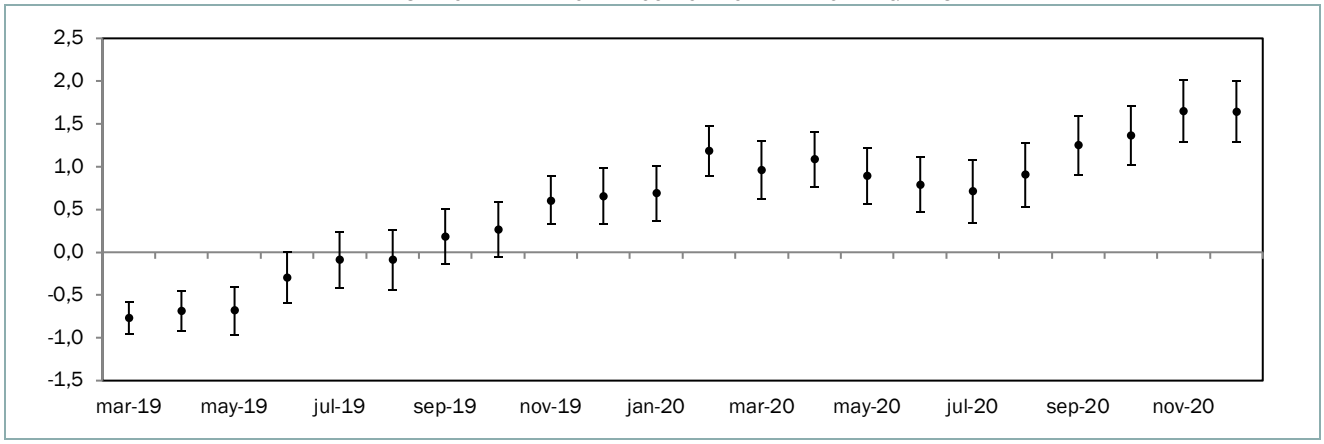
Figure 3.
*ATT*S BY DEMOGRAPHIC CHARACTERISTICS - 95% CI - MONTHLY WORKING DAYS



5.4 Effects by calendar month

The aggregation of $ATT(p, g, t)$ by calendar month shows further clear evidence. In the first three months of introduction, *RDC* decreases employment of treated of about 0.7 monthly working days (significant at 5% level). On first reading, this negative effect could be interpreted as disincentive of labour supply. However, this initial negative effect could also be explained by a physiological delay in the time of implementation of the new policy which directed to PES a large number of recipients all together. In addition, the law itself leaves up to a month of time for PES to taking in charge *RDC* beneficiaries. Finally, after the first months of implementations, the effects of *RDC* becomes zero and even positive, and significant different from zero, starting from November 2019 until December 2020. In our interpretation, this change could depend, on one side, on the increased time to adapt to the new policy available for PES workers, on the other side, to the positive contribution by "navigators" who, for bureaucratic reasons, actually started their work only from autumn 2019.

Figure 4.
*ATT*S BY CALENDAR MONTH - 95% CI- MONTHLY WORKING DAYS



Lastly, what can be noticed from the dynamics of ATTs by calendar month is an interruption of a positive trend starting from March 2020 until July 2020. This "parenthesis" is clearly linked to the lockdown decided by the Italian government in February 2019 to tackle the Covid-19 pandemic. Indeed, the Italian lockdown has forced all companies and public institutions of non-essential services to stop, included PES. Only, a few months after the onset of the pandemic, during summer 2020, PES reorganized and resumed their functions in smart working.

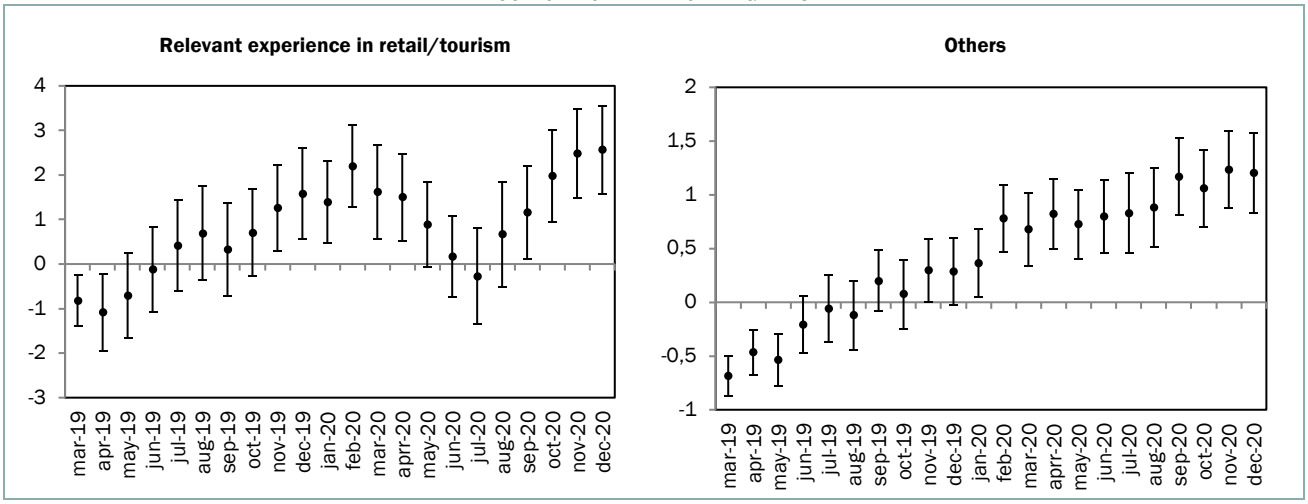
5.5 Effects during Covid-19 pandemic

Following the Covid-19 outbreak, waves of lockdowns and restrictions were enforced from March 2020 onwards in Italy. All economic sectors affected, though restrictions have been particularly sharp and impacting in non-essential services such as retail trade, expect for food goods, and tourism services (restaurants, hotels, cultural cervices).

In order to better understand the effects of *RDC* on employment during the pandemic, we reassess all recipients' CV to establish who had relevant, recent work experience in retail and tourism, which might have led to further employment opportunities in these sectors had the epidemics not broken out. More precisely, we consider recipients with relevant experience in retail/tourism those with more than 50% of worked time in these sectors from March 2019 to February 2020[14]. Since we use post-treatment information to reclassify units, we need to assume that *RDC* did not systematically promote career turnarounds from/to retail and tourism from March 2019 to February 2020. In other words, we assume that any turnaround from/to retail/tourism occurred under *RDC* would have occurred the same under the alternative treatment status (without *RDC*). In our opinion, this assumption is quite reasonable, since career developments from March 2019 to February 2020 could not be driven by the knowledge of future restrictions affecting retail and tourism.

Under this assumption, we estimate $ATT(p,g,t)s$ separately for recipients with relevant experience in retail/tourism and the others and average them by calendar months as shown in Figure 5.

Figure 5.
ATTS BY CALENDAR MONTH AND RELEVANT EXPERIENCE IN RETAIL/TOURISM
95% CI- MONTHLY WORKING DAYS



Results are very clear. For recipients with relevant experience in retail/tourism there is an evident fall in ATT's during the lockdown, from March 2020 to July 2020, while for the others only a very slight attenuation is observed, in the same period, of a trend that continues to be positive until December 2020.

6. CONCLUDING REMARKS

In this paper we described our evaluation of the employment effects of *RDC*, the Italian GMI, on recipients who live in the Region of Tuscany in 2019, sent to PES for labour market participation commitments and previously registered as unemployed. The policy is national and many aspects, such as the entity of income support, the timing of the taking in charge by PES or the types of labour market participation commitments, have been decided and set at central level. However, each Region has a peculiar PES organization and offer different labour active policies to unemployed, albeit with some similarities. Further, the labour market could be very different across territories, especially for the relative importance of tourism and services with respect to industry. For these reasons, we believe that

[14] Nearly 20% of GMI beneficiaries satisfy this condition.

our results can be generalized and extended to other Italian Regions or local contexts, with similar system of services for employment and labour markets.
With respect to the existing literature on the causal effects of GMI on employment we think that the main limitation of our study is the unavailability of information about the family conditions of recipients, like the presence of children or about being married or not. However, to the best of our knowledge, this is the first study that applies a counterfactual impact evaluation on the employment effect of the Italian GMI that allows assessing the policy for different types of recipients, by personal demographic characteristics.
Our findings suggest that *RDC*, considering the entire period of our observation, does not decrease labour market participation of recipients. The first few months after the introduction, the policy causes a reduction in working days, but subsequently, after a probable initial run-in of PES and the hiring of navigators, the employment effect becomes positive, with only a slight slowdown during the Covid-19 pandemic. The increase of the number of working days due to *RDC* is, however, very limited, about +3%, and, therefore presumably not able to make recipients exit from poverty and to exclude welfare dependency. Among recipients, those who benefit more from *RDC* are Italian young men with a previous work experience concentrated in retail and tourism.
In our opinion, these results suggest that the strengthening of PES and active labour policies implemented by the government when *RDC* was introduced is not sufficient and is still inadequate. Despite the extraordinary plan for the enhancement of public employment services, the staff should be further increased compared to the needs, while the "recollocation allow", currently intended only for a part of recipients, should be generalised and extended. Otherwise, *RDC* will be only an additional source of income on which recipients will become dependent.

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