

**Do Employers Discriminate Participants in Active Labour Market Policies?
A field experiment during the Covid-19 pandemic**

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Abstract

This work aims at uncovering whether employers discriminate candidates that participated in Active Labour Market Policies at the beginning of their career, examining the case of the Youth Guarantee subsidized internships in Italy. Through a field experiment 4,066 fictitious resumes were sent to employers in 11 Italian cities. The data collection was conducted throughout the first Covid-19 lockdown started in March 2020, providing a unique occasion to study employers' hiring behavior following a negative shock in the labor market. Each resume randomly displayed participation in the Youth Guarantee internship, a regular internship, or an unemployment gap after university graduation, followed by at least 32 months work experience. Employers discriminate former Youth Guarantee interns compared to those unemployed or regular interns. Discrimination is strongest when sectorial IT skills are not on the resume and disappears as more information is disclosed. Results suggest that employers perceive the program as detrimental for human capital accumulation and use it as a cue to statistically discriminate. After the lockdown, discrimination against former regular interns also emerges, revealing employers' ranking of candidates in the labour queue. This study provides novel evidence on the long-term scarring effect of ALMPs in the form of subsidized internships.

1 Introduction

A considerable amount of literature documents discriminatory practices in the labour market. Most studies concern ethnicity (Pager 2003; Bertrand and Mullainathan 2004; Booth, Leigh and Varganova 2012; Zschirnt and Ruedin 2016; Baert and Vujić 2016; Bertrand and Duflo 2017; Quillian et al. 2017; Neumark 2018; Busetta, Campolo and Panarello 2018; Quillian et al. 2019; Larsen and Di Stasio 2021) and gender (Riach and Rich 2006; Correll, Benard and Paik 2007; Maurer-Fazio and Lei 2015; Baert, De Pauw and Deschacht 2016; Quadlin 2018; Busetta, Fiorillo and Palomba 2021; Sarsons et al. 2021; Gërkhani et al. 2022). Much scholarly work has also been dedicated to detecting discrimination on the ground of age, disability, wealth, sexual orientation, religion, political beliefs, and many other individual characteristics (Gaddis 2018; Lippens, Baert and Vermeiren 2021). These studies provide evidence that the labour market is far from being a meritocratic place. Employers may be sensitive to subtle cues, which could trigger a preference for one candidate over another even though they share similar skills and experience.

One of the consequences of this unequal playing field is that those discriminated against experience longer spells of unemployment (Auer, Bonoli and Fossati 2015) and more difficulties in re-entering the labour market (Van Belle et al. 2018). In most developed countries an extensive welfare system has been created with the idea of ensuring that the playing field gets leveled, providing the same opportunities for everyone. Increased effort has gone into designing welfare policies that are meant to help people get back to work. An increasing amount of spending has been concentrated on Active Labor Market Policies (ALMPs), especially after the 2008 Great Recession (Martin 2015). However, little is known on how these policies impact recipients' future employability from the employers' perspective.

The present study aims to shed light on this aspect and attempts to answer the following research question: do employers discriminate against candidates that participated in Active Labour Market Policies (ALMPs) at the beginning of their career? Scholars only started to explore the effects of ALMPs on employers' perception and hiring practices in recent years (Liechti et al. 2017; Van Belle et al. 2018; Cahuc and Hervein 2020; Fossati, Liechti and Wilson 2021; Hervein and Villedieu 2022), reporting mixed findings. Moreover, whether ALMPs *leave a durable mark* on participants' employment history, even after they have

accumulated more work experience, remains an overlooked aspect. At the same time ALMPs are seen as crucial for the stable reintegration of job seekers into the labour market. The increased tendency to tie welfare and work requirements together made past welfare use more salient to employers, as it increasingly became part of recipients' employment history. At the same time, the increased salience of welfare use may become stigmatising and could prompt employers to discriminate against past ALMPs participants. As a result of this, ALMPs could potentially produce a backlash effect, thus hindering participants' future employability.

A large part of the literature on the impact of ALMPs uses surveys or job seekers administrative data (Card, Kluge and Weber 2010; Hohmeyer and Wolff 2012; Bredgaard 2015; Card, Kluge and Weber 2018; Benda, Koster, and van der Veen 2019; Anpal 2020; Pastore and Pompili 2020) and focuses on the supply side of work. However, these studies do not allow for the disentanglement of job seekers and employers' behaviour in determining employment outcomes. Supply side approaches highlight the role of job seekers' effort and characteristics in getting a job, while downplaying the importance of the employers' power in deciding who gets hired. Insisting on job seekers activation might be of little use if recruiters end up discriminating against past participants in ALMPs. This calls for more research which considers the demand side of labour, as ultimately employers remain the gatekeepers of employment opportunities (Bills, Di Stasio and Gërxhani 2017; Liechti et al. 2017).

The Active Labor Market Policy considered in this study is the Youth Guarantee in Italy. It is a subsidised internship programme targeted towards young people that are aged 15-29 and Not in Education, Employment or Training (NEET). This policy is of particular relevance in Italy. Italy has the highest percentage of NEETs in Europe, which is as high as 23.1% of the youth aged 15-29 in 2021 (Eurostat 2022a). However, despite the efforts made through this programme to better connect the youth to the labour market, its long-term impact on future employability remains poorly understood. A field experiment was used to test whether employers discriminate against candidates that participated in the Youth Guarantee internships. Fictitious resumes were randomised and fell into one of the following three conditions: participation in a Youth Guarantee internship, a regular (unsubsidised) internship, or an unemployment gap at the beginning of the employment history. Data collection took place in 11 cities that were distributed evenly across the Italian territory. Thus, it was possible to leverage wide geographical variation in labour market conditions in the country, which also increased the external validity of the study.

The contribution of this work to the literature on ALMPs is threefold. First, it provides evidence that employers discriminate against past programme participants compared to those in unemployment and those in a regular internship. Moreover, it uncovers the mechanism behind differences in call back rate. The experimental design includes four additional information treatments that are meant to signal candidates productivity. This made possible to not only detect discrimination, but also to distinguish whether such discrimination stemmed from a lack of information, thus supporting the Statistical Discrimination Theory, or from cognitive bias, thus supporting the Status Characteristic Theory of discrimination (Foley, Gërxhani and Van de Rijt 2022). The results show that discrimination is at its highest when little additional information about the candidates' productivity is provided. In contrast, discrimination disappears when more information is available, thus supporting the Statistical Discrimination Theory.

Strikingly, employers preferred fictitious candidates with longer periods of unemployment at the beginning of their employment history compared to those in the Youth Guarantee internship. Moreover, candidates with longer periods of unemployment also had a similar callback rate to those in the regular internship. This occurred even though regular and Youth Guarantee interns had 6 months of more work experience than those that were in the unemployment treatment. This finding indicates that internships that do not lead to retention, as the ones in this experimental design, are not valued by employers. The possibility that non-retention signals low quality of the applicant is unlikely to explain this outcome. Following the internship, the fictitious candidates had a short period of unemployment and were hired afterwards in a similar occupation by another employer. Moreover, they were not discriminated against when applications were randomised to show an additional career break later on in the employment history. More plausibly, this result could be linked to recruiters' understanding that where firms had not retained interns, this was most likely to be because the internship had been used opportunistically to obtain cheap labour, with little intention to provide high quality training (Kazepov and Ranci 2017). Moreover, since Youth Guarantee interns are free labour for employers, employers' incentive to hire through this programme in order to provide good training is even lower than for regular internships. Thus, participation in the Youth Guarantee internship can become a negative signal that is used by employers to discriminate against candidates. When additional information about sectorial IT skills was shown on a resume, it offset discrimination against past Youth Guarantee participants. Consequently, employers are only willing to call back past participants at the same rate as other candidates if they can demonstrate consolidated

job-relevant skills.

Second, by observing employers' behaviour before and after the Covid-19 lockdown, this study reveals how employers rank programme participants in the labour queue, against those in unemployment and those in regular (unsubsidised) internships. After the lockdown, past participants in regular internships were also discriminated against compared to the unemployed. Individuals that would have been relatively unaffected by discrimination in good times were put at a disadvantage after the Covid-19 pandemic because of the tightening of the labour market. This is particularly relevant given the emphasis on policies such as the Youth Guarantee (OECD 2020) to counteract the negative effect of the Covid-19 economic crisis on youth employment.

Third, this study explores heterogeneity in employers' hiring behaviour. This heterogeneity may arise from ALMPs' geographical diffusion in the local labour market and the quality of the job positions to be filled. Evidence indicating a reduction in discrimination due to wider diffusion of the programme is not conclusive. However, results suggest that Youth Guarantee participants receive fewer calls for long term positions.

2 Theoretical Background

2.1 Previous studies

The earliest field experiments that investigated whether participation in welfare programs affect employers' actual hiring behavior were related to wage subsidies for the disabled (Kauer and Deucher 2013; Baert 2016). The purpose of this stream of studies was to investigate how employers weighed the stigma attached to a condition of disadvantage (i.e. a disability) against the economic incentive to hire that was offered by the state. More work has been carried out on employers' hiring behaviour when faced with applicants participating in training programmes, with mixed results (Falk, Lalive and Zweimüller 2005; Kübler, Schmid and Stüber 2019). Falk et al. (2005) find that a computer skills course reduces the chances of an individual being called for an interview as it indicates low levels of IT literacy. On the contrary, Kübler et al. (2019) show that when employers have advertised an apprenticeship position, they are more likely to call back candidates that have participated in a training programme.

Using a factorial study, Liechti et al. (2017) surveyed how recruiters perceived job candidates that had participated in different ALMPs. They found that recruiters had a positive perception of programme participants who were distant from the labour market, but that they negatively rated high quality candidates. Van Belle et al. (2019) used a vignette experiment to uncover whether job seekers under a job vacancy referral scheme had more of a chance of being perceived as hireable by recruiters than applicants applying without support from the Public Employment Services (PES). They found evidence indicating that recruiters consider candidates applying through referral schemes to be less motivated and less hireable. This indicates the presence of negative associations with welfare schemes and PES support. Fossati et al. (2021) argue that the signaling effect of ALMPs can be positive or negative, depending on whether employers believe that participation was voluntary or imposed by caseload workers.

These studies significantly advance scholarly knowledge of how different ALMPs impact employers' perception of candidates. However, they are mostly limited to the context of Belgium, Switzerland and Sweden. In these countries, ALMPs are organised around the compulsory and systematic screening of job seekers by PES. Participation in a certain programme is therefore interpreted by employers as an indirect indication of the screening that has already been conducted by a caseload worker (Liechti et al. 2017; Fossati et al. 2021). Thus, it is unclear what the effect of ALMPs is in countries where PES are less structured, such as in Southern European countries.

While vignette studies are an important tool to study employers' perception (Van Hove and Lievens 2003; Di Stasio 2014; Auer et al. 2015; Di Stasio and Gërkhani 2015; Liechti et al. 2017; McDonald 2017; Van Belle et al. 2018), they do not provide insights on actual hiring behaviour and rely on rather small samples of employers that self-select in the participant pool. This limits our current knowledge on whether employers' attitudes towards ALMPs translate into real outcomes in the labour market.

Moreover, the studies outlined so far that dealt with subsidised employment (Kauer and Deuchert 2013; Baert 2016; Liechti et al. 2017), focused on whether eligibility for a subsidy produced positive economic incentives to hire for employers that balanced the possible negative signaling about candidates' productivity level. However, they do not explore the hypothesis that participation in subsidised employment might *leave a mark* on participants employment history. This is a crucial point, as even if wage subsidies improve employability

in the short term, they could still hinder it afterwards. This might be particularly true if employers do not retain the workers hired through subsidised employment.

Evidence on employment outcomes of job seekers *after* joining subsidised employment is scarce and findings are mixed. Cahuc and Hervein (2020) showed, through the submission of fictitious resumes, that young people who had completed an apprenticeship but were not retained by the firm were not likely to have a higher callback rate from employers than those in vocational education. On the contrary, Hervein and Villedieu (2022) found that participation in a subsidised employment programme increases the callback rate for school dropouts compared to those that were unemployed. However, these studies give little insights into how subsidised employment compares with non-subsidised employment, the other possible alternative to being in a welfare-to-work program. Moreover, these studies do not consider whether past participation *durably* affects future employability, even after further work experience has been gained.

Assessing these long-term outcomes is important for understanding if programme participation has a stigmatising effect that goes beyond what has been empirically measured and theorised in the literature so far. Moreover, the scholarly work outlined has not attempted to distinguish between whether the observed effect of programme participation stems from the employers' lack of information, as predicted by the Statistical Discrimination Theory (Arrow 1972; Phelps 1972), or cognitive bias, as predicted by the Status Characteristics Theory (Correll and Ridgeway 2006). This would be crucial to understanding the mechanisms behind discrimination and to find solutions to counteract it.

2.2 Theories of discrimination in the labour market

The main theories explaining employers' discrimination are Statistical Discrimination Theory (SDT) (Arrow 1972; Phelps 1972), Status Characteristics Theory (SCT) (Correll and Ridgeway 2006) and taste-based discrimination (Becker 1957). According to these theories, recruiters discriminate based on attributes (1) that can distinguish people and (2) that are associated with certain beliefs that link desirable characteristics, such as competence and productivity, to only one category of the attribute (Correll and Benard 2006).

These theories provide different explanations as to why employers discriminate. According to SDT, the source of discrimination is a lack of information. Recruiters faced with little available information will rationally decide to discriminate against candidates who have characteristics that are generally believed to be associated with lower levels of productivity. It follows that in a setting with full information discrimination would not exist. On the other hand, cognitive biases that are produced by stereotypes, and enforced and reproduced through societal hierarchies are identified by SCT as the source of discrimination (Correll and Ridgeway 2006). SCT stems from Expectation States Theory (Berger, Cohen and Zelditch 1972), that was developed to explain how power relations emerge in groups and produce bias in individuals when they are evaluating others. SCT further theorises “how beliefs about status characteristics get translated into performance expectations” (Correll and Ridgeway 2006, pp. 33). Status characteristics are salient attributes, such as gender, ethnicity, or in the case of this study, whether a person joined an ALMP. According to SCT, employers discriminate against candidates that display a visible attribute that has been assigned a lower status (i.e. being a non-white, when considering race). Candidates that belong to lower status groups experience discrimination because employers evaluate them through the lens of persistent socially constructed prejudices and stereotypes.

The crucial element that distinguishes SCT from SDT is the role of available information. According to the SCT, employers would still irrationally discriminate candidates belonging to lower status groups despite the availability of new information. On the contrary, SDT predicts that discrimination results from the rational use of stereotypes when there is limited information available, which would disappear if more information was provided. Finally, taste-based discrimination theory (Becker 1957), like SCT, predicts that more information would not decrease discrimination as it originates from the recruiters’ intrinsic preferences and prejudices. However, taste-based discrimination does not examine the root of such preferences, while SCT identifies it in social power structures.

Both Status Characteristics Theory and Statistical Discrimination are based on beliefs. Participation in an ALMP can be considered a status characteristic, or information that employers could use to statistically discriminate provided that two conditions are fulfilled. These conditions include: (1) past programme participation is salient to employers and (2) individuals who display signs of past participation in a welfare-to-work programme are believed to have lower status and productivity. The belief that certain characteristics are connected to lower social status and productivity is informed by socially enforced

stereotypes. This is how such characteristics in turn produce *stigma* (Link and Phelan 2001). This means that they become “an attribute that is deeply discrediting” (Goffman 1963:3), and this potentially leads to discrimination in the labour market.

2.3 The value of work experience and welfare stigma in the labour market

Why would employers believe that job seekers that have participated in a welfare-to-work program in the past are likely to have lower status or lower productivity levels than those that did not? If we consider the case of subsidised internships, they can be considered as both work experience and a welfare programme as they are paid by the state. These two aspects can send opposite signals to employers.

Additional work experience on the resume can produce two types of positive signals. The first is a human capital accumulation signal (Becker 1962; Liechti et al. 2017). Previous work experience correlates with the acquisition and consolidation of practical skills through on-the-job training. The second is a sign of achievement, as the candidate made the effort to look for a job and was able to convince an employer to offer a paid employment opportunity. Additional work experience can increase a candidates’ status, irrespective of whether it led to the actual accumulation of skills relevant for the job.

Participation in a welfare programme could send a negative signal, as it may represent a stigmatising characteristic and could be a source of discrimination for two reasons. The first reason is that welfare users might be perceived as unwilling or unable to make it on their own. This is linked to the social norm that one should live off one’s work (Lindbeck, Nyberg and Weibull 1999; Lindbeck and Nyberg 2006). The stigma associated to participation in a welfare programme can be conceptualised using the definition provided by Stafford and Scott (1986) that is the “characteristic of a person that is contrary to a social norm”. In this case the characteristic would be past participation in a welfare programme, and the violated social norm would be that a person should be able to manage without external support. People using state help may not be deemed as compliant with this social norm, and they may ultimately become stigmatised (Page 1984). Through this process, the “mark” of having participated in a welfare programme may become a lower status characteristic. The second factor that may lead to the discrimination of participants of welfare programmes is that they are usually already part of stigmatised and marginalised groups. Participation in welfare-to-work programmes might make affiliation with such marginalised groups more

salient. If a welfare programme is targeted towards a particularly disadvantaged group that is already subject to discrimination, then a candidate that is found to be a past participant is likely to be associated with that group and experience discrimination.

When a job is subsidised, the potential positive signals sent by work experience can be weakened or even reversed by the negative signal of participation in a welfare programme. The signal of human capital accumulation can become irrelevant or even negative. Recruiters might interpret subsidised internships as low-quality work experiences. They anticipate that mostly budget constrained firms will choose to hire through these kinds of programmes. In turn, these firms have little incentive to offer employees high quality on-the-job training or opportunities for growth. Fossati et al. (2021) provide evidence that employers perceive subsidised employment programmes as exploitative, especially if the employee is not retained. Thus, they may perceive work experience that is paid with public money to be of little value for human capital accumulation and might even see it as detrimental if it is considered a lost opportunity to upskill through better employment opportunities.

The signal of achievement might be outweighed by the negative associations with welfare programme participation as well. Candidates who found a job through a state subsidy could be perceived as either unwilling or unable to make it on their own, or could be recognised as being part of a stigmatised group. Moreover, this kind of work experience might be perceived of as less of an achievement. It is not possible to tell whether a candidate was hired because they were good or just because they were cheap labour. Thus, rather than gaining more status as a result of additional work experience, past programme participants may end up stigmatised and discriminated against.

In sum, the direction of the human capital accumulation signal depends on the *content* of the ALMP and on its perceived value in increasing candidates' productivity. The direction of the achievement signal is influenced by *prejudice* linked to mere participation in the ALMP, which can be seen as representing a lower status characteristic. If it is the human capital accumulation signal that is driving discrimination against past participants of subsidised internships, providing more information about candidates' productivity should eliminate such discrimination. In this instance, Statistical Discrimination Theory would be supported. If the achievement signal is the most impactful, then the cognitive bias generated by prejudice should drive discrimination against past participants of subsidised internships, even if more information is available. Status Characteristics Theory would then

be supported. If this is the case, then welfare-to-work programmes would carry a high level of stigma, like passive welfare payments. This theoretical framework gives rise to the testable hypotheses outlined in the next paragraph.

2.4 Hypotheses at test

There are two possible counterfactuals to job applicants who took part in a subsidised internship at the beginning of their employment history: (1) candidates that were employed without public support and (2) candidates that were unemployed in the same time period. Public support is expected to act as a negative signal when past programme participants are compared with those employed without public support. Past literature has shown that the stigma attached to welfare is the driver of several phenomena such as benefit dependence (Page 1984; Bane and Ellwood 1994) and the non-take-up of welfare provisions (Moffitt 1983). Considering the far-reaching consequences of welfare stigma, welfare-to-work programmes are expected to produce a cognitive bias in recruiters, who may discriminate against programme participants even when additional information is available. At the same time, past participants of welfare-to-work programmes can have better chances of being hired than candidates with a history of unemployment. The scarring effect of unemployment (Omori 1997; Ho et al. 2011; Kroft, Lange and Notowidigdo 2013; Eriksson and Rooth 2014) is expected to be stronger than participation in the Youth Guarantee, as those unemployed are accumulated less work experience than those participating in subsidised internship programmes. Thus, I formulate the following hypotheses:

Hypothesis 1a. *Employers discriminate against past participants in the Youth Guarantee internship compared to those that did a regular (unsubsidised) internship during the same time period.*

Hypothesis 1b. *Employers are more likely to invite past participants in the Youth Guarantee internship for an interview, compared to those that were unemployed during the same time period.*

Hypothesis 1c. *Employers discriminate against past participants in the Youth Guarantee internship even when additional information about candidate's*

productivity is available. This reverses the (positive) achievement signal that is linked to work experience, supporting Status Characteristics Theory.

Even less is known on whether discrimination against past participants in welfare programmes may be exacerbated during periods of economic downturn. Queuing Theory (Reskin and Roos 1990; Model and Ladipo 1996; Waldinger 1996; Reskin 2019) suggests that employers rank candidates in labour queues based on attributes such as gender and ethnicity, and that they place lower status applicants at the bottom. When job vacancies decrease compared to applicants, employers will only hire job seekers at the front of the labour queue and will push further back those that are already disadvantaged in normal times. Previous empirical studies testing Queuing Theory show results that point in different directions. Vuolo, Uggen and Lageson (2017) found that the 2007 Great Recession did not exacerbate further racial inequalities in hiring in the United States. However, Visser (2017) showed that ethnic minorities were less likely to be hired in high quality jobs. The OECD declared that the economic crisis that followed the Covid-19 pandemic exacerbated existing labour market inequalities (OECD 2020). Thus, based on Queuing Theory the following hypotheses are formulated:

Hypothesis 2a. *During periods of economic downturn employers discriminate more against past participants in the Youth Guarantee internship, compared to those that did a regular (unsubsidised) internship.*

Hypothesis 2b. *During periods of economic downturn employers are more likely to invite past participants of the Youth Guarantee internship for an interview, compared to those that were unemployed during the same time period.*

Previous studies showed that participation in welfare programmes was positively affected by diffusion in the immediate surroundings (Oster and Thornton 2009; Dahl, Løken and Mogstad 2014; Holford 2015). This literature suggests that this is driven by a lower perception of stigma attached to programme participation as diffusion increases. Through a similar mechanism, policy diffusion might also affect employers' perception and hiring behaviour. If taking part in ALMPs is common practice among job seekers in the local labour market, this might weaken the negative signal attached to past participation and reduce discrimination against past participants. The following hypothesis is formulated:

Hypothesis 3. *Employers discriminate more against past participants in the Youth Guarantee internship in regions where it is less commonly used than those where its use is more widespread.*

Discrimination might also depend on the quality of the job vacancy that needs to be filled (Visser 2017). If recruiters need to fill a long-term position, they will have more pressure to select the best possible candidate, as firing is costly. This might lead them to discriminate more. On the contrary, employers offering low quality jobs might have to juggle two opposite needs: finding a good candidate, and avoiding candidates with enough bargaining power to negotiate or reject the offer. In such a situation, recruiters might need someone that is good enough, but not “too good”, thus potentially reversing discrimination. This gives rise to the following hypotheses:

Hypothesis 4a. *Past participants in the Youth Guarantee internship are discriminated against more by employers that offer long term jobs, than candidates that did a regular (unsubsidised) internship.*

Hypothesis 4b. *Past participants in the Youth Guarantee internship are more likely to be invited for an interview by employers offering short term jobs, compared to candidates that did a regular (unsubsidised) internship.*

Hypothesis 4c. *Past participants in the Youth Guarantee are more likely to be invited for an interview by employers offering long term jobs, compared to those that were unemployed during the same time period.*

Hypothesis 4d. *Past participants in the Youth Guarantee are discriminated against by employers offering short term jobs, compared to those that were unemployed during the same time period.*

3 The Italian labour market and the Youth Guarantee

Since the Financial Crisis 2008 Italy has experienced sluggish economic growth. The unemployment rate was at 10% in 2019 (Eurostat 2022b), which is well above the 6.3% average in the European Union. The Covid-19 outbreak and the country’s subsequent lockdown in March 2020 further deteriorated the conditions of the Italian labour market. The employment rate decreased by 1.6% while the rate of inactive individuals aged 15-64 increased by 6.6% in the period March-May 2020, compared to December 2019-February 2020 (Istat 2020a). In the

first months of the lockdown, between March and May 2020, 45% of firms with more than 3 employees suspended their activities and 4 in 10 lost more than 50% of their revenues (Istat 2020b). The Italian government attempted to limit the economic disruptions caused by the lockdown through the approval of several measures. The most important was the Covid Wage Guarantee Fund (in Italian “Cassa Integrazione Guadagni Covid”), a wage subsidy that supported employers in paying employees while activities were suspended or work hours reduced. The Wage Guarantee Fund existed in Italy also before the pandemic, and it was used by firms in periods of economic restructuring. However, with the pandemic this instrument was extended significantly, involving 63.1% of firms by May 2020 (Italian Ministry of Work et al. 2020). Overall, the Covid Wage Guarantee Fund represented around 70% of the total expenditure for pandemic measures – 19 billion euros over a total of 27 billion (Italian Ministry of Work et al. 2020). This wage subsidy came together with government regulations forbidding firms from firing employees. This combination of support packages, regulations and uncertainty regarding the future negatively impacted labour demand. Even though not all the economic sectors were targeted by the closures imposed by the lockdown, the vacancy rate decreased sharply in 2020 compared to the pre-pandemic years in both industrial and service sectors (Istat 2022a). The impossibility to fire employees with permanent contracts led to a significant use of the Covid Wage Guarantee Fund to reduce the cost of labour. At the same time, the intake of new personnel was reduced. Between March and May 2020 only 1.8% of firms hired new employees and 12.2% decided to delay plans to hire. Moreover, 6.6% of firms reduced their labour costs by not renewing temporary contracts due to expire (Italian Ministry of Work et al. 2020).

Furthermore, the negative economic shock generated by the pandemic severely impacted youth unemployment (OECD 2020), which was already one of the highest in Europe. In 2019, the rate of unemployment for those aged between 15 and 29 in Italy was of 22.4% against a European average of 11.1% (Eurostat 2022c). The phenomenon of rising youth unemployment in developed countries gave rise to a new category with a name of its own: the NEET (Bynner and Parsons 2002; Batini et al. 2017). The acronym NEET stands for “Not in Employment nor in Education or Training” and refers to young people that do not work and are not studying. Italy has the highest percentage of NEETs in Europe. In 2021, as many as 23.1% of young people aged 15-29 (Eurostat 2022b) were classified as NEETs. Part of the reason for this was the failure of the Italian educational system to facilitate the school-to-work transition, making entry into the labour market far from a smooth process (Alfieri et al. 2015; Pastore 2019).

Due to this situation, a lot of effort was made by the state to devise policies which could reintegrate young job seekers into the labour market. The most important policy so far was the Youth Guarantee Programme, which was part of the European Commission's *Social Investment Package for Growth and Social Cohesion* which was launched in Italy in May 2014 (European Commission 2013). In Italy participants to the Youth Guarantee Programme had to be between 15 and 29 years old, and they must have been unemployed and not pursuing any study or training programme. Thus, the NEET were the exact target of this policy. Those that decided to sign up for the programme received support from the Public Employment Services (PES) and could access training, occupational incentives, volunteering, and internship opportunities (Isfol 2016). Among the NEET recipients of an activation measure, 57% of them joined a subsidised internship, which represented the most common intervention (Anpal 2020).

The Youth Guarantee internship is funded by public money. It therefore represents a form of subsidised employment for employers. The main assumption of this programme is that subsidised internships would provide young job seekers an opportunity to gain valuable work experience, and that it would improve their employability prospects. Previous work focusing on the supply side of labour showed the positive effect of participation on the probability of being employed within 18 months from initial contact with the PES (Anpal 2019). However, this evidence is based on propensity score-matching which compares participant and non-participant NEETs, where the former tend to have systematically higher education levels and are rated as more employable by the PES.

A field experiment such as the one proposed in this study provides estimates that are not affected by selection bias, as profiles are randomly assigned to participation in the programme. Moreover, at the end of the 18 months period only 53.4% of internships participants were employed, pushing a substantial part of them back into the labour market searching for a job (Anpal 2019). It is therefore important to consider the employers' perspective and understand if participation in this ALMP has any impact on their hiring behaviour in the long term. Employers may discriminate against individuals that display signs of participation in the programme, which undermines the underlying assumption of this policy intervention.

However, there are two conditions that need to be fulfilled for a welfare programme to become a potential trigger for discrimination in the hands of employers. The first is that the sign of past participation should be visible on the job seekers' resume. Second, the employers must be aware of the programme so that it can be used as a cue in the hiring process. Regarding the first assumption, applicants that participated in the Youth Guarantee Programme often

specify in their resume that the internship was done through the programme, in order to justifying the short duration of the work experience. The Youth Guarantee is linked to an internship contract with a maximum duration of 12 months. A more thorough search of LinkedIn profiles further evidenced that job seekers that had previously participated in the programme tend to mention it when describing previous work experience in their profiles. This suggests that the signal of participation in the welfare programme is available.

The second assumption is also fulfilled, as there is evidence that firms in Italy are acquainted with the Youth Guarantee Programme. A study carried out by an Italian private employment agency on a sample of 515 Italian companies showed that 56.1% knew the Youth Guarantee Programme by September 2014 (Gi Group 2014). This is a high percentage considering that the data collected only refers to the first five months after the initial implementation of this policy. I expect that in 2020 even more employers were informed of the existence of this program. Overall, past participation in the Youth Guarantee Programme has the potential to be used as a cue by employers to discriminate against job applicants.

4 The experimental design

4.1 General procedure

Discrimination against recipients of the Youth Guarantee Programme by employers was detected through a field experiment, in the form of a correspondence study. This method is best suited to observing the real behaviour of employers, free of social desirability bias. Recruiters select the fictitious candidates as part of the actual hiring process, as they are not aware that they are participants in an experiment (Gaddis 2018). This also presents clear ethical concerns¹ (Banton 1997; Riach and Rich 2004; Zschirnt 2019). However, the potential ethical issues that arise are admissible in light of the relevance of understanding employers' hiring behaviour, especially in relation to discriminatory practices. Moreover, if discrimination is grounded in participation in taxpayers funded programmes, it becomes even more important to study whether they may have unintended consequences.

For this study 4,066 fictitious resumes were sent out to real employers in 11 Italian cities between September 2019 and June 2020. Figure 1 shows the geographic distribution of the

¹ This study is part of a larger project that received Ethics Clearance from the Ethics Committee of the European University Institute in November 2018.

cities and the characteristics of the regions where they were located. The regional rate of NEET in the Italian territory is unevenly distributed (Figure 1(a)). Southern regions show a percentage of NEET of up to 38%, while it reaches a minimum of 11.1% in Northern regions. The cities used for the experiment were distributed across Northern, Central and Southern regions. This made it possible to study employers' discrimination net of specific local labour market dynamics, increasing the external validity of the experiment. The geographical distribution of the Youth Guarantee Programme does not follow a straightforward gradient from North to South (Figure 1(b)). The diffusion of the measure depends not only on the presence of the NEET in the region, but also on the efficiency of the Public Employment Services that are responsible for managing the measure.

The experiment targeted six job positions: human resources (HR specialist and generalist), marketing, social media manager, accountant, and secretary. These occupations qualify as white collar, back-office jobs in the private service sector. The focus on this latter sector is motivated by its prominent role in job creation, thus being highly relevant for the study of labour demand. In Italy in years 2017-2020 the private service sector had both the highest share of employees and vacancy rate across economic sectors (Istat 2022a). Thus, the occupations included in the experiment represent a relevant share of those employed and of jobs that are in demand. Furthermore, job posts for the selected white-collar occupations tend to be highly standardized, simplifying the creation of fictitious resumes fitting a wide range of jobs (Patacchini, Ragusa and Zenou 2015). These factors were essential to reach a large enough sample size. The choice to restrict the experiment to back-office jobs, within the broader category of white-collar jobs, is motivated by the literature on physical appearance and hiring in Italy. There is evidence that employers discriminate unattractive candidates especially in front-office jobs (Patacchini et al. 2015; Busetta et al. 2021). Therefore, the experimental design was simplified by focusing only on back-office jobs and avoiding profile pictures on resumes. An informal investigation carried out among recruiters working at 4 different companies prior to the beginning of the study further confirmed that the omission of profile pictures on resumes would not significantly affect the hiring process for the occupations targeted in the experiment.

Job profiles were designed to fit job posts in 11 cities, for six occupations. In total the experiment counted 66 available job profiles (6x11). All of the profiles had the following fixed characteristics: a business-oriented high school diploma (this type of school is called "Ragioneria" in the Italian educational system), a bachelor's degree in Economics and Business, a certified C1 level of English, and the ability to use of the Microsoft Office package.

All of the fictitious candidates had a minimum of 32 months of work experience and were between 27 and 28 years old throughout the experiment. The fixed characteristics described were selected to ensure that fictitious candidates qualify for most of the job posts in the occupations targeted. Given the choice of white-collar occupations, resumes were all assigned a bachelor's degree to maximize callback probability and minimize power issues. In the Italian labor market, characterized by stiff competition for jobs (Saraceno 2015), tertiary education is likely to make fictitious candidates stand out from the rest of the applicant pool (Modestino, Shoag and Balance 2016).

The profiles generated were attractive enough while still representing plausible past Youth Guarantee participants. The occupations targeted by the experiment belong to the two ISCO-08 occupational macro-categories of clerical support workers and technical professionals. These occupational macro-categories represent 36.8% of young people employed through the Youth Guarantee internship (Anpal 2019). Moreover, NEET that are Youth Guarantee participants are more likely to be university educated than non-participants (Anpal 2019). Therefore, it is plausible that tertiary education and participation in the ALMP could be on the same resume. This is unsurprising given the difficult school-to-work transition and low return to education that characterises the Italian labour market, even for university graduates (Naticchioni, Ricci and Rustichelli 2010; Pastore 2019).

The employers were reached online, through common job search platforms available in Italy (i.e, Indeed.com and Bakeca.it). Job posts with the following characteristics were sampled on a day-to-day basis: (1) employers had to be within a range of a maximum of 30 km from one of the 11 cities included among the available profiles (95% where within 20 km from the city centre), (2) the job advertised had to be recent (93% of the resumes were sent within 2 days of the job opening), (3) the minimum educational and work experience requirements had to be equal or below those of the fictitious candidates. These criteria were important to maximise the probability of being called back by an employer.

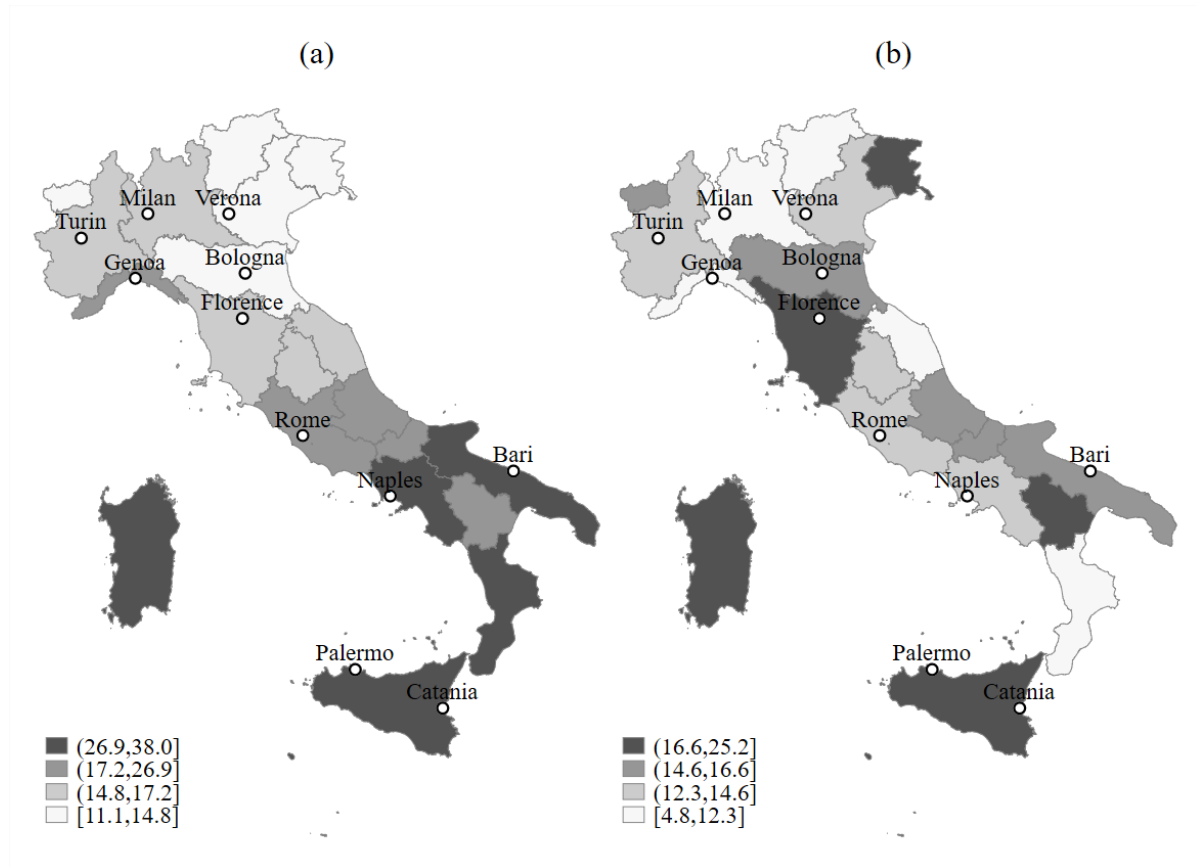


Figure 1 Geographical distribution of cities included in the study, regional rate of NEET in 2019 (panel a) and regional rate of Youth Guarantee use by 2019 (panel b).

Notes: The variable in panel (a) is the ratio at the regional level between the young people aged 15-29 that were not in employment, education or training and the population of young people aged 15-29 at the regional level in 2019. The variable in panel (b) is calculated as the ratio at the regional level between the young people that used the Youth Guarantee Programme by the end of 2019 and the population of young people aged 15-29 in 2019.

Source: The variable in panel (a) is retrieved from Istat (2022b). The variable in panel (b) is calculated by the author based on the Anpal (2020) report data on Youth Guarantee and Istat (2022b) population data.

Once the job posts matching these criteria were sampled, the selected employers would receive 4 applications during the same day, including a resume and a cover letter. Table 2 summarises the treatment levels available in the experiment. The large variation in the information displayed in the resumes reduced chances of the experiment being detected. Detection was further minimised by using slightly different templates for the resumes and cover letters that were sent to the same employer. Four templates were used for the resumes with varying fonts and colors. Furthermore, the cover letters were occupation specific. There were four cover letter templates for each of the six occupations considered in the experiment. Each of the 24 cover letter templates available (6x4) had similar content but slightly different

wording to avoid detection of the experiment. An example of a resume and cover letter is respectively available in figures A1 and A2 of the appendix.

The dependent variable of interest was whether employers ultimately contacted the fictitious job applicants through either an email with an invitation for an interview or a phone call. An email account was created for each surname-name combination that could be randomly assigned to a resume, for a total of 72 email accounts². The surname of each fictitious job applicant was linked to a phone number, for a total of 10 surnames and 10 phone numbers. The randomisation of the surnames was designed to avoid each employer receiving two or more resumes with the same surname. Each surname was linked to one phone number and a surname would not be repeated more than once in a batch. Therefore, when an employer contacted the number on the resume it was possible to univocally identify which resume in the batch had received the call.

4.2 Treatments design

The main treatment of interest consisted of fictitious resumes that were randomised to display one of the following conditions: 6 months of subsidised internship through the Youth Guarantee Programme, 6 months of regular internship (unsubsidised), and 6 months of unemployment. The choice of the duration of the internship was in line with the distribution of the actual Youth Guarantee internships, which have an average duration of 6.2 months (Anpal 2019). These three treatment conditions were assigned with equal probability. Randomisation and generation of fictitious profiles were performed through an adaptation of the computer program built by Lahey and Beasley (2009).

In Table 1 I detail the possible employment histories arising from the random combination of treatments introduced, presented in Table 2. The internship treatment was placed at the beginning of the fictitious candidates' employment history. Before the internship each fictitious candidate completed a bachelor's degree and spent four months in unemployment. This ensured eligibility for the Youth Guarantee Programme. After the 6 months internship (or 6 months in

² In the experiment 8 Italian sounding names and 8 Italian sounding surnames were available, for a total of 64 possible Italian sounding surname-name combinations. Moreover, 2 Romanian sounding surnames and 4 Romanian sounding names were available, for additional 8 possible surname-name combinations. Finally, 72 (64+8) surname-name combinations were available.

unemployment), all the resumes had a short unemployment spell³ and then displayed a fixed amount of work experience. Placing the treatment of interest at the beginning of the employment history could weaken its strength. Employers might focus on the most recent work experience, and therefore ignore the internship. Nevertheless, it was important to have it at the beginning of the employment history, rather than at the end. This way the mention of the Youth Guarantee Programme acts exactly as a ‘mark’ of past programme participation. Any variation in the call back rate of resumes mentioning the Youth Guarantee Programme can therefore be attributed to the signaling effect of the programme that persists even after more work experience has been accumulated.

The experiment focused on the case of internships that do not directly lead to retention because they represent the most common real-world scenario. In years 2014-2019 only 29.9% of internships in Italy resulted in hiring by the same employer within 6 months from their end (Anpal 2021). This means that at least 70.1% (=100-29.9) of interns were not immediately retained at the same firm. Similarly, profiles of candidates that entered standard employment directly after university were not included because they are rather uncommon, given the lengthy school-to-work transition that characterizes the Italian labor market (Di Stasio 2014; Pastore, Quintano and Rocca 2021; Pastore, Quintano and Rocca 2022). In Italy the average duration of the transition from university to the first (non-internship) job is about 1.36 years (Pastore et al. 2021). Thus, resumes showing the Youth Guarantee and regular internships were compared with profiles displaying in total at least 12 months of unemployment⁴ before finding the first (non-internship) job.

After the internship treatment, the resumes had a 50% probability of displaying either a continuous employment history (CV1 in Table 1), or a discontinuous employment history (CV2 in Table 2). The internship and the subsequent work experiences within a resume were at different firms but they were all in the same occupation. This resulted in coherent work experience overall. The duration of unemployment after the last job was also randomised, by changing randomly the end date of the last occupation. Finally, the size of the firms where the work experience was attained was also randomised. These treatments are important to avoid results that are driven by a specific composition of the employment history.

³ The unemployment spell after the internship treatment is of 4 months when the resume is randomized to the employment history in Table 1 CV1, and of 2 months when the resume is randomized to that in Table 1 CV2. This feature of the design keeps the total time in unemployment fixed to 4 months from the end of the internship treatment to the last work experience.

⁴ When the internship treatment is randomized to 6 months in unemployment and: (1) the resume is assigned to CV1 in Table 1, then the first spell of unemployment is 14 months long; (2) the resume is assigned to CV2 in Table 1, then the first spell of unemployment is 12 months long.

Following Neumark (2018), the experiment included treatments providing for additional information that could be used by employers to infer candidates' productivity. They were meant to assess the role of unobservable characteristics in the hiring process, and enabled Statistical Discrimination Theory to be tested against the Status Characteristic Theory (Foley et al. 2022). Four treatments detailing additional information were included: (1) an elevator pitch mentioning availability to work overtime and relocate, signaling a commitment to work (2) participation in a training course, which signaled trainability and a willingness to learn (3) the mention of sectorial IT skills, which signaled technical skills that were relevant to the job (4) and participation in volunteering activities, which signaled cooperative and altruistic attitudes which may have been potentially valuable for employers (Katz and Rosenberg 2005; Baert and Vujić 2016). Each of these treatments had a 50% chance of being displayed on the fictitious resumes. If employers stop using participation in the Youth Guarantee Programme as a sorting criterion when additional information is provided, then Statistical Discrimination is the driver of the observed hiring behaviour. If instead employers discriminate against job applicants irrespective of the presence of additional information, then it means that discrimination is not due to a lack of information, but rather due to cognitive bias (Correll and Benard 2006), supporting Status Characteristics Theory.

Finally, gender (signaled by name), ethnicity (signaled by surname), city of birth and city of study were randomised across resumes. It is important to note that there was no stratification of treatments by employer. Each resume was randomly assigned to each of the treatments independent of all of the other resumes that were sent to the same job position and to other employers. Through the comparison of seven audit studies, Phillips (2019) shows that matched and stratified designs can induce an underestimation of discrimination that can be as large as 19%. This is driven by potential spillovers, as the recruiters' decision on one application may also depend on the characteristics of the applicant pool. To avoid this bias, the strategy adopted was to randomise characteristics within and between resumes batches. In practice this means that employers could receive any random combination of four resumes, where each resume showed either the Youth Guarantee internship, the regular internship or an unemployment gap with 1/3 probability. Since each employer received four resumes and the internship treatment had only three levels, by design at least two resumes in each batch were randomly drawn to have the same internship treatment. The same was also true for other treatments. For example, the same employer could receive four resumes which all indicate continuous work experience as, can be seen in CV1 Table 1. Equally, an employer could also

receive resumes which all show a discontinuous work experience, as illustrated in CV2 Table 1, or the employer may receive any other combination of these two. Every resume had a 50% probability of being assigned to one of the two work experiences, independent of all the others in the same batch. Thus, employers did not receive a fixed combination of resumes.

Table 1 Employment and education history structure (from the most recent to the oldest).

CV 1	CV 2
Randomised unemployment duration: 0, 2 ,6 14, 22 months	
Randomised employment history structure (2 treatment conditions):	
1 work experience (32 months minimum)	2 work experiences (total 32 months minimum): <ul style="list-style-type: none"> • Most recent work experience (20 months minimum) • 2 months of unemployment • First work experience (12 months)
4 months of unemployment	2 months of unemployment
Randomised internship: <ul style="list-style-type: none"> • Unemployment 6 months • 6 months internship with the Youth Guarantee (paid by the state) • 6 months regular internship (paid by the employer) 	
4 months of unemployment (fixed)	
BSc in Economics and Business (fixed)	
High school (fixed)	

Note: one month was added to the last work experience for every month of data collection. Consequently, resumes sent in September 2019 display 32 months of work experience, while those sent in June 2020 have 41 months of work experience. This was necessary to keep the unemployment duration treatments at the end of the work history constant throughout the study.

Cover letters highlighted the skills that had been acquired by fictitious candidates throughout their whole employment history and were relevant for the targeted occupations. The jobs listed in each resume were all in the same occupation (but a different employer), and involved similar tasks and skills. Letters did not address specific work experiences, which made them suitable to all resumes irrespective of the employment history that was randomly drawn. This means that the Youth Guarantee internship and the regular internship were not explicitly mentioned in the cover letters. Explicitly referring to an internship on the resume but not in the cover letter could potentially send a negative signal to employers. They could interpret the internship as low-quality work experience, especially if ‘real-world’ cover letters mention internships. However, job seekers are typically encouraged to focus on their most recent experiences when writing cover letters (Farr 1994; Rosenberg 2007). By design, all fictitious candidates worked for at least 32 months after the internship or the unemployment gap that was placed at the beginning of their employment history. Recent work experiences are likely to be deemed more valuable, and therefore it would be unusual for real candidates, with similar employment characteristics, to mention their first internship in a cover letter.

Before running the experiment, power calculations were performed through the *DeclareDesign* package (Blair et al. 2019). I simulated the experimental data assuming (1) that resumes that display the Youth Guarantee internship have a callback rate of 10%, (2) that the callback rate for the Youth Guarantee internships is 3.5 percentage points lower than the callback rate for those in a regular internships and 3.5 percentage points higher than those with an unemployment gap (3) a 0.05 significance level. The assumed baseline callback rate and effect sizes are in line with those from a correspondence study run in Italy by Patacchini et al. (2015). Like the design of this experiment, their main treatment was represented by participation in an internship, which was randomised to be in a pro-gay advocacy association to reveal discrimination against homosexuals. In the simulation a sample size of $N=4,000$ allowed sufficient power to detect differences in callback rate between the resumes in different treatment conditions⁵. Thus, with a sample size of 4,066 resumes the experiment should have been sufficiently powered to detect the effect of the three levels of the internship treatment.

⁵ The difference in callback rate between the Youth Guarantee internship and the regular internship treatments could be detected with a power of 0.82, and the difference between the Youth Guarantee Internship and the unemployment treatment could be detected with a power of 0.92.

Table 2 Treatment levels

Main treatment	Treatment levels		
Internship	6 months Youth Guarantee internship	6 months regular internship	6 months unemployment
Work experience treatments			
Work experience(s) after internship	One work experience	Two work experiences	
Unemployment spell after last employment	Employed (0 months)	Short spell (2 or 6 months)	Long spell (14 or 22 months)
Firm size last work experience	Large	Medium	
Firm size first work experience	Large	Medium	
Firm size internship	Large	Medium	Small
Additional information treatments	Treatment levels		
Sectorial IT skills	Included	Not included	
In training	Included	Not included	
Volunteering	Included	Not included	
Commitment to work	Included	Not included	
Sociodemographic treatments	Treatment levels		
Sex	Male	Female	
Ethnicity	Italian-sounding surname	Romanian-sounding surname	
City of birth	City in Northern Italy	City in Central Italy	City in Southern Italy
City of education	Same as city of birth	Same as city of job post	

Note: The treatment “Firm size first work experience” applies to the first work experience when the resume is randomised to receive two work experiences after the internship. Small size firms have 2 to 10 employees, medium size firms have 11 to 50 employees and large firms have more than 50 employees.

5 Results

5.1 Descriptive statistics

Among the 4,066 fictitious resumes sent out between September 2019 and June 2020, 460 were contacted back, by phone or email. The total callback rate was 11.3%. The sample is slightly unbalanced towards employers in the North of the country, where more job openings are normally available due to better economic conditions. Out of all of the resumes sent, 44.4% were addressed to employers in Northern Italy; 35.2% in Central Italy; and 20.4% in Southern Italy. Most of the job applications were sent to the three main cities of Milan (28.8% of applications), Rome (21.5% of applications) and Naples (12.6% of applications). This was mostly due to the greater availability of job openings in big cities when compared to smaller ones. The sample is less balanced in terms of the job sectors targeted. Most of the resumes were sent for the accountant and administrative assistant/secretary profiles (67.2% of the applications) and for marketing positions (26.5%). Only a small number of applications were sent for jobs in human resources (6.3%).

Most of the resumes were sent for job posts which mentioned or required a Bachelor's degree or less (51.8%), less than 1% job posts that were applied to required more than a Bachelor's degree, and the rest did not mention the educational level required. In terms of the hard skills that were required, 80.5% of job posts mentioned or required experience, 36.6% mentioned English, 45.0% required IT skills, and 19.5% mentioned other specific skills that were necessary for the job. All of the resumes were assigned by design a Bachelor's degree, basic IT skills, knowledge of English, and at least 32 months of work experience. This represented the ideal candidate for the job posts that were sampled.

The job posts also mentioned soft skills. Among all of the resumes submitted, 14.2% were sent to employers that emphasised trainability and the ability to learn; 23.4% of job posts highlighted teamwork orientation; and 21.2% referenced a commitment to work. These soft skills are closely related to the additional information treatments which show participation in a training course, in volunteering, and the elevator pitch which referred to a commitment to work. Therefore, these variables were considered in the multivariate analysis.

In terms of programme diffusion, 21% of the resumes were sent to employers located in regions where the ratio between Youth Guarantee users and the population aged between 15-29 was above the median. Moreover, only 18% of resumes were sent to employers offering

long-term contracts; 31% offered a short-term contract; and 17% offered a short-term contract with the perspective of organising a long-term arrangement afterwards. The rest of the job posts did not mention the contract type.

Table 3 Descriptive statistics

Variable	Obs	Mean	Std. dev.	Min	Max
Total callback rate	4,066	0.11	0.32	0	1
<i>Macro area</i>					
Northern city	4,066	0.44	0.50	0	1
Centre city	4,066	0.35	0.48	0	1
Southern city	4,066	0.20	0.40	0	1
<i>Job sector</i>					
Accounting and secretary	4,066	0.67	0.47	0	1
Human Resources	4,066	0.06	0.24		
Marketing and social media	4,066	0.26	0.44	0	1
<i>Education mentioned in job post</i>					
Not mentioned	4,066	0.47	0.50	0	1
Bachelor's degree not required	4,066	0.30	0.46	0	1
Bachelor's degree preferred or required	4,066	0.21	0.41	0	1
Master's degree mentioned	4,066	0.01	0.10	0	1
Experience mentioned in job post	4,066	0.80	0.40	0	1
English mentioned in job post	4,066	0.37	0.48	0	1
Specific skills mentioned in job post	4,066	0.20	0.40	0	1
IT skills mentioned in job post	4,066	0.45	0.50	0	1
Learning attitude mentioned in job post	4,066	0.14	0.35	0	1
Teamwork orientation mentioned in job post	4,066	0.23	0.42	0	1
Commitment to work mentioned in job post	4,066	0.21	0.41	0	1
After Covid	4,066	0.22	0.42	0	1
Regional YG diffusion rate above median ^a	4,066	0.21	0.41	0	1
<i>Contract offered in job post</i>					
NA	4,066	0.34	0.47	0	1
Short-term	4,066	0.31	0.46	0	1
Short-term with perspective	4,066	0.17	0.38	0	1
Long-term	4,066	0.18	0.38	0	1

Note: (a) the reference is the distribution of the regional Youth Guarantee diffusion rate across the 20 Italian regions.

The callback rate was 10.1% for resumes displaying the Youth Guarantee internship, 11.6% for those displaying the regular internship and 12.3% for resumes displaying 6 months of unemployment (Figure 2(a)). A two-sided t-test rejects the null hypothesis that the callback rate for the Youth Guarantee resumes is the same as for those unemployed with $p < 0.1$. Of all the resumes, 22% were sent after March 2020, the start of the first lockdown in Italy due to the Covid-19 outbreak. After that, the callback rate dropped sharply for all the resumes, from 12.3% to 8.0% (Figure 2(b), $p < 0.001$ two-tailed t-test). However, the drop did not affect the callback rate for all the levels of the internship treatment in the same way. After the lockdown the callback rate for resumes displaying the regular internship had the sharpest drop, from 13.3% to 6.0% ($p < 0.001$ two t-tailed test) while it was less pronounced for the other two treatment levels (Figure 2(c)).

The callback rate shows significant variations by internship treatment when one of the additional information treatments is present on the resume (Figure 3). The resumes in the Youth Guarantee treatment received less calls compared to the rest when additional information was not included. However, the magnitude and significance of this effect varied across additional information treatments. When one of the additional information treatments was included in the resume, the callback across treatments tended to equalise. This was particularly pronounced when sectorial IT skills were shown in the resume. The difference in callback rate between the Youth Guarantee internship and the unemployed became significant with $p < 0.01$ (two-tailed test) when no IT skills were shown (Figure 3(a)). In the next paragraph this descriptive evidence is further explored through regression analysis.

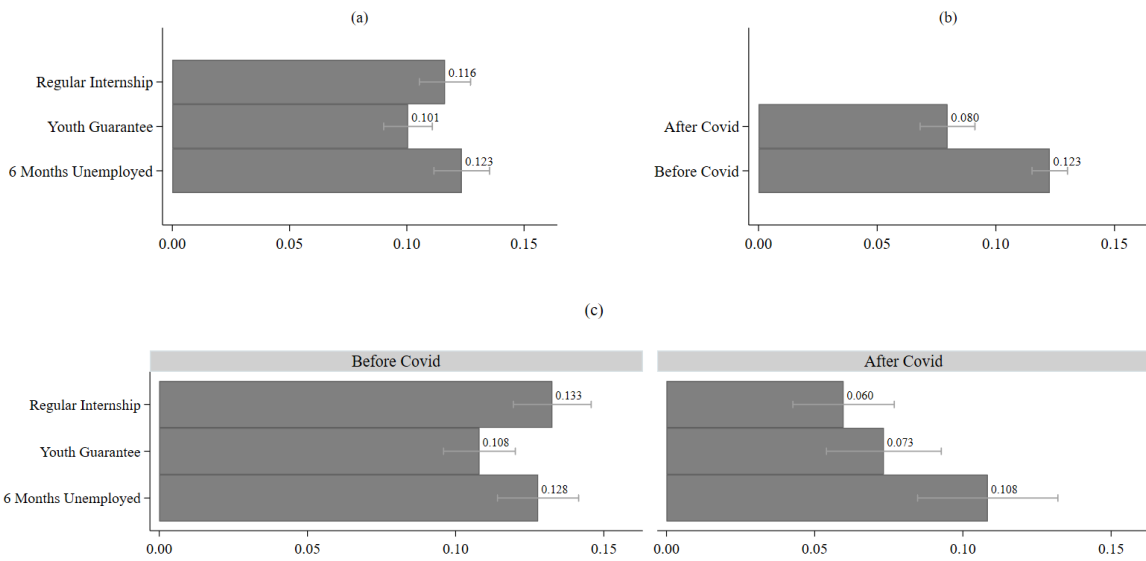


Figure 2 Total callback by internship treatment (a) before and after Covid-19 outbreak (b), and by both internship treatment and Covid-19 outbreak (c).

Note: 90% Confidence Intervals are displayed. In panel (a) and (b) N=4,066, in panel (c) N= 3,162 before Covid and N=904 after Covid.

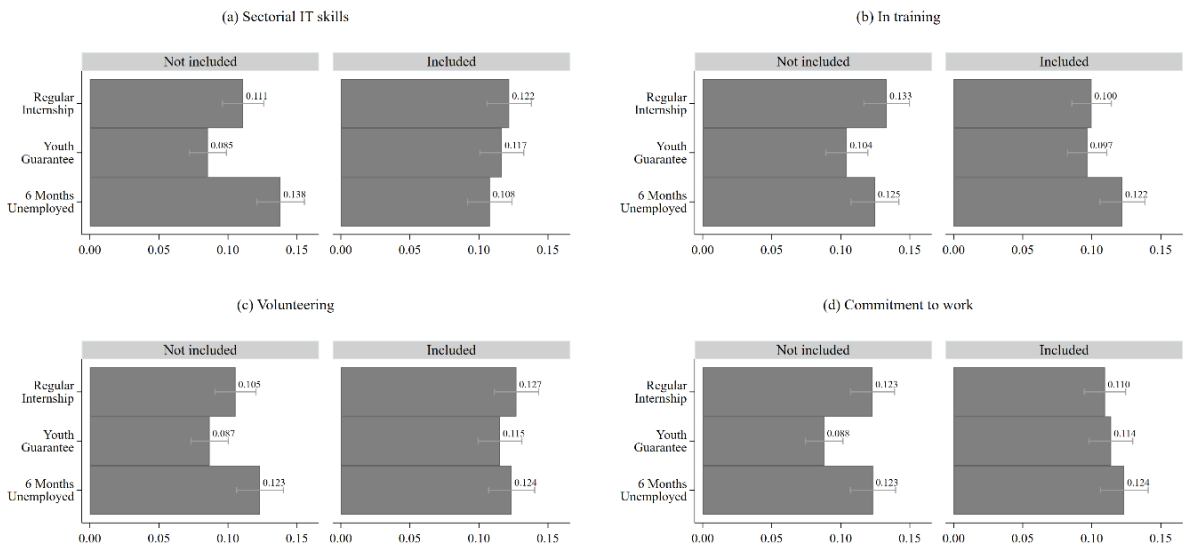


Figure 3 Total callback by internship treatment and type of additional information mentioned in resume.

Note: 90% Confidence Intervals are displayed. N=4,066.

5.2 Main results

Figure 4 Model 1 shows the average marginal effect of the internship treatments on callback rate, the additional information treatments, and the other treatment conditions described in Table 2. Controls are included for geographic area, month, job sector, number of days since the job post was published, distance from the workplace, whether the resume was sent before or after the first Covid-19 lockdown, and other application controls. These controls are included in the specification together with hard skills (including experience, knowledge of English, IT skills, education, request of specific skills) and soft skills (learning attitude, teamwork orientation and commitment to work) that were mentioned in the job post. These variables increase the precision of the estimates, and control for the match between the resumes and the job posts.

In Model 1 and 2 of Figure 4, the Youth Guarantee treatment significantly decreases the callback probability by 2.4 percentage points compared to those that had been unemployed for six months ($p < 0.05$). At the same time, the regular internship only slightly decreases callback probability by 0.6 percentage points ($p > 0.1$), which is not significant at conventional levels. Resumes displaying the Youth Guarantee internship experience a 19.3% ($= 2.4/12.4$) decrease in the callback probability compared to those displaying an additional 6 months unemployment gap, and a 15.4% ($= [2.4 - 0.6]/11.7$) reduction in callback probability compared to those displaying a regular internship⁶. While confirming hypotheses 1a, this evidence disconfirms hypothesis 1b, positing that Youth Guarantee participants would be preferred to those that were unemployed, as they had six months more work experience.

In Figure 4 Model 2 I test whether the estimate in Model 1 is robust to the inclusion in the specification of the amount of additional information that was present on the resume. This variable is derived from the additional information treatments and has three categories. The first refers to resumes where 0 to 1 of the additional information treatments were displayed (32.4% of observations); the second concerns resumes where 2 additional information treatments were displayed (36.3% of observations); and the third relates to resumes where 3 to 4 additional information treatments were displayed (31.4% of observations). Categories

⁶ The calculation is performed using as a denominator for the first fraction the predicted probability of the callback for the unemployed (12.4), and as denominator for the second fraction the predicted probability of the callback for the regular internship (11.7). The specification considered for the estimation in the one in Table 4 column (2).

were chosen in order to contain a similar number of observations, and to avoid power issues in the estimation. Hereafter, these categories will be referred to as the low, medium and high additional information treatments. The average marginal effects in Figure 4 Model 1 and Model 2 are derived from the logistic regression models available respectively in columns (1) and (2) of Table A1 of the appendix.

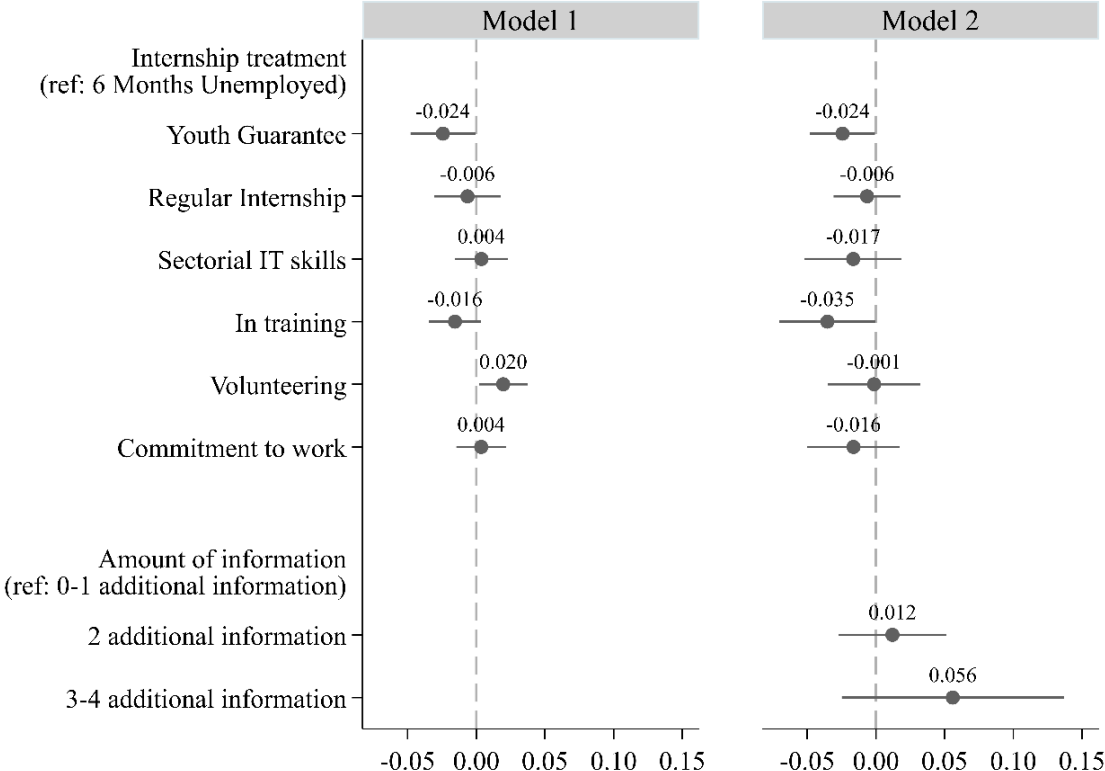


Figure 4 Average marginal effects on callback probability of internship treatment, type (Model 1) and amount of additional information (Model 2).

Note: The estimation in Model 1 refers to the specification in column (1) of Table A1. The estimation in Model 2 refers to the specification in column (2) of Table A1. Average marginal effects are calculated based on a logistic regression model, including the other treatment conditions presented in Table 2, dummies for hard and soft skills mentioned in the job post, dummies for job sector and geographic area, the number of days since the job post was published, the distance to the workplace, dummies for the resume templates, a dummy for whether a resume belongs to an incomplete batch, dummies for the platform used to send the application and for whether the application was sent after the Covid-19 lockdown on 8th March 2020. See Table A1 for the extended table with odds ratios. Bars refer to 95% confidence intervals. Robust standard errors clustered at the job post level, N=4,066.

Adding the amount of additional information in the specification in Figure 4 Model 2 does not result in any relevant change to the coefficients of the Youth Guarantee and the regular internship treatments. A high amount of additional information increases the callback probability by 5.6 percentage points, but the estimate is not precise at conventional levels. However, in Model 2 the coefficients of all of the additional information treatments shift downwards. In Figure 4 Model 1 participating in a volunteering course increased the callback probability by 2 percentage points ($p < 0.05$). All of the other additional information treatments showed a positive non-significant effect on callback probability, except where resumes specified applicants were in training. In Figure 4 Model 2 the coefficient of the volunteering treatment becomes non-significant and discrimination against resumes that mention training activities increases, that become 3.5 percentage points less likely to receive a callback ($p < 0.05$).

Overall, employers tend to discriminate against previous Youth Guarantee participants and those showing signs of being in training. However, discrimination might depend on the information available to recruiters, according to Statistical Discrimination Theory. I test this hypothesis through logistic regressions where the amount and type of additional information are interacted with the internship treatment, using the same baseline regression as per Figure 4 Model 2.

Figure 5 shows the internship treatments average marginal effect on callback rate interacted with the amount of additional information displayed on resume. The Youth Guarantee treatment decreases the callback probability by 4.5 percentage points and 4 percentage points ($p < 0.05$) when the amount of additional information is respectively low and medium. When the amount of additional information is high, then the average marginal effect converges towards zero. This evidence disconfirms hypothesis 1c, supporting Statistical Discrimination Theory.

A low level of additional information almost doubles the negative effect size of the Youth Guarantee. Participation in the program reduces the callback probability from 2.4 percentage points in the baseline model (Figure 4, Model 2) to 4.5 percentage points when a low amount of additional information is displayed (Figure 5). It is also important to understand whether the type of information matters in reducing discrimination amongst employers. Figure 6 panels (a) to (d) display the average marginal effect on callback rate of the internship treatments interacted with each of the additional information treatments. Compared to the six months unemployment treatment, the callback rate for the Youth Guarantee resumes decreases by 5.7

percentage points when sectorial IT skills are not mentioned, with $p < 0.01$ (Figure 6(a)). The absence of sectorial IT skills also slightly reduces the callback rate for the regular internship, but the average marginal effect of the interaction is not significant at conventional levels. Conversely, when sectorial IT skills are mentioned in the resume, no significant discrimination against any of the two internship treatments is detected.

Finally, the callback rate for the Youth Guarantee treatment decreases by 2.5 percentage points when participation in a training program is displayed (Figure 6(b)). However, the average marginal effect is significant only at 10% level. Being in training is the only additional information treatment that tends to reduce the callback rate rather than increase it. This is the case when it is considered alone (Figure 4), and when it is considered in interaction with the Youth Guarantee treatment (Figure 6(b)). When information about volunteering and commitment to work is missing, the callback rate for the Youth Guarantee treatment reduces respectively by 3.8 and 3.6 percentage points ($p < 0.05$).

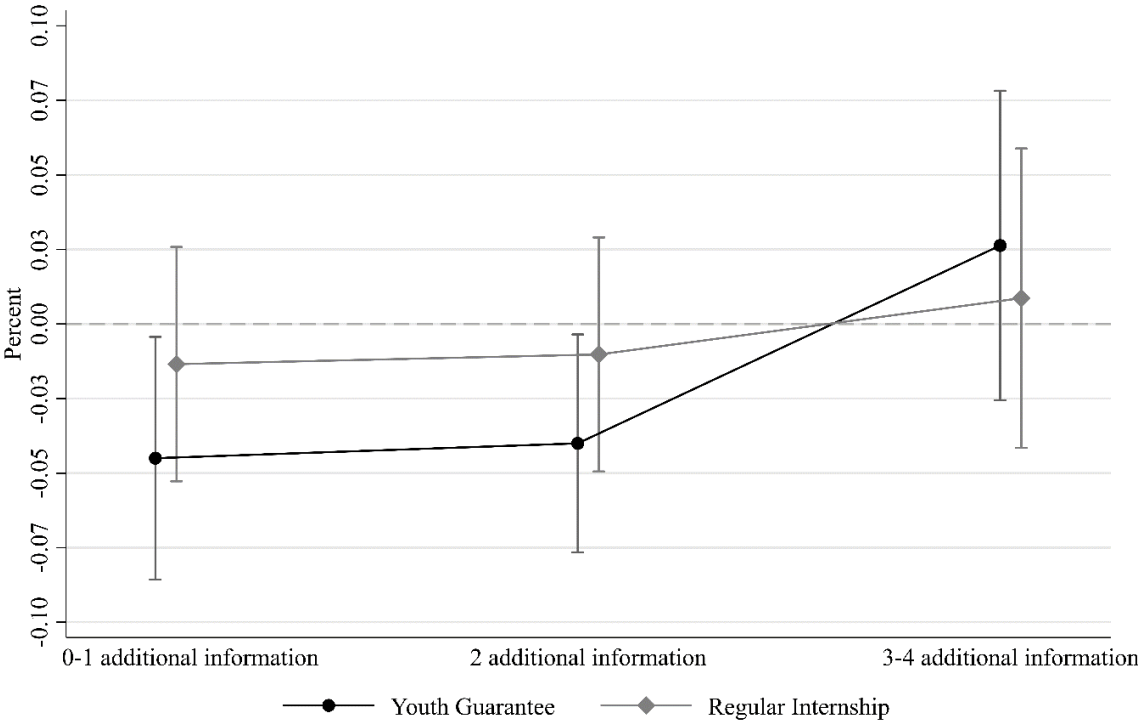


Figure 5 Average marginal effects of internship treatment on callback probability by amount of additional information included in the resume (reference category: 6 months in unemployment).

Note: Average marginal effects are calculated based on a logistic regression model including controls as per Model 2 in Figure 4, that represents the baseline model. Bars refer to 95% confidence intervals. Robust standard errors clustered at the job post level, $N = 4,066$.

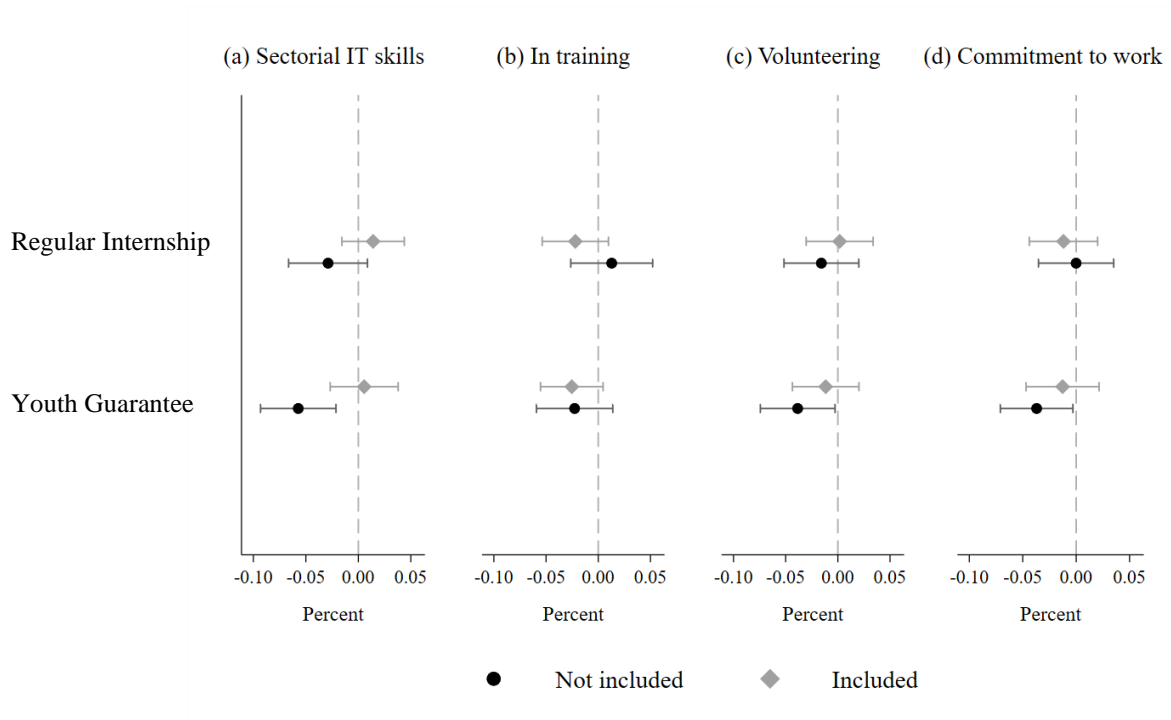


Figure 6 Average marginal effect of the internship treatment on callback probability, by type of additional information included in the resume (reference category: 6 months in unemployment).

Note: As per Figure 5.

5.3 Heterogeneous treatment effects: the Covid-19 lockdown, peer effects and job quality

The lockdown following the Covid-19 outbreak in Italy induced a sharp decrease in the volume of calls and emails received from employers. It is possible that this shock affected the distribution of the callback rate for job seekers in different internship treatment conditions. Moreover, the results obtained may be heterogeneous, depending on the diffusion of the Youth Guarantee Programme among young job seekers in the local labour market and on the quality of the job being advertised.

5.3.1 The Covid-19 lockdown

Figure 7 shows the average marginal effect of the interaction between the internship treatment and the first Covid-19 lockdown on callback rate. The callback rate for the regular internship treatment decreases by 4.4 percentage points ($p < 0.10$) after lockdown. In contrast, the average

marginal effect of the Youth Guarantee on callback rate did not change significantly after the lockdown. Thus, hypothesis 2a and 2b are not supported. The individuals most affected by discrimination after the slow down of economic activity brought about by the Covid-19 pandemic were not former Youth Guarantee participants, but those in a regular internship. However, there is a significant unbalance in the number of resumes sent before (3,162 resumes) and after the Covid-19 outbreak (904 resumes). If this were also generating an imbalance in the distribution of treatment levels over time, there would be a correlation between treatment assignment and timing of the experiment that could drive the observed results. Nonetheless, I find no statistically significant differences in the proportion of resumes assigned to the three internship treatment levels before and after Covid (table available upon request). This is in line with the fact that each resume was randomly assigned to one of the three internship treatments irrespective of the time of the application, of other resumes in the same batch and of any other external factor. Thus, the observed effect is likely to be genuinely driven by the disruption of the pandemic.

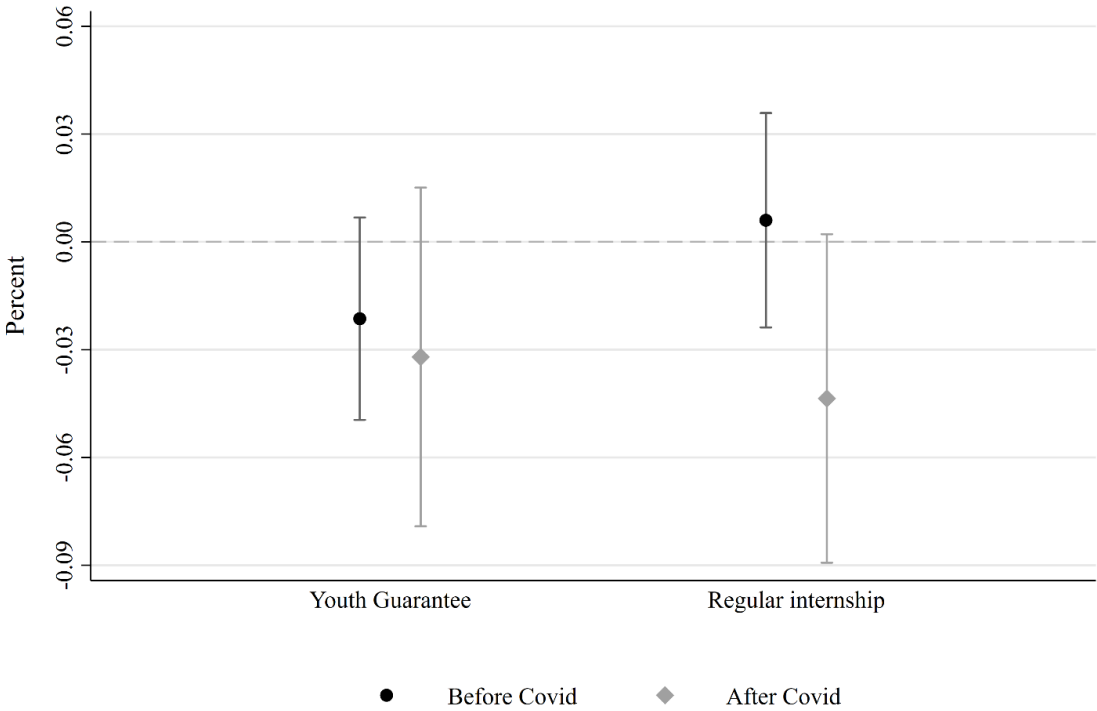


Figure 7 Average marginal effect of the internship treatment on callback probability, before and after Covid (reference category: 6 months in unemployment).

Note: As per Figure 5.

5.3.2 Diffusion effects

Figure 8(a) shows the average marginal effect of the internship treatments interacted with the Youth Guarantee diffusion dummy on callback rate. The diffusion rate dummy takes value 0 if the workplace is located in a region where the diffusion rate of the Youth Guarantee is below the regional median, and 1 if it is above it. The Youth Guarantee diffusion rate was calculated as the ratio between the number of participants in the Youth Guarantee Programme (Anpal 2020) and the total population aged 15 to 29 (Istat 2022b) at the regional level in 2019.

Resumes in the Youth Guarantee condition experience a reduction in the callback rate by 2.7 percentage points ($p < 0.05$) when applications are sent to regions where the Youth Guarantee diffusion rate is below the median (Figure 8(a)). However, when Youth Guarantee profiles are sent to areas where the diffusion rate is above the median, the callback rate still decreases by 1.6 percentage points. However, the reduction is not significant at conventional levels. This suggests the presence of a trend, as the average marginal effect is significant and only negative for the Youth Guarantee resumes that were sent in areas with low diffusion of the program. However, the interaction effect is not estimated with sufficient precision. This is unsurprising given that only 21.4% of the resumes were sent to an employer in a region with a Youth Guarantee diffusion rate above the median. Low power might undermine the possibility of accurately detecting this interaction effect. Resumes in the regular internship condition were not significantly discriminated against regardless of the degree of the Youth Guarantee's diffusion.

5.3.3 Job quality

In Figure 8(b) I plot the average marginal effect on callback rate of the interaction between the internship treatment and job quality. When applications were sent to an employer offering a long-term contract, Youth Guarantee resumes are 6.6 percentage points ($p < 0.05$) less likely to receive a callback. At the same time, the callback probability for the Youth Guarantee profiles also decreases on average by 4.7 percentage points for short-term contracts ($p < 0.1$). The callback probability for resumes in the regular internship treatment is not significantly affected by the type of contract offered. This evidence provides some support for hypotheses 4a and 4d, while disconfirming hypotheses 4b and 4c.

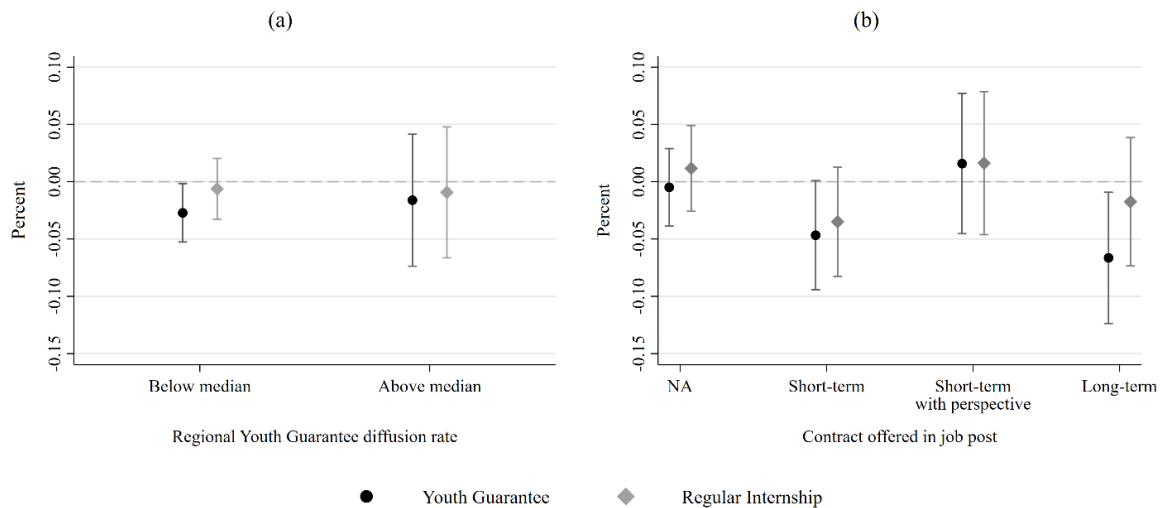


Figure 8 Average marginal effect of the internship treatment on callback probability, by regional Youth Guarantee diffusion rate (a) and contract offered in job post (b) (reference category: 6 months in unemployment).

Note: Average marginal effects are calculated based on a logistic regression model, including controls as per Model 2 in Figure 4, that represents the baseline model. The regional Youth Guarantee diffusion rate is the ratio at the regional level between the number of young people that used the Youth Guarantee by the end of 2019 and the population aged 15-29 in 2019. The variable Regional Youth Guarantee diffusion is a dummy that takes value 0 if the workplace where the resume was sent is in a region where regional Youth Guarantee diffusion rate is below the median across Italian regions, and 1 if it is above the median. Bars refer to 95% confidence intervals. Robust standard errors clustered at the job post level, N=4,066.

6 Explaining the ‘internship penalty’ and ‘programme penalty’

The results point to two unexpected findings: those that were unemployed at the beginning of their employment history (1) are more likely to be called back than Youth Guarantee internship participants, and (2) have a similar callback probability to regular internship participants. This occurs despite both types of internship participants having accumulated six more months of work experience compared to those in unemployment during the same time period. What could be driving this ‘internship penalty’?

Fictitious candidates that displayed a Youth Guarantee internship or a regular internship had a short unemployment gap followed by another period of work experience in a different firm than the one in which they undertook the internship. Non-retention in an internship could send two negative signals: (1) that the intern was not good enough to be hired (2) that the

employer who hired the intern used the internship opportunistically to hire cheap labour and get rid of the intern straight after. In the first case, the value of the work experience accumulated through the internship could be offset by a negative signal linked to the perceived ability of the candidate. In the second case, the work experience could be considered of low value for human capital accumulation since the employer did not have much of an incentive to train the intern.

There are two observations that suggest that the ‘internship penalty’ is not driven by a negative signal related to the quality of the job seeker. The first is that shortly after the internship, fictitious candidates were employed by another employer in the same occupation as the internship, demonstrating an ability to secure other opportunities in the labour market. Second, if discrimination is driven by the perceived lower quality of job seekers who have a discontinuous employment history, then fictitious candidates that worked in two non-internship jobs (Table 1 CV2) would also be discriminated against. Employers are less likely to hire opportunistically through non-internship contracts since they are much more costly than internships. Therefore, any discrimination against those with a discontinuous employment history could reasonably be attributed to a negative cue regarding the candidate’s quality. As reported in Table A1, no evidence of discrimination was found against fictitious candidates who had two non-internship jobs at different employers compared to those employed in one non-internship job at the same firm.

Overall, these observations suggest that in this experiment non-retention after the internship might be a signal of low quality work experience, rather than an indication of the candidate’s low quality. This could explain similar callback rate between resumes with a regular internship and those displaying unemployment at the beginning of the employment history. Work experience acquired through a regular internship might be interpreted by recruiters to be the result of budget-constrained firms systematically hiring different candidates through cheap short-term internships. These types of experiences are unlikely to represent a valuable opportunity for human capital development.

In addition to this, Youth Guarantee internship participants are more discriminated against compared to those in unemployment, and those in the regular internship. They experience the strongest discrimination when compared to those in unemployment. What could be driving the additional ‘programme penalty’? The Youth Guarantee internship is even more likely to be considered low-quality work experience when compared to the regular internship. Since Youth Guarantee interns are paid by the state, employers have even less of an incentive to train them and increase their human capital. Thus, participation in the Youth Guarantee internship might

strengthen the negative signal that applicants had previously been hired by an opportunistic employer. Applicants that have worked for this type of firm could be seen as a wasting time that could have been better used for upskilling through higher quality experiences. This could in turn explain why individuals that spent longer periods in unemployment but found a longer-term non-internship job were preferred to Youth Guarantee participants.

The explanation outlined above suggests that Youth Guarantee interns are discriminated against due to the reversal of the human capital accumulation signal that is attached to work experience. An alternative explanation to the ‘programme penalty’ could be related to the reversal of the achievement signal, which is a positive signal that the candidate had been hired by an employer in the past. In the latter case discrimination would be linked to prejudice against programme participants. They could be stigmatised and assigned lower status because (1) they have been part of a stigmatised group targeted by the ALMP, such as the NEET, or (2) are deemed as unable or unwilling to find a job without state support. Furthermore, the achievement signal could be reversed by programme participation because work experience through a subsidised internship might be considered less of an achievement, given the possibility that participants were hired just because they were cheap.

If prejudice against programme participants were to be the main driver of discrimination, this would remain unchanged irrespective of the quantity and type of additional information provided to employers. However, when more information is provided, discrimination against Youth Guarantee participants decreases. This confirms that the observed ‘programme penalty’ is driven by Statistical Discrimination, rather than being the result of prejudice as theorised by the Status Characteristics Theory. Discrimination disappears when sectorial IT skills are mentioned in the resume. This further supports the hypothesis that discrimination against Youth Guarantee participants arises from the reversal of the human capital accumulation signal. As soon as employers are reassured that there are job-relevant skills on the resume which signal human capital, the ‘programme penalty’ vanishes.

Ultimately, a question remains open: what would have happened if past participants in the two types of internships had been retained by the employer right after the experience? Would the ‘internship penalty’ and ‘programme penalty’ have disappeared? The one treatment that resembles this case the most is the one featuring unemployment at the beginning of the employment history followed by the first stable job. This first instance of work experience could well be the combination of an internship that led to retention in the same company through a longer-term position. In a real resume, a job seeker would pool the two jobs together and put them under the same work experience at the same company. In that case, the effect

on the callback probability would be positive, as shown in the experiment. The implication would then be that it might be better to wait longer in unemployment and find a more stable occupation or internship that leads to longer-term employment, rather than accumulating work experience through internships that do not lead to retention.

While the present study can provide some cues in the above-mentioned scenario, the experimental design is not suited to investigate what happens if a fictitious candidate that is assigned to either of the two internships (1) was retained by the same employer and (2) referenced the internship as a separate but contiguous work experience in the resume. Studying this case would have required the introduction of two additional treatment levels, one with the Youth Guarantee internship and one with the regular internship. Each of them would have also needed to be followed immediately by a job at the same firm. However, the addition of two treatment levels, for a total of five, would have required a much larger sample size in order to have the necessary power to detect the ensuing treatment effects. Moreover, as detailed in section “4.2 Treatment design”, internships that do not lead to retention represent the most common scenario in the Italian labour market (Anpal 2021). Thus, the employment histories designed for this experiment are likely to be realistic and relevant for studying employers’ discrimination against the Youth Guarantee internships. This, in turn, is important in explaining how the programme affects the future employability of those who were not retained by their employers.

7 Discussion

This work investigated whether employers use signs of past participation in ALMPs as a sorting criterion. This question becomes of particular interest, given the expansion in the last years of work-to-welfare programmes (Alber and Heisig 2011). This type of policy is likely to make programme participation more salient to employers as it becomes more tightly intertwined with work experience. I tested whether employers are less likely to call back a candidate that has taken part in a subsidised internship through the Italian Youth Guarantee Programme, compared to candidates that did a regular internship, or spent the same period of time in unemployment. The results show that past participation in the Youth Guarantee reduces the callback probability by 19.3%, compared to unemployment and by 15.4% compared to the regular internship.

Surprisingly, candidates that participated in a regular internship do not experience a significantly higher callback than those that were unemployed, even though they were employed for six more months. This finding is similar to Di Stasio's (2014) research on internships in Italy, and Cahuc and Hervein's (2020) work on apprenticeships in France. This finding seems to be more closely related to recruiters assigning little value to work experience that is accumulated through internships and does not lead to retention.

Employers might believe that firms that do not retain interns are more likely to be using internships systematically to hire cheap labour, with little investment in their human capital. Following this line of reasoning, the Youth Guarantee internships might send an even more negative signal, as employers might have even less incentive to train their interns if they are not paying them themselves. However, this negative signal disappears as soon as employers are provided with additional information, and in particular job-relevant IT skills, thus supporting Statistical Discrimination Theory. The importance attributed by employers to hard skills that have already been consolidated through work experience is further confirmed by the lower callback rate of resumes that showed existing involvement in training activities. This finding replicates that of Falk et al. (2005), and suggests that employers interpret training as a sign of lack of proficiency in job-relevant skills.

These results are heterogeneous to a number of factors. The economic downturn brought about by the Covid-19 induced lockdown also generated more discrimination towards regular internship participants. This contrasts with what was predicted by Queuing theory (Reskin and Roos 1990; Model and Ladipo 1996; Waldinger 1996; Reskin 2019). As opposed to worsening the condition of those that were already discriminated against before the pandemic, the downturn also dragged down the callback rate for those that were not discriminated against before the pandemic began. This reveals employers' implicit ranking of workers in the labour queue based on how they started their employment history. They consider top candidates to be individuals who remained unemployed until they found a long-term job. Second-choice candidates began employment with a precarious work arrangement that did not lead to retention. Third-choice candidates began their employment with a subsidised precarious work arrangement that did not lead to retention. In normal times, employers would call both top and second choice candidates at the same rate for an interview. As soon as vacancies decreased and competition for jobs became tougher, they started to discriminate against both second and third choice candidates. Moreover, the generally low callback rate, while in line with that found in Italian studies targeting similar occupations (Patacchini et al. 2015), highlights the difficulty of finding a job in the Italian labour market. This evidence disproves popular beliefs

that the youth are unemployed because they do not look for a job hard enough or tend to be “choosy”. The drastic drop in callback rate after the Covid-19 lockdown further shows the power of external economic conditions – net of job seekers effort - as determinants of individual labour market outcomes.

The findings are not precise enough to confirm increased discrimination against Youth Guarantee participants in areas where diffusion of the programme is lower, despite the presence of a trend. Finally, there is suggestive evidence that Youth Guarantee participants received less calls from employers offering long term jobs than those in unemployment and those in regular internships. At the same time, there is no evidence that Youth Guarantee participants received more calls for short term jobs than resumes in other treatment conditions.

There are a number of limitations to the present study. First, the experimental design does not provide a comparison of the existing treatments with fictitious candidates that (1) were retained after the internship, or (2) found a standard (non-internship) job right after university. While the fictitious resumes were designed to be representative of young job seekers in the Italian labor market, still they do not cover profiles that might be more common in other countries. However, studying these cases would have required additional treatment levels, that demand a much larger sample size to detect treatment effects. Thus, further research should compare former ALMPs participants with other job seeker profiles not included in this work. The second limitation is that estimates of the effect of the Youth Guarantee internship tend to be noisy due to a considerable amount of variation in labour market conditions during the period of the field experiment. The shock brought about by the Covid-19 pandemic generated considerable uncertainty in the labour market, as was demonstrated by the sharp reduction in callback rates after the first lockdown in Italy, in March 2020. Size effects were also smaller than those expected from the power calculation, thus indicating that the analysis is likely to be underpowered. This limitation is particularly problematic when studying interaction effects, that are estimated with a considerable degree of uncertainty. Moreover, while placing the main treatment of interest at the beginning of the employment history was necessary for the design of the study, this tends to produce conservative estimates. The third limitation is that the results are limited to white collar occupations. The job posts covered did not include low-skilled manual occupations, such as those targeted in previous studies (Liechti et al. 2017; Van Belle et al. 2019; Cahuc and Hervein 2020; Hervein and Villedieu 2022) where participation in ALMPs might be more widespread and less stigmatising. This choice was driven by the larger volumes and standardisation of job posts available in white collar occupations (Patacchini et al. 2015) and was necessary in order to achieve an acceptable sample size. Given the

occupational sectors targeted, all of the resumes were assigned with a bachelor's degree to maximise callback probability and minimise power issues. Therefore, it was not possible to vary the quality of resume in terms of educational credentials, despite the fact that previous research has pointed to their relevance in determining the employers' perception (Di Stasio 2014; Bills et al. 2017; Liechti et al. 2017). Thus, the experimental design does not investigate whether the stigmatizing effect of the Youth Guarantee internship would persist even for youth with a different level of education. Employers might be more lenient with low-skilled youth using the Youth Guarantee, while penalizing high skilled ones. The latter might be less expected to resort to activation programs compared to the former, given their higher level of education. Therefore, program participation might represent a particularly negative signal of low competence and motivation for high skilled youth. At the same time data from Anpal (2019) on a sample of NEET that subscribed with employment centres shows that only 16.7% of Youth Guarantee participants have not completed high school (against 34% of non-participant NEETs). Thus, program participation could be stigmatizing also for low-skilled youth, as it is relatively uncommon among this category. Overall, it is not clear what the effect of the Youth Guarantee internship would be for low-skilled youth. Further research is needed to better understand the interplay between program stigmatization and educational level. Despite this limitation, the experiment is still relevant for the study of activation policies. While only a minority of NEET are university educated, they have a higher probability of participating in the Youth Guarantee program. Among Youth Guarantee participants 23.5% are university educated, against 13.0% of non-participant NEETs (Anpal 2019). This is unsurprising given the documented presence of Matthew effects in the access to public policies (Cantillon 2011; Cantillon and Van Lancker 2013; Bonoli, Cantillon and Van Lancker 2017), pointing to a positive relationship between individual socio-economic status and program participation. Thus, the experimental results are still relevant – despite their limitation to high skilled youth. Fourth, the present study only focuses on employers' screening behaviour, thus it does not provide any direct insight into employers' perception of the Youth Guarantee internship program. Future research based on qualitative interviews and surveys is necessary to complement the evidence here reported and draw a complete picture of the mechanisms at work.

Finally, the study is only conducted in Italy and considers a specific ALMP, namely subsidised internships, which may limit the ability to generalise the findings. The observed 'programme penalty' and 'internship penalty' are likely to be related to the 'revolving internships' phenomenon, that is when employers systematically hire interns without retaining

them (ILO 2012; Eurofound 2017; Stewart 2021). However, this phenomenon is tightly linked to Italy's sluggish economic growth. Employers tend to be concentrated in low value-added sectors and pursue a strategy of compression of labour costs, which pushes them to behave opportunistically (Kazepov and Ranci 2017). Thus, the observed discrimination against former subsidized internship participants could be generalisable to other countries that also have a stagnating economy, similar to the Italian one. In this context firms have an incentive to misuse ALMPs in the form of subsidised internships, thus negatively affecting the reputation of the programme and ultimately leading to the observed 'programme penalty'. This is consistent with previous research suggesting that ALMPs tend to work well only in countries with a healthy economy that creates jobs (Martin 2015; Kazepov and Ranci 2017). However, further research is needed to assess how employers respond to ALMPs in other countries, extending the analysis beyond the one-country and one-program analysis carried out in this study.

8 Concluding remarks

This work contributes to the existing literature on ALMPs showing that subsidized internships that do not lead to retention and skills acquisition have scarring effects in the long-term. This is very relevant for subsidized internship programmes such as the Youth Guarantee, that tends to push the youth into participating in internships that in most cases do not result in a job offer (Anpal 2020; Anpal 2021). Meanwhile, this experiment only focuses on the demand side of labor and considers a limited set of fictitious biographies, thus it is not suitable to draw definitive conclusions on the real-world effectiveness of subsidized internship programs. This analysis should be complemented considering supply-side factors such as the characteristics of participants and the heterogeneity in the implementation of the Youth Guarantee across Italian regions. Despite this limitation, this study provides novel evidence on stigmatization as a potential threat to program effectiveness. However, this effect is likely to be generated by the negative reputation of the program as a source of inadequate training, rather than being driven by welfare stigma in the form of prejudice against programme participants. Therefore, it is important to move towards designing programs for the youth that promote actual skills development (Card et al. 2018) and improve the matching between employers and employees (Baert, Cockx and Verhaest 2013). To this end, policy makers should limit the use of subsidized internships only to employers that can guarantee the provision of adequate training. However, it can be difficult in practice to check the quality of such training (Eurofound 2017; Stewart et

al. 2021). Moreover, results show that former Youth Guarantee interns that display also sectorial IT skills in their resume receive as many calls as those that waited longer in unemployment *at best*. Thus, an alternative proposal would be to invest in ALMPs that provide intensive job search assistance and monetary support for the unemployed youth until they find the right job, even if this means spending more time in unemployment.

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Appendix

Table A1 Logistic regression of total callback on internship treatment full controls specification showed, odds ratios displayed.

	(1)	(2)
Internship treatment (reference= 6 months unemployed)		
Youth Guarantee	0.775* (0.097)	0.774* (0.097)
Regular Internship	0.939 (0.114)	0.938 (0.114)
Sectorial IT skills	1.039 (0.106)	0.842 (0.157)
In training	0.852 (0.085)	0.693* (0.128)
Volunteering	1.225* (0.114)	0.986 (0.175)
Commitment to work	1.037 (0.099)	0.844 (0.149)
Two work experiences (reference=one work experience)	1.074 (0.112)	1.079 (0.113)
Amount of information (reference=0-1 additional information)		
2 additional information		1.149 (0.272)
3-4 additional information		1.741 (0.698)
Job sector (reference=Accounting and secretary)		
Human Resources	0.937 (0.359)	0.943 (0.361)
Marketing and Social Media	0.978 (0.192)	0.977 (0.192)
Online platform of application (reference=Bakeca.it)		
Indeed.com	0.560* (0.154)	0.555* (0.153)
Other	0.217 (0.229)	0.213 (0.224)
Days since job post publication	0.882 (0.085)	0.880 (0.085)
Distance from city centre	1.006 (0.012)	1.007 (0.012)
Geographic area (reference=Southern city)		
Centre city	0.720 (0.166)	0.726 (0.167)
Nothern city	0.741 (0.156)	0.743 (0.157)
Resume template (reference=Template 1)		
Template 2	0.917 (0.103)	0.915 (0.103)
Template 3	0.977 (0.106)	0.981 (0.106)
Template 4	0.998 (0.107)	0.994 (0.107)
Incomplete batch	0.610 (0.276)	0.616 (0.281)

Table A1 Continued

After Covid	0.507 (0.292)	0.503 (0.289)
Work experience mentioned in job post	1.400 (0.326)	1.408 (0.328)
English mentioned in job post	1.086 (0.190)	1.086 (0.190)
Education in job post (reference category=Not mentioned)		
Bachelor's degree not required	1.315 (0.256)	1.325 (0.258)
Bachelor's degree preferred or required	1.631* (0.348)	1.633* (0.349)
Master's degree required	0.613 (0.513)	0.628 (0.526)
Advanced skills in job post	1.393 (0.262)	1.396 (0.263)
IT skills in job post	1.026 (0.175)	1.023 (0.174)
Learning attitude in job post	1.248 (0.261)	1.252 (0.263)
Teamwork orientation in job post	1.035 (0.209)	1.033 (0.209)
Commitment to work in job post	1.082 (0.221)	1.082 (0.222)
Constant	0.160* (0.119)	0.193* (0.145)
Other treatment conditions controls	Yes	Yes
Month fixed effects	Yes	Yes
Observations	4,066	4,066
Pseudo R-square	0.050	0.051

* $p < 0.05$ (two-tailed tests).

Note: Other treatment conditions are those showed in Table 2. Tables including coefficients for the other treatment conditions available upon request. Robust standard error in parentheses clustered at the job post level. The variable “Two work experiences” takes value 0 if the fictitious candidate was randomised to have an employment history with one non-internship work experience at the same employer, and 1 if two non-internship work experiences at different employers are drawn. The total number of months in employment in both treatments is the same. For more details, see Table 1 and 2 for the structure of the employment history in resumes.

Figure A1 Example of resume for accounting positions, with placeholders for the internship treatment (in red), the unemployment treatment (in green), work experience treatments (in blue) and the additional information treatments (in orange). (Page 1).

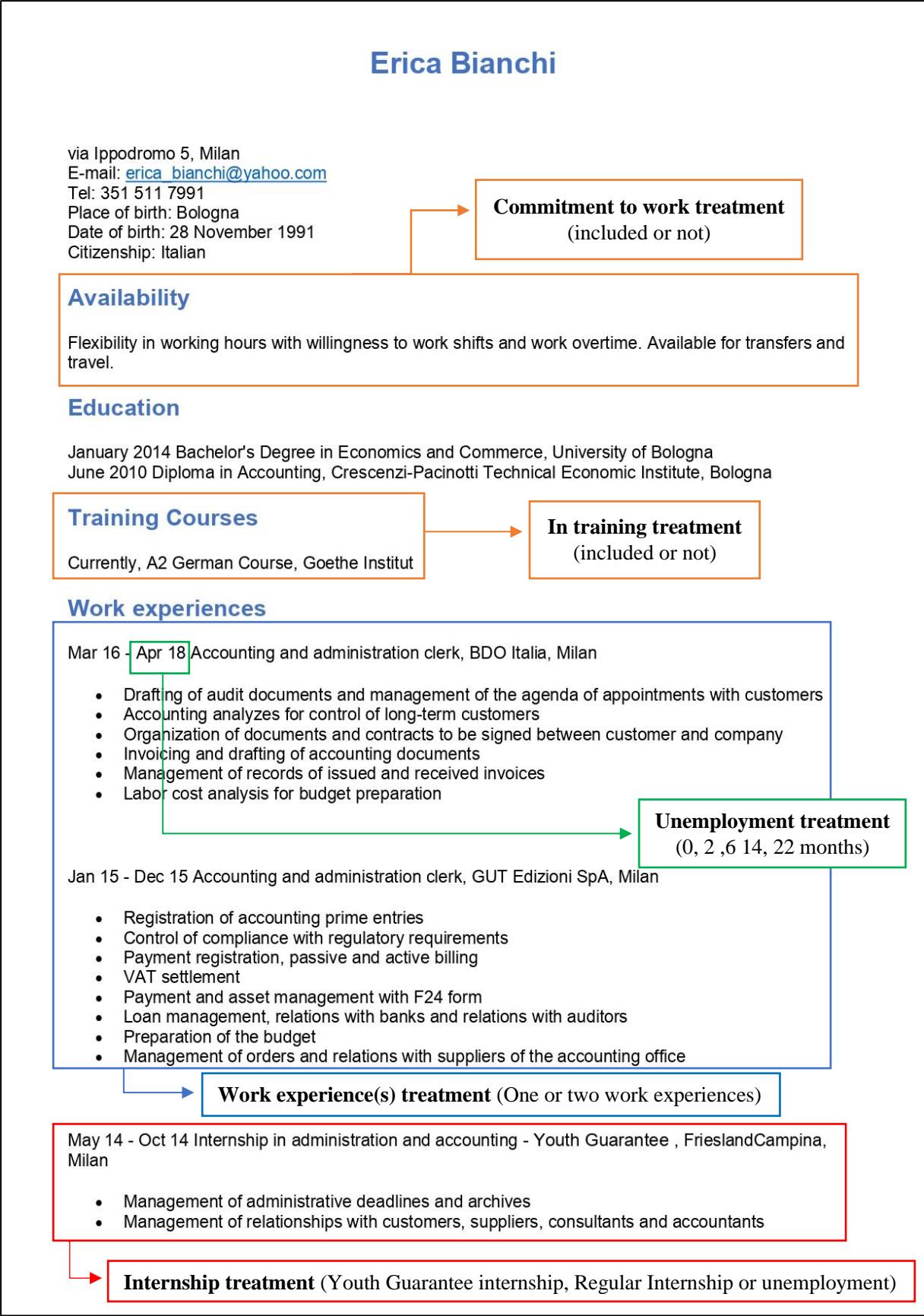
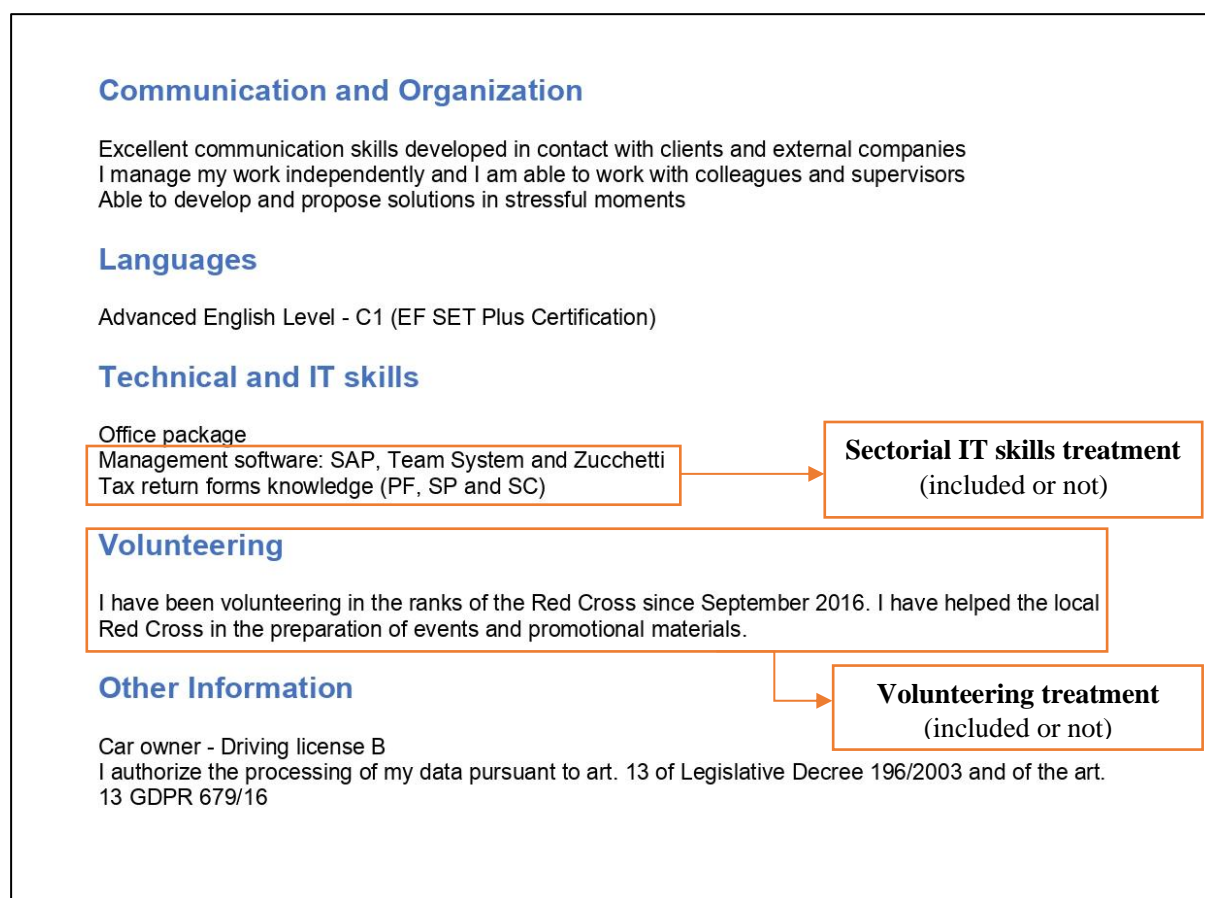


Figure A1 (continued, page 2).



Note: The resume represents the case of a fictitious candidate that was randomly drawn to participate in the Youth Guarantee internship as their first work experience, had two work experiences afterwards and had been unemployed for 22 months. The resume is also randomised to show sectorial IT skills, ongoing participation in training activities, volunteering, and an initial statement signaling commitment to work. In resumes that featured the regular internship, the treatment is similar to the one in the red square, but does not contain the “Youth Guarantee” text. When the treatment is randomised to show a six-month unemployment gap instead of an internship, the whole text in the red square is removed. The original text of the resumes was in Italian, the figure presents an English translation.

Figure A2 Example of cover letter for accounting positions (in original in Italian, hereby translated in English)

Dear Personnel Manager,

I am writing to you in reference to the online job post on [name of website where the job was posted] for the position of [position name as per job post] at [company name].

In addition to my strong interest in the role of [position name as per job post], and in general in operational accounting activities, I would bring to [company name] my many years of experience in this area of work. In fact, I have carried out activities related to invoicing and payment processing, drafting of financial statements, as well as management of internal/external communications. Therefore I believe that my profile is in line with your needs and can give you the administrative/accounting support you are looking for.

I am attaching my curriculum vitae to the present letter waiting for your kind reply.

Thank you for your consideration and for your time.

Sincerely,

[Name and surname of fictitious candidate]

