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How Does Recruiting Through IEFP Affect Firm Performance? An Empirical Analysis Using Portuguese Firm-Level Data.

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Abstract:

This dissertation examines the firm-level consequences of recruiting through Portugal's public employment service, IEFP, addressing two related questions. The first asks whether firms that use IEFP as a recruitment channel perform differently from those that do not, and whether hiring individuals whose qualifications exceed those required for their position (overqualified workers) measurable consequences for firm performance. To answer this questions, we draw on a sample of medium and large Portuguese firms, and employ a difference-in-differences and event-study design. The results suggest that recruiting through IEFP is, overall, associated with persistent employment growth and gradual improvements in labor productivity and wages, consistent with labor market intermediation facilitating employment, and positive effects on sales and net profit per worker. These gains are, however, accompanied by a deterioration in asset efficiency and other profitability measures, such as operational result and EBITDA per worker, raising concerns about whether recruiting via IEFP yields higher costs than benefits. The heterogeneity analysis shows that larger firms benefit the most from this recruitment channel, exhibiting a delayed but broad-based positive trajectory across most outcomes, while smaller firms show a consistent rise in employment accompanied by detrimental effects on most profitability measures. Hiring overqualified workers shows no evidence of heterogeneous effects between treated and control firms, with only marginal associations found for employment, sales, net profit, and export volume.

JEL Codes: J24, J68, L25, J21

Keywords: public employment services, overeducation, Portuguese labor market, firm economic performance

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1. Introduction

Public employment services play an important role as labor market intermediaries. In Portugal, this role is carried out by the *Instituto de Emprego e Formação Profissional* (IEFP), founded in 1930 and operating under the supervision of the Ministry of Labor. IEFP's main goal is to reduce unemployment duration and speed up labor market reintegration for job seekers through a variety of interventions such as vocational guidance, professional training and rehabilitation, the promotion of school and professional, and the promotion of job creation and retention, facilitating employer-worker matching (Instituto do Emprego e Formação Profissional [IEFP], s.d.). The relevance of IEFP as a labor market intermediary is underscored by the scale of unemployment over the period covered by this study. For example, in 2022, the total unemployed population stood at approximately 323,000 individuals (Instituto Nacional de Estatística [INE], 2026), of whom 307,000 were registered with IEFP employment centers (PORDATA, 2026).

IEFP, is not only an important intermediary for individual, as firms also choose to recruit through this institution. Firms do not rely on a single recruitment channel. The channel chosen, and the type of workers sought through it depend on labor market conditions. Russo et al. (2001) show that the use of public employment services and the hiring of unemployed candidates are more likely to occur in a slack labor market, while advertisements and the hiring of already employed job seekers is more common when labor demand is high. The choice of recruitment channel reflects a comparison of expected costs and benefits, as more recruitment effot yields better candidates (Russo et al., 2001). Within this cost-benefit logic, educational requirements play a distinctive role: they are found to negatively affect both the likelihood of using informal recruitment methods and the public employment service relative to advertisements, and the likelihood of hiring an unemployed candidate relative to hiring school leavers or already-employed applicants (Russo et al., 2001). Given IEFP's role as a public employment service, and the rich records on both applicants' qualifications and the vacancies requirements, it offers a relevant setting to examine the following question: how recruiting recruiting through IEFP affects firms' performance, measured by indicators such as value added, sales, wages, number of employees, assets and profit rubrics.

The effect of workers hired through IEFP on firm performance can depend, in part, on the quality of the match between worker and job, particularly on the difference between the skills possessed by the worker and those required by the job position. IEFP's registration records

make this gap directly observable, since they require information about the formal qualifications held by the worker and those demanded by the job offer, allowing the mismatch to be identified. This is particularly relevant given the trends observed in the Portuguese labor market. Around 50% of private-sector workers have experienced a job mismatch at the moment of hire, with, on average, overeducation accounting for 30% of cases and undereducation for 23.4% (Araújo & Carneiro, 2023). This risk of mismatch is amplified by the rapid expansion of the highly educated workforce: the number of higher education graduates rose from 60,091 in 2001/02 to 95,608 in 2022/23 (Direção-Geral de Estatísticas da Educação e Ciência [DGEEC], 2026), increasing the pool of workers who may be placed in jobs below their qualification level. Given this context, and leveraging IEFP's ability to identify mismatch at hiring, we propose a second question: whether hiring overeducated workers through IEFP has distinct effects on firm performance.

Despite the extensive research on public employment services, important gaps remain. The existing PES literature is predominantly worker-centered, focusing on the outcomes of workers after reemployment through one of the PES's interventions, while the firm-level perspective has received less attention. Additionally, most studies focus on a single type of intervention, instead of assessing the performance of these institutions as a whole (Card et al., 2010, 2018). The few firm-level studies that exist concentrate on specific national programs and a narrow set of outcomes, with evidence from Southern European countries, and Portugal in particular, remaining scarce. Similarly, overqualification research tends to focus on the effects of educational mismatch on individual wages and job satisfaction, while the consequences for the firms that hire overqualified workers are less explored. Finally, to our knowledge, no study has examined firm-level outcomes of overqualified hiring through a public employment service, given that PES registrants may systematically differ from overqualified workers hired through private channels.

Therefore, this dissertation intends to contribute to the literature on the effects of public employment services and overqualification by combining administrative firm-level data from IEFP and the Central Balance Sheet covering the period 2013 to 2022. The empirical strategy relies on a difference-in-differences specification and an event-study design that allows the treatment effects to vary across periods, capturing the dynamic evolution of firm outcomes over time.

The main fidings of this dissertation are as follows. Overall, recruiting through IEFP is, associated with persistent employment growth and gradual improvements in labor productivity and wages, sales and net profit per worker. However, is also accompanied by the deterioration of asset efficiency and other profitability measures such as operational result and EBITDA per worker, raising questions about whether the financial cost associated with recruiting via IEFP. Larger firms seem to benefit the most from hiring through these services, exhibiting a delayed but broad positive trajectory for most outcomes, while smaller firms exhibit a consistent increase in the number of employees with detrimental effects for most profit measures. Hiring overqualified workers has no heterogenous effects between treated and control firms, only showing some marginal effects on employment, sales, net profit and export volume.

The remainder of this dissertation is organized as follows. Section 2 reviews the literature on public employment services and overqualification, situating this study within the existing evidence. Section 3 describes the data sources, sample construction, and the empirical methodology. Section 4 presents and discusses the main results as well as the heterogeneity analysis by sector and firm size. Section 5 concludes with a summary of the main findings, their policy implications, and direction for future research.

2. Literature Review

2.1. Public employment services and match quality

Firms typically have access to several recruitment channels, and the choice among them is shaped by labour market conditions rather than fixed firm preferences. Russo et al. (2001) find that employers shift toward the public employment services when the regional labour market is slack, and toward advertisements and the hiring of already-employed job seekers when labour demand is high, reflecting cyclical fluctuations in the expected composition of the pool of potential applicants. This suggests that public employment services are not simply one channel among many, but one whose use is tied to specific labour market conditions and to a specific segment of job seekers.

Given their intermediary role, employment services can influence a company's recruitment strategy, affecting employer-employee match quality and, subsequently, firms' performance. They may accelerate the matching process, but it can come at the cost: vacancies filled by less suitable workers (Blasco & Pertold-Gebicka, 2013). Public employment services encompass several types of interventions, each with different implications for firms. Job search assistance and counselling programs help unemployed individuals improve their search skills, directing them towards offers that better suit their qualifications, which simultaneously lower firms' recruitment costs (Blasco & Pertold-Gebicka, 2013; Lechner et al., 2013). Subsidized employment reduces the wage costs for companies, which can generate a competitive advantage for covered firms (Lechner et al., 2013), and incentivizes companies to recruit from the pool of unemployed workers by offsetting the negative signal of unemployed status, since firms prefer to hire employed candidates expecting them to be more productive (Eriksson & Lagerström, 2006). Monitoring programs sustain job search effort and raise the number of applications sent, which can loosen market tightness by encouraging firms to open more vacancies as search and recruitment costs fall (Blasco & Pertold-Gebicka, 2013; Gautier et al., 2018; Graversen & van Ours, 2008; van Ours, 2007). Broadly speaking, most of the literature analyzed finds that ALMPs improve the matching process (Black et al., 2003; Blasco & Pertold-Gebicka, 2013), though their effects on match quality remain less clear for four main reasons.

The first is the threat effect, when unemployed individuals intensify their job search to avoid mandatory PES program participation, leading to a quicker acceptance of job offers (Black

et al., 2003; Rosholm & Svarer, 2008). The second, the compulsion effect, works through intensive monitoring and sanctioning (for example, reduction of unemployment benefits), which lowers the utility of remaining unemployed, urging workers to accept employment without adequate consideration of occupational fit (Blasco & Pertold-Gebicka, 2013; Graversen & van Ours, 2008; van Ours, 2007; van Ours & Vodopivec, 2008). Sanctioned individuals tended to move to lower-paying, lower-skilled occupations and less stable jobs resulting in human capital losses, detrimental for future job matches (Arni et al., 2013; Black et al., 2013; van den Berg & Vifaström, 2014). van Ours (2007), by contrast, observes that the negative impacts of the compulsion effect were offset by a more efficient job search and increased attractiveness of the unemployed to employers. van Ours and Vodopivec (2008) find no impact of benefits reduction on the quality or duration of the first job after exit from unemployment in Slovenia. Third, the lock-in effect emerges when mandatory training or long subsidized employment programs prevent participants from accepting an offer while reducing their search effort, shrinking the pool of available workers and potentially leaving the most suitable candidates unavailable at the moment of recruitment (Graversen & van Ours, 2008; Lechner et al., 2013; van Ours, 2004). The displacement effect emerges when companies overlook applicants who are not covered by the ALMPs program, foregoing potentially better-quality matches (Crépon et al., 2013; Gautier et al., 2018; Graversen & van Ours, 2008). Finally, the population covered by PES is generally composed of lower-skilled workers expected to produce lower-quality matches from the start (Blasco & Pertold-Gebicka, 2013; Eriksson & Lagerström, 2006).

Empirically, Blasco and Pertold-Gebicka (2013) found that the effects of counselling and monitoring differed by firm size. For small firms, these measures increased the hiring of unemployed workers but also led to higher workforce turnover, resulting in no impact in firm performance measured by total factor productivity and value added. By contrast, large firms did not change their recruitment strategies. Using German data, Lechner et al. (2013) find that companies were generally negatively affected by subsidized employment and long training programs. While subsidized employment could, in principle, generate a competitive advantage through savings in wage costs, it proved detrimental in the long run, as treated firms were more likely to contract in size, rely more heavily on temporary workers, and ultimately exit the market (Lechner et al., 2013). Lombardi et al. (2018), using Swedish administrative data on small and medium firms, found that wage subsidies encourage firms

to hire workers who would otherwise struggle to find regular employment. A positive impact was observed for profits and value added when a caseworker first approved the subsidy, as they screened out firms likely to displace existing employees or perform poorly, mitigating the displacement effect associated with PES interventions. The two regimes yielded different benefits: caseworker screening improved the performance of surviving firms, while the system with a reduced presence of caseworkers reduced firms' closure and thus increased employment. Therefore, how PES performs its role as an intermediary has to take into account what type of results are prioritized (Lombardi et al., 2018). Depalo and Viviano (2024) find that hiring subsidies improved firms' growth, as they stimulate permanent hires, but resulting in a decrease in the capital-labor ratio and value added per worker. Cheaper labor induced firms to decrease their capital-to-labor ratio, with detrimental effects on productivity (Depalo & Viviano, 2024). The empirical evidence on the consequences of ALMPs on firm performance are mixed and limited for three main reasons: first, the majority of the programs are short, which makes it difficult to determine the impact on economic metrics, as these variables tend to be path dependent and because human capital needs time to develop; second, match quality can only be measured after accepting the job offer, creating a selection problem; and third, the negative and positive effects of each intervention can compensate for each other (Card et al., 2018; Crook et al., 2011; Lechner et al., 2013).

2.2. Qualifications and firm performance

Human capital is one of the key strategic resources of firms and is widely regarded as a source of competitive advantage (Crook et al., 2011; Hatch & Dyer, 2004; Pantea, 2019). According to human capital theory, qualifications are a stronger predictor of firm performance, which suggests that those who possess additional qualifications will tend to perform better (van Dijk et al., 2020). A high level of qualifications is also essential to create knowledge. Firms rely on two sources of knowledge: general, tied to academic qualifications and cognitive capabilities; and tacit knowledge, which is firm-specific, created internally, and difficult to imitate and transfer, making it essential to sustain competitive advantage (Crook et al., 2011; Hatch & Dyer, 2004; Pantea, 2019). However, developing tacit knowledge requires individuals to possess a broad base of general skills (Hatch & Dyer, 2004). Deming and Kahn (2018) corroborate this, showing that differences in the level and type of skills demanded by employers are positively correlated with firm performance, and that firms increasingly value workers with complementary cognitive and social skills. Resource-Based

Theory (RBT) complements this argument by explaining why highly qualified employees can generate a sustainable advantage, but only if they are retained long enough (Crook et al., 2011). Firms skilled at hiring, developing, and deploying human capital gain learning and cost advantages, though this requires a high initial investment during which productivity starts low before rising as workers gain experience (Hatch & Dyer, 2004; Hitt et al., 2001).

At the moment of hiring, the level of education is often the only observable proxy recruiters have for these underlying skills and capabilities (Hatch & Dyer, 2004; Hitt et al., 2001). Hamburg and van der Velden (2015) also note that displaying high credentials during the selection process signals greater human capital and, consequently, lower expected training costs. This creates an incentive for firms to hire overqualified workers. Overqualification occurs when a worker's education or skills surpass what the position they were hired for demands (Chevalier, 2003; Green & Zhu, 2010)[1]. Overqualification can be considered an educational mismatch (Chevalier, 2003) or a type of underemployment, as employees' skills are not being exploited to their full potential (Maynard et al., 2006). Overqualified workers are regarded as possessing a broader base of general knowledge, technical and social skills, which enhances their ability to learn and solve problems. This implies that firms may prefer to hire overqualified workers in the expectation that they will adapt more quickly, absorb and apply firm-specific knowledge more efficiently, which in turn decreases the training costs (Lee et al., 2021; Leiponen, 2000). This preference is borne out empirically by Verhaest et al. (2018), who find that employers tend to favor candidates with the highest level of education, with master's graduates receiving substantially more interviews than bachelor's graduates for the same job positions.

However, firms often have to pay higher initial wages to attract overqualified workers (Crook et al., 2011; Hitt et al., 2001; Kampelmann et al., 2020), and their higher propensity to leave (Erdogan et al., 2011; Maynard et al., 2006; McGuinness, 2006), which raises recruitment and training costs (Pantea, 2019), may prevent firms from ever reaching the value-generating phase where the initial investment is recuperated (Crook et al., 2011; Hitt et al., 2001). Firms

[1] Overeducation may not necessarily indicate overqualification or overskilling (Allen & van der Velden, 2001; Chevalier, 2003; Green & McIntosh, 2007; Green & Zhu, 2010). However, because our analysis focuses on firm performance and our educational mismatch variables do not allow us to separate the two concepts, they will be used interchangeably.

hiring overqualified workers may also underperform relative to rivals if these workers' qualifications are underutilized (Hatch & Dyer, 2004). Additionally, paying overqualified workers more can raise feelings of unfairness among well-matched colleagues performing the same tasks for less, reducing their effort and productivity, resulting in a double loss for firms Grunau (2016). Job dissatisfaction is an underlying channel through which overqualification can harm firm performance. Overqualified employees may feel they are not fully using their knowledge, leading to dissatisfaction, absenteeism, and higher turnover intentions (Erdogan et al., 2011; McGuinness, 2006; Verhaest & Omey, 2009; Song et al., 2026), a pattern described as relative deprivation (Feldman et al., 2002; Lee et al., 2021; Liu et al., 2015; Malik et al., 2019), which in turn reduces productivity and profitability (Kampelmann et al., 2020), and that may make employers reluctant to hire overqualified individuals (Büchel, 2002).

Kampelmann and Rycx (2012) is one of the first analyses on the relationship between productivity and educational mismatch using value added per worker rather than wages as a proxy for workers' productivity. Their results show that hiring overqualified individuals rather than workers with the required level of qualification yields larger increments in productivity. Kampelmann et al. (2020) find that an additional year of overqualification increases productivity by 2.3% and profitability by 5.1% in the following year, with required education showing effects of similar magnitude. Mahy et al. (2015) report that a one-year increase in a firm's mean overeducation significantly improves firm productivity by 2.3% on average. For example, Moretti (2004) observed that plants that had strengthened their workforce with more college graduates experienced higher productivity, but that these gains were offset by rises in labor costs. In contrast, Grunau (2016), using German employer-employee data, found the effect of overeducation to be negative but statistically non-significant. McGowan and Andrews (2015) also documented a non-statistically significant relationship between formal overeducation and productivity, while overskilling has a significant and positive effect. Rocha et al. (2025), using linked employer-employee data, find that overeducation has a limited but positive effect, with a one-year increase raising productivity by 1,3%. But reallocating overqualified workers to suitable occupations yields a 0.4% average productivity gain, while raising the level of qualification of the undereducated to the required level increases productivity, on average, by 1% (Rocha et al., 2025). Undereducation has consistent detrimental effects on firm performance indicators (Grunau,

2016; Kampelmann et al., 2020; Rocha et al., 2025). Coraggio et al. (2025) document a positive association between job-worker allocation quality and sales per worker, while no significant effect is observed on return on assets. Exporting firms tend to employ a higher share of skilled workers than non-exporters (Brambilla et al., 2018), as entering export markets requires firms to restructure, adopt newer technologies, and reorganize their workforce to improve productivity and efficiency (Brambilla et al., 2018; Caliendo & Rossi-Hansberg, 2012). Overqualified workers are potentially better suited for this environment, given their greater adaptability and innovation capacity, and because more productive exporting firms are able to offer superior jobs, becoming more attractive to these workers (Grunau, 2016, Rocha et al., 2025). As firm-specific knowledge is accumulated, the initial degree of mismatch narrows, and turnover intentions decline (Coraggio et al., 2025; Kampelmann et al., 2020). Büchel (2002) also noted that workers who experience overqualification tend to be more involved in on-the-job training and have longer tenure years, showing contradictory results on the relationship between the two.

The relationship between qualifications and performance may be heterogeneous across industries. In high-tech and knowledge-intensive industries, firms have a stronger incentive to hire highly educated workers, who tend to be more productive (Erdogan et al., 2011; Vermeylen & Waroquier, 2021), better at solving complex problems (Wu, 2015), and more innovative (McGuirk et al., 2015), since profits in innovative firms are more closely tied to educational competencies than in non-innovative ones (Leiponen, 2000 & Leiponen, 2005). Vermeylen and Waroquier (2021) observe that firms, especially in high-tech sectors, that have increased their share of overqualified workers experienced higher productivity. Mahy et al. (2015) and Kampelmann et al. (2020) empirically find that overeducation raises productivity and profits more in high-tech firms than in low-tech ones. Due to rapid technological change, whereby workers' initial surplus skills can quickly become the required level or even fall short, hiring overqualified workers may offer a hedge against skill obsolescence (Leiponen, 2000; Malik et al., 2019). These effects may be more pronounced in smaller firms, where innovative human capital seems to be more determinant (McGuirk et al., 2015), with education, experience, and skills playing an especially critical role in growth and internationalization (Onkelinx et al., 2016).

3. Methodology

3.1. Data

To answer the questions of whether hiring through IEFP and, secondly, hiring overqualified individuals through public employment services have significant impacts on firms' performance, this dissertation draws on aggregated data derived from the Institute for Employment and Vocational Training (IEFP) microdata and made available through Banco de Portugal's Microdata Research Laboratory (BPLIM) for the period 2013–2022 (BPLIM, 2026b). The underlying microdata contain monthly information on individuals registered with IEFP as job seekers, including their educational attainment, as well as information on vacancies posted by firms, including the minimum educational requirements for each job position. In accordance with BPLIM's data access rules[2], these microdata are available only to internal researchers. Consequently, this study was conducted using a customized anonymized dataset containing aggregated firm-level information, to which on-site access was granted. Additionally, we make use of microdata from the Central Balance Sheet Harmonized Panel Data (CBHP) (BPLIM, 2026a).

The IEFP database contains records of vacancy postings by firms and job seeker registrations. As Portugal's public employment service (PES), the IEFP's primary mandate is to support unemployed individuals in re-entering the labor market through targeted interventions, the most prevalent of which is vocational training (IEFP, n.d.). As registration with the IEFP is primarily driven by unemployment status, the job seekers it covers do not constitute a representative sample of the Portuguese labor force as a whole, given that employed individuals and other active workers outside the unemployment system are largely absent from its records. While registration with the IEFP is not universally mandatory, individuals are legally required to register within 90 days of becoming unemployed in order to access unemployment benefits (Portal de Serviços Públicos da República Portuguesa, 2026). This requirement ensures broad coverage of the unemployed population and substantially mitigates the risk of selection bias. Critically for the purposes of this study, the

[2] Banco de Portugal's Microdata Research Laboratory. (2025). *BPLIM datasets guide*. Banco de Portugal. https://github.com/BPLIM/Manuals/blob/master/Guides/02_BPLIM_Datasets_Guide/BPLIM_Datasets_Guide_v072025.pdf

IEFP records include both the qualification level attained by job seekers and the qualification level required by employers for each advertised vacancy, which constitutes the foundation for constructing the educational mismatch variable. Notwithstanding its merits, several limitations of this data source warrant acknowledgement. The platform does not encompass all employers, as many firms recruit through alternative channels outside the IEFP system. As a result, even when both parties are registered, job matches occurring outside the platform remain unobserved and cannot be quantified. Despite these constraints, the granular individual- and firm-level information available in the IEFP dataset represents a meaningful segment of the Portuguese labor market, rendering it a valuable source for analyzing job matching dynamics.

The second dataset, the Central Balance Sheet Harmonized, includes companies' financial information and firm characteristics, such as size and founding year. A limitation of this dataset is that it lacks information about workforce characteristics; that is, we don't have individual-level characteristics aggregated at the firm level. This means that, differently from other papers that investigate this topic (Coraggio et al., 2025; Kampelmann et al., 2020; Mahy et al., 2015), we cannot control for aggregate individuals' characteristics (for example, tenure, age, gender…) and therefore cannot account for how these differences interfere with firm performance, for example it doesn't allow us to discard the hypothesis that bigger firms have higher quality workers or to capture the heterogeneity in the workforce composition and how this could be correlated to the educational mismatch variables (Rocha et al., 2025). However, in the absence of these data, we employ firm fixed effects to control for constant, time-invariant firm characteristics, whether observed or unobserved. Therefore, if workforce characteristics do not substantially change over time within a firm, this concern is partially mitigated by the inclusion of firm fixed effects. By merging the two datasets, we can identify, for instance, how many firms have recruited at least once through IEFP, which allows us to later on define the treatment and control groups.

3.2. Cleaning the sample

The IEFP dataset contains 357,778 observations with information regarding 150,577 uniquely identified firms. The CBHP dataset contains 4,411,379 observations corresponding to 720,635 unique firms. After merging the two, we obtain a dataset with 4,448,059 observations for 731,908 firms containing three types of observations: those with information available only from IEFP, those with information available only from CBHP,

and those containing information from both sources. We refer to the third category as linked observations.

Between 2013 and 2015, the number of linked observations increased significantly, suggesting that firms increasingly relied on IEFP to recruit new workers during this period. The years 2015, 2016, and 2014 register the highest number of linked observations, in that order, with 2015 alone accounting for 44,921 linked observations. From 2016 onward, this trend reverses, reaching its lowest point in 2020, with 23,321 linked observations. The following year shows a sharp recovery, with the number rising to approximately 33,000, a level sustained through 2022. This evolution is consistent with the mechanism described by Merkl and Sauerbier (2024) in their study of the matching efficiency of the Federal Employment Agency in Germany. The authors found that during economic expansions, firms post a larger share of their vacancies through the public employment agency, as increased congestion in the private search market makes the public employment agency channel comparatively more attractive. This parallel is broadly consistent with the economic context of our sample period: 2013-2015 corresponds to a period of recovery following the financial and sovereign crisis in Portugal, while 2021-2022 corresponds to a similar recovery following the COVID-19 pandemic (Aguiar-Conraria et al., 2024).

Additional steps were applied to construct the final database. We removed firms with inconsistent reports (i.e., time gaps), firms with 0 employees, or missing values for all years. Following Dillon et al. (2025), this restriction helps us focus on active, operating firms. We then restricted the dataset to firms that posted at least one vacancy through IEFP, keeping only firms with at least one linked observation between the IEFP and CBHP datasets. The resulting sample comprises 1,088,707 observations, corresponding to 134,217 unique firms.

Next, we compare the characteristics of the firms in both datasets. Regarding their dimension, because firms may change size over time, we use a fixed value for categorization. Firm-size categories are defined based on the number of employees in the firm's first year in the database: micro firms (5 or fewer employees), small firms (6–9 employees), medium firms (10–49 employees), and large firms (50 or more employees). The overall sample is composed predominantly of microenterprises, which represent around 82% of the total, while small and medium-sized firms each account for a similar share of approximately 8%, and large firms represent only 1.5% of the sample. Within the subsample of firms with at least one

linked observation, microenterprises are less dominant, even though they still account for the majority (68%) of observations, with the remaining three categories now carrying greater weight: small and medium firms each maintain a similar share of around 12–13%, while large firms now reach approximately 4% of the subsample. This suggests that for larger firms, more established within their market, recruiting through IEFP may be more beneficial than for the other categories. Since firms in the subsample are relatively larger than those in the overall sample, they employ, as expected, a substantially larger workforce, averaging 18 workers, roughly twice as many compared to the overall average of 8. Sector-wise, firms in both groups are present in 5 main sectors: wholesale and retail trade; manufacturing; accommodation and food service activities; professional, scientific and technical activities (consultancy); and construction.

3.3. Overeducation measure

In particular, the variables present in this database measure the proportion of job positions occupied by individuals with the levels of qualification superior, inferior, and equal to the minimum level of qualification required by employers, respectively. In addition, it also contains a dummy variable that identifies whether the firm covered at least one position through IEFP in that year, and another dummy variable that takes the value 1 if the firm had at least one posting in that year.

From the literature, there are three common approaches to measure overeducation. The first is called the subjective or self-assessment approach, and, as the name suggests, it requires the employee and/or the employer to identify the level of formal education associated with performing a given job (Kampelmann et al., 2020). This approach is heavily criticized as workers might under- or overestimate the skills they possess relative to the ones actually necessary to do the job (Rocha et al., 2025). Additionally, if the firms in the analyzed sample are small, assessing the necessary skills might be tricky as they lack a solid base of comparison, leading to errors (McGuinness, 2006). The second approach is known as the objective measure or job analysis approach, and is based on the assessment by professional analysts of the level and type of education required by a specific occupation (Kampelmann et al., 2020). A disadvantage can be attributed to the fact that these classifications are not regularly updated, which, in a time of rapid technological change, fails to capture the changes in job requirements (Rocha et al., 2025). The final approach, and the most used in recent studies such as Kampelmann et al. (2020), Mahy et al. (2015), and Rocha et al. (2025), is called the

realized matches approach, and it determines the required level of education for a position from the level of education that workers in the corresponding occupation have usually obtained (Kampelmann et al., 2020). The required education is generally computed based on the mode or mean of the education in a given position (Kampelmann et al., 2020). For more strengths and weaknesses of each approach, see the explanations by McGuinness (2006).

The construction of the educational mismatch variables in this study follows a method that, while distinct from those employed in the existing literature, more closely resembles an objective measure of mismatch. Educational attainment and job education requirements, as derived from employment offers advertised through IEFP, are each classified into one of three categories: (1) nine or fewer years of formal schooling; (2) more than nine years of schooling but below tertiary education; and (3) at least a university degree. A worker is then classified as adequately matched, overqualified, or underqualified based on a direct comparison between their educational category and that required by the position advertised in the job offer. This approach allows for a systematic assessment of the degree to which firms fill their vacancies with workers whose qualifications align with, exceed, or fall short of the stated requirements.

3.4. The Model

Up until now, the firm-level literature on educational mismatch has developed around the over-, required-, and undereducation (ORU) framework, where the measures of educational mismatch are obtained through the realized matches approach (Grunau, 2016; Kampelmann & Rycx, 2012; Kampelmann et al., 2020; Mahy et al., 2015; Rocha et al., 2025) implemented through the Generalized Method of Moments (GMM) to investigate the effects on productivity and profitability. Our research seeks to establish an association between a firm's decision to hire through IEFP and, within that, hiring overqualified workers, and firm-level outcomes such as sales, value added (as a proxy for productivity), exports, and profit measures. These questions more closely resemble a treatment-effect question; therefore, we opted for a difference-in-differences (DiD) and event-study method.

A difference-in-differences (DiD) design identifies this causal effect by comparing the change in outcomes for treated firms (before vs. after adopting recruitment via IEFP) against the change in outcomes for control firms over the same period, differencing out any time-invariant firm-specific characteristics (e.g., persistent management quality or sector

positioning) as well as any aggregate shocks common to both groups (Goodman-Bacon, 2021). We complement the static DiD method with an event-study specification for two main reasons. First, it allows us to analyze the treatment effect over time, informing us not only about the average effect of IEFP hiring but also how quickly it materializes, persists, or fades in the post-treatment period. Second, the event-study provides visual confirmation of whether the parallel trends assumption holds, through the pre-treatment coefficients: if treated and control firms evolved similarly before treatment, these coefficients should be close to zero and statistically insignificant. The method chosen is more commonly used to measure the impacts of public employment service interventions, for example, in Lombardi et al. (2018) or Blasco and Pertold-Gebicka (2013) than to investigate the effects of overqualification, as previously described.

The baseline difference-in-differences specification takes the following form:

$$Y_{i,t} = \beta \cdot Treat_coloc_i \cdot Post_{i,t} + \gamma Y_{i,t-1} + \alpha_i + \lambda_t + \varepsilon_{i,t} \quad (3.1)$$

Where $Y_{i,t}$ is the outcome of interest for firm i in year t. Variables of interest include: productivity (measured by value added per worker); employment (measured by the number of employees); sales per worker; wages per worker; profit measures per worker such as EBITDA, operating result, and net profit; total assets per worker; intangible assets per worker; and export volume per worker. The outcome variables are expressed in per-worker terms to account for differences in firm size. The monetary variables were normalized using the Consumer Price Index (CPI) obtained from INE (INE, 2026). Additionally, all variables were transformed into natural logarithms. $Y_{i,t-1}$ represents the one-period lag of the dependent variable, included to control for the persistence commonly observed in firm-level outcomes. α_i and λ_t denote the firm and year fixed effects, respectively. The first absorbs all time-invariant firm-specific heterogeneity, while the second absorbs aggregate shocks common to all firms in a given year. $\varepsilon_{i,j}$ represents the residual term. $Treat_i$ is a time-invariant indicator equal to one when the firms belong to the treatment group, that is, when firms successfully hire through IEFP at any point during the sample period, and zero for firms that attempted to recruit via this institution but were unable to. While our treatment indicator is invariant over time, firms in our sample can choose to recruit via IEFP in different calendar years. $Post_{i,t} = 1(t \geq T_i^*)$ equal to one from firm i's own treatment year

T_i^* onward. The coefficient β is estimated by ordinary least squares within this two-way fixed-effects (TWFE) framework, and is interpreted as the average treatment effect of hiring through IEFP on $Y_{i,t}$: a positive and statistically significant coefficient β indicates that firms hiring through IEFP experience higher outcomes than they would have in the absence of treatment, relative to firms that were unable to recruit IEFP participants. For the coefficient β to be interpreted as an average causal treatment effect, two identifying assumptions are required. First is no anticipation of treatment, which requires firms not to adjust their behavior in advance of actually hiring through IEFP, in anticipation of future treatment. Second is the parallel trends assumption, which requires that, in the absence of treatment, treated and control firms would have followed the same trend outcomes over time.

The event-study specification is obtained by allowing the coefficient on the treatment interaction to vary by event-time period:

$$Y_{i,t} = \sum_{\substack{-5 \le k \le 5 \\ k \neq -1}} \beta_k \cdot Treat_coloc_i \cdot D_{i,t}^k + \gamma Y_{i,t-1} + \alpha_i + \lambda_t + \varepsilon_{i,t} \qquad (3.2)$$

Now, each coefficient β_k can be interpreted as the gap between the treated and never-treated firms' outcomes at relative period k, relative to the omitted pre-treatment reference period, allowing us to trace the magnitude and persistence of this association over time. $D_{i,t}^k$ is an indicator variable equal to one when firm i, in calendar year t, is k periods away from its first overqualified hire, where $k = t - t_i^*$ and t_i^* denotes the calendar year of the first hire through IEFP. The remaining terms maintain the meaning described earlier.

To investigate whether the effect is heterogeneous depending on whether the firm hires overqualified workers through IEFP, we extend the specification as follows:

$$\begin{aligned} Y_{i,t} = & \sum_{\substack{-5 \le k \le 5 \\ k \neq -5}} \beta_k \cdot Treat_over_i \cdot D_{i,t}^k + \sum_{\substack{-5 \le k \le 5 \\ k \neq -5}} \theta_k \cdot Hires_{i,t} \cdot D_{i,t}^k \\ & + \sum_{\substack{-5 \le k \le 5 \\ k \neq -5}} \delta_k \cdot Treat_over_i \cdot Hires_{i,t} \cdot D_{i,t}^k \\ & + \gamma Y_{i,t-1} + \alpha_i + \lambda_t + \varepsilon_{i,t} \end{aligned} \qquad (3.3)$$

The specification includes three sets of interactions, each summed over the sample period from 2013 to 2022, and excluding the baseline relative period $k = -5$. Compared to (3.2), $D_{i,t}^{k}$ is an indicator variable equal to one when firm i, in calendar year t, is k periods away from its first overqualified hire, where $k = t - t_i^*$, but t_i^* denotes the calendar year of the first hire through IEFP in which more than 50% of vacancies were filled with overqualified workers. The first interaction captures the average difference in outcomes between treated and control firms at each period k relative to the event, where $Treat_over_i$ is a time-invariant binary indicator equal to one if the firm belongs to the treatment group. The coefficient β_k measures the average level difference in each period, regardless of the number of overqualified workers hired. The second interaction captures the association between the cumulative number of overqualified hired, $Hires_{i,t}$, and firm outcomes at each period k, for the control group. The term $Hires_{i,t}$ is constructed as the running sum of overqualified workers placed via IEFP by firm i up to year t, and is calculated by multiplying the number of vacancies offered and proportion of vacancies filled with overqualified. By construction, this term takes the value 0 in all pre-treatment periods and increases monotonically thereafter. The coefficient θ_k captures how the cumulative intensity of overqualified hiring is associated with firm outcomes at each horizon for control firms. The final interaction is the term of primary interest. The coefficient δ_k measures whether the association between cumulative overqualified hires and firm outcomes differs between treated and control firms in each relative period, that is, whether the intensive margin of overqualified hiring has a differential effect on firm performance for firms that systematically hire overqualified workers relative to those that do not. $Y_{i,t}$, $Y_{i,t-1}$, α_i and λ_t retain the same meaning as in the previous specifications.

3.5. Defining the treatment and control group

The specifications presented above rely on a comparison between treated and control firms' evolution over time. Defining these two groups is therefore essential to the identification of β. To answer our first research question, we define the treatment group as firms that posted at least one vacancy through IEFP and that successfully hired a worker as a result. The control group consists of firms that registered with IEFP, but that had no placements. We assume these firms are a more suitable counterfactual than firms that never engaged with IEFP, as they are likely more comparable to treated firms in terms of underlying hiring needs

and firm characteristics. However, this construction did not satisfy the parallel trends assumption. Therefore, we restrict the treated-firm group to firms that registered with IEFP, filled at least one vacancy, and had an IEFP usage intensity below 50%. Usage intensity is defined as the ratio of vacancies posted through IEFP to the number of employees in the period immediately before the first treatment. We measure intensity using posted vacancies rather than actual placements, as it better captures the firm's overall exposure to the IEFP as a recruitment channel. This 50% threshold is intended to exclude firms that are excessively dependent on IEFP as a recruitment channel, whose behavior and performance trajectories could be different from firms less reliant on IEFP. This restriction to the treatment as an identification strategy has been used before. For instance, Blasco and Pertold-Gebicka (2013) restrict their initial sample to firms for which more than 33% of the workforce was employed in Southern Jutland County. The control group remains as defined before. After this restriction, the treatment group is composed of 21,733 firms and the control group of 55,340 firms. Firm size composition differs substantially between treatment and control groups: control firms are predominantly micro (71.49%), with small, medium, and large firms accounting for 13.26%, 13.57%, and 1.68%, respectively, while treated firms are more evenly distributed across categories, with 41.42% representing micro firms, 20.71% small firms, 30.33% medium firms, and 7.54% large firms. Given this distribution, control firms employ an average 9 workers, compared to 31 workers for treated firms, and are, on average, four years younger than treated firms, further confirming that firms that make greater use of IEFP are larger and more established.

For our second research question, the treatment and control groups are constructed by subdividing the treatment group from research question one. Specifically, the new treatment consists of firms that filled more than 50% of their posted vacancies with overqualified individuals the first time they successfully hired through IEFP. The new control group consists of the remaining firms from the original treatment group, that is, those that hired through IEFP workers at the required qualification level, underqualified workers, or overqualified workers at a lower proportion. This yields a treatment group of 14,716 firms and a control group of 40,624 firms. The two groups have similar characteristics. Firms in both groups are, on average, 17 years old, and size composition is nearly identical: micro firms account for around 42% of the total, small firms comprise 20%, medium firms account

for up to 30%, and large firms represent 7%. Overall, hiring a higher share of overqualified workers are not markedly different from other firms posting vacancies through IEFP.

We next define how the treatment and post-treatment periods are constructed for each firm. The post-treatment period begins in the first year in which the firm hired a worker through IEFP. The treatment is modeled as absorbing: once treated, it remains treated in the subsequent periods (Sun and Abraham, 2021). This assumption reflects the fact that firms do not necessarily hire through IEFP in every year they appear in the dataset, and, therefore, avoids misclassifying previously treated firms as untreated in years without IEFP activity. For control firms, the post-treatment period begins in the first year in which the firm posted a vacancy but did not find a suitable candidate. This construction ensures that both treated and control firms have pre- and post-treatment periods necessary for the event-study/DiD design (Goodman-Bacon,2021). Unlike settings in which treatment is triggered by an exogenous event common to all units, in this setting it occurs at firm-specific moments in time, since firms turn to IEFP and succeed or fail in filling vacancies at different points during the sample period. As a result, there is no natural calendar-time cutoff that can be applied uniformly to define the post-treatment period for control firms. To preserve comparability between treated and control units despite this asymmetry, the post-treatment indicator for control firms is constructed around an analogous firm-specific reference point, the firm's own first unsuccessful recruitment attempt, rather than a common date shared across all firms.

Before applying the specification, we trim the event-time window to . The initial relative-time window spanned the full sample period (), but retaining it would mean estimating coefficients in the extreme periods from a small, unbalanced set of firms, raising concerns about compositional imbalances across event-time bins, multicollinearity (Sun & Abraham, 2021), and the appropriateness of the counterfactual (Miller, 2023).

Even after applying the intensity-of-use criteria and trimming the event-time window, the parallel-trends assumption was not supported, as shown in Table A.1. in the appendix. Therefore, we imposed three additional restrictions targeting remaining threats to identification. The first requires firms to have at least ten employees in the year before treatment. This threshold recurs throughout the firm-level literature, albeit for distinct reasons. Lombardi et al. (2018) restrict their sample to firms with less than 30 employees in

the year prior to treatment, reasoning that treatment intensity relative to firm size affects comparability between treated and control groups. Our concern is the mirror image of theirs: a single IEFP placement can represent a disproportionately large share of the workforce in very small firms, regardless of the true reliance on IEFP. Maintaining this subgroup risks generating noisy and non-comparable estimates rather than a clear measure of the treatments' effect. The same threshold is also used by Martins et al. (2022) for reasons of data comparability and representativeness in a firm-level study of the Portuguese economy, and in Depalo and Viviano (2024), to avoid contamination from current policies. These examples show that firm-size restrictions of this kind are present in the literature, even though the motivation behind the cutoff differs from study to study. In our case, the restriction is motivated primarily by the disproportionate and mechanically driven treatment intensity that very small firms would exhibit. The second restriction excludes firms that reduced their workforce in the year immediately before or after treatment, to avoid capturing substitution effects (replacing existing employees with subsidized or PES-hired workers), rather than net employment growth from treatment itself. Such substitution would make it difficult to distinguish between workforce expansion and a compositional change. This concern is related to the one raised by Depalo and Viviano (2024), who exclude firms that enter or exit the sample around the policy window, since new entrants mechanically need to hire and exiting firms mechanically stop hiring, independent of any real treatment effect. Although our restriction operates on the intensive margin, both serve to prevent employment dynamics unrelated to the treatment being mistaken for its effect. The third restriction requires each firm to be observed for at least five relative-time periods (two years before and two years after treatment), ensuring every firm has a genuine pre- and post-treatment window. Without this restriction, firms treated near the start or end of the event-time window would contribute to some relative-time bins but not others, which could introduce other dynamics into the event-study. Dillon et al. (2025) impose an analogous minimum of five pre-treatment years for every cohort in their sample for the same reason. Although these restrictions reduce our sample significantly, they make the identifying assumption of the event-study design more plausible. The final samples of firms are as follows: the general PES-hiring analysis, 21,248 treated firms and 5,166 control firms; for the overqualification analysis, 5,712 treated firms and 15,536 control firms. Tables 1 and 2 present descriptive statistics for both final samples.

Table 1. Descriptive statistics (final sample): treated vs control firms

	Control			Treated		
	Mean	Median	SD	Mean	Median	SD
Wages per Worker	15,995.75	13,107.35	(12,987.41)	17,501.18	15,425.80	(12,149.13)
Productivity	21,371.79	15,997.39	(33,444.82)	25,438.22	20,259.74	(43,956.35)
Employment	21.23	10.00	(131.29)	62.48	21.00	(344.76)
Sales	91,117.44	44,340.50	(230,624.90)	109,157.02	57,967.82	(411,237.96)
Intangible Assets	5,232.20	0.00	(79,873.01)	4,835.88	0.00	(108,513.53)
Total Assets	152,230.72	41,568.03	(1,634,948.37)	143,761.75	56,850.80	(2,451,148.97)
EBITDA	8,343.87	2,845.53	(94,810.44)	10,182.71	4,466.31	(233,667.19)
Net Profit	10,200.90	8,597.62	(9,481.33)	12,235.52	10,826.69	(8,411.24)
Operational Result	5,163.18	1,361.84	(93,359.96)	6,472.05	2,222.99	(230,934.07)
Export Volume	13,271.09	0.00	(89,592.48)	17,889.54	2.67	(82,189.27)
Nr. of Firms		5,166			21,248	

Table 2. Descriptive statistics (overqualification study): treated vs control firms

	Control			Treated		
	Mean	Median	SD	Mean	Median	SD
Wages per Worker	17,749.83	15,598.04	(11,688.25)	16,831.42	15,002.42	(13,288.38)
Productivity	25,834.92	20,448.32	(48,138.24)	24,369.52	19,825.13	(29,884.84)
Employment	59.08	22.00	(260.17)	71.61	21.00	(506.41)
Sales	106,800.74	57,234.39	(341,661.10)	115,504.72	60,162.34	(556,945.50)
Intangible Assets	5,457.09	0.00	(116,262.65)	3,162.39	0.00	(84,137.80)
Total Assets	138,706.66	57,311.44	(1,224,005.98)	157,379.92	55,822.03	(4,261,192.14)
EBITDA	9,816.87	4,428.13	(233,911.65)	11,168.28	4,567.88	(233,007.14)
Net Profit	12,417.15	10,934.72	(7,921.23)	11,746.21	10,563.85	(9,590.74)
Operational Result	6,102.73	2,204.36	(231,257.77)	7,466.97	2,272.73	(230,059.44)
Export Volume	17,692.70	9.57	(69,775.93)	18,419.81	0.00	(108,796.95)
Nr. of Firms		15,536			5,712	

In our treatment design, firms begin hiring through IEFP at different points in time rather than at a single common date, closely resembling a staggered-timing treatment setting. This has direct implications for how the coefficients from our difference-in-differences and event-study should be interpreted: rather than causal treatment effects, we interpret them as associations, given the assumptions underlying these models. Beyond the parallel trends assumption, causal identification in a TWFE design requires the treatment to be assigned at a common point in time, a condition our setting does not satisfy. This staggered timing means the coefficients cannot, in general, be given a clean causal interpretation without further correction. Goodman-Bacon (2021) show that, under staggered timing, the TWFE coefficient does not come from a single clean comparison between treated and control firms, but implicitly averages many two-group, two-period ("2x2") comparisons across different

pairs of firms and periods. Some of these comparisons may use firms that were already treated earlier as if they were a valid control group for firms treated later, contaminating the estimates when treatment effects evolve. When this happens across enough comparisons, the overall TWFE coefficient can diverge substantially from the true average treatment effect, and in extreme cases even reverse its sign. Sun and Abraham (2021) extend this insight to event-study specification, showing that individual period coefficients, including pre-treatment ones, can be contaminated by treatment effects from other relative-time periods, potentially generating spurious pre-trends even when the true parallel-trends assumption holds. Recent estimates address these concerns. Callaway and Sant'Anna (2021), for instance, estimate group-time average treatment effects using not-yet-treated units as controls, avoiding comparisons where already-treated cohorts bias the estimate. These estimators, however, still require the same core identifying assumptions (parallel trends, no anticipation), simply offering more robust ways of aggregating treatment effects under heterogeneity.

We retain the simpler TWFE specification for various reasons. First, it still informs about the association between recruiting via IEFP and the firm outcomes, enabling comparisons with prior PES-intervention studies. Second, the DiD and event-study together provide complementary information: the former summarizes the average conditional difference, and the latter describes its dynamic evolution. Third, our diagnostics offer some reassurance, with pre-treatment coefficients satisfying the parallel trends assumption, and post-treatment coefficients generally showing no strong trends, reducing (but not eliminating) severe distortion concerns by the staggered timing (Goodman-Bacon, 2021). Therefore, we interpret the resulting coefficients as associations, leaving heterogeneity-robust DiD estimators as an important extension for future work.

4. Results

This section presents and discusses the empirical results of this dissertation. The analysis is structured into two parts, following the research questions outlined throughout this dissertation. The first part examines the impact of recruiting through IEFP on firm-level outcomes. We begin by presenting the results of the main specification, which estimates the average effect of hiring via IEFP on the full sample. Subsequently, we examine whether the results vary according to industry type (high-tech and low-tech industries) and firm size,

where we forgo the restriction on the number of employees in the pre-treatment period. Afterwards, we analyse whether the effects are heterogeneous according to the number of overqualified workers hired through IEFP. We report full coefficient tables for our main specification and robustness checks to better compare between specifications. For the heterogeneity analysis by industry and firm size, we rely only on event-study graphs, given the focus on comparing patterns across subgroups.

4.1. Main results

Table 3 presents the aggregate results for the first part of our study following the differences-in-differences specification, capturing the average treatment effect over the entire post-treatment period. Table 4 then presents the dynamic treatment effects from the event-study specification. Each column identifies the effects for each outcome of interest.

Table 3. DiD: Effects on firm-level outcomes

	(1) Wages	(2) VA	(3) Number of Employees	(4) Sales	(5) Intangible Assets	(6) Total Assets	(7) EBITDA	(8) Net Profit	(9) Operational Result	(10) Export Volume
Treatment x Post	0.013***	0.011***	0.026***	0.018***	-0.021	-0.006*	-0.009	0.023***	-0.015	-0.002
	(0.002)	(0.004)	(0.003)	(0.003)	(0.021)	(0.004)	(0.008)	(0.002)	(0.010)	(0.015)
Lag DV	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	168,213	163,223	168,940	167,514	58,263	168,834	136,235	165,340	125,481	70,981
Adjusted R2	0.792	0.737	0.940	0.892	0.855	0.925	0.763	0.783	0.715	0.849

Notes: All dependent variables are expressed on a per-worker basis. Standard errors clustered at the firm level are reported in parentheses. All specifications include firm and year fixed effects and the lagged dependent variable as a control. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 4. Event-study: Effects on firm-level outcomes

	(1) Wages	(2) VA	(3) Number of Employees	(4) Sales	(5) Intangible Assets	(6) Total Assets	(7) EBITDA	(8) Net Profit	(9) Operational Result	(10) Export Volume
t=-4	-0.019**	-0.021	-0.012	-0.014	-0.176	-0.024	0.023	-0.018	0.007	-0.117
	(0.010)	(0.017)	(0.014)	(0.016)	(0.109)	(0.017)	(0.039)	(0.011)	(0.049)	(0.084)
t=-3	-0.003	0.009	-0.013	0.002	0.125	0.009	-0.009	-0.011	-0.012	-0.050
	(0.008)	(0.014)	(0.011)	(0.013)	(0.095)	(0.014)	(0.033)	(0.008)	(0.041)	(0.068)
t=-2	-0.004	0.011	-0.013	-0.001	0.029	-0.000	-0.015	-0.005	-0.004	-0.061
	(0.007)	(0.012)	(0.010)	(0.012)	(0.072)	(0.012)	(0.027)	(0.008)	(0.034)	(0.054)
t=0	-0.005	0.004	0.035***	-0.007	0.039	-0.029***	-0.059**	-0.003	-0.059*	-0.005
	(0.006)	(0.011)	(0.008)	(0.010)	(0.059)	(0.010)	(0.024)	(0.006)	(0.031)	(0.049)
t=+1	-0.017**	-0.004	0.026***	-0.038***	0.021	-0.030***	-0.072***	-0.010	-0.087***	-0.017
	(0.007)	(0.012)	(0.010)	(0.011)	(0.074)	(0.011)	(0.026)	(0.008)	(0.033)	(0.052)
t=+2	0.010	0.024*	0.056***	0.019	0.063	-0.035***	-0.053*	0.019**	-0.078**	-0.022
	(0.008)	(0.013)	(0.010)	(0.012)	(0.073)	(0.012)	(0.028)	(0.009)	(0.035)	(0.054)
t=+3	0.022***	0.033**	0.075***	0.046***	0.111	-0.035***	-0.049*	0.038***	-0.064*	0.057
	(0.008)	(0.014)	(0.011)	(0.013)	(0.075)	(0.013)	(0.029)	(0.009)	(0.037)	(0.057)
t=+4	0.025***	0.019	0.096***	0.055***	0.091	-0.035**	-0.104***	0.035***	-0.129***	0.056
	(0.009)	(0.015)	(0.011)	(0.014)	(0.081)	(0.015)	(0.031)	(0.010)	(0.039)	(0.058)
t=+5	0.013	0.014	0.094***	0.046***	0.087	-0.038***	-0.072**	0.027**	-0.065	0.032
	(0.009)	(0.015)	(0.012)	(0.015)	(0.080)	(0.014)	(0.032)	(0.011)	(0.040)	(0.062)
Base Period	t=-1	t=-1	t=-1	t=-1	t=-1	t=-1	t=-1	t=-1	t=-1	t=-1
Lag DV	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	168,213	163,223	168,940	167,514	58,263	168,834	136,235	165,340	125,481	70,981
Adjusted R^2	0.792	0.737	0.941	0.892	0.855	0.925	0.763	0.783	0.715	0.849

Notes: All dependent variables are expressed on a per-worker basis. Standard errors clustered at the firm level are reported in parentheses. All specifications include firm and year fixed effects and the lagged dependent variable as a control. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

The DiD estimates in Table 3 provide an informative first picture of the average association between IEFP recruitment and firm-level outcomes. Since the outcome variables are expressed in logarithms, while the treatment indicator is binary, coefficients are converted into percentage effects using . The results indicate that treated firms, on average and statistically significantly, pay higher average wages (+1.3%), are more productive, as measured by the value added per worker (+1.1%), have a larger workforce (+2.6%), and increase their sales per worker relative to the control group after their first hire through IEFP. Net profit per worker also increases significantly (+2.3%). In contrast, total assets per worker, EBITDA, operational result, intangible assets, and export volume show no statistically significant average effect. Together, these estimates suggest a positive but selective association between hiring via IEFP and firm performance, more concentrated in labor and output-related outcomes than financial efficiency measures. However, because the DiD estimates are average effects, it conceals potentially important dynamics about the

evolution of these effects over time. To address this, we complement the DiD with the event-study presented in Table 4.

The pre-treatment coefficients are generally small in magnitude and statistically insignificant across all specifications. The absence of systematic pre-trends in the periods closest to treatment provides broad support for the parallel trends assumption underlying the identification strategy. We find a strong positive association between IEFP hiring and employment growth, measured by the natural logarithm of the number of employees. The number of employees increases significantly from the year of first hire onwards, reaching approximately 9.4% above the control group five years after the treatment occurs. This result should be interpreted with caution. By construction, the firms enter the treatment group because they successfully hired through IEFP, which implies an increase in their workforce at that moment. Control firms are those whose recruitment attempt have not (yet) resulted in a placement, which does not indicate this firms are not intending to expand, since they are also trying to recruit through IEFP. This suggests some divergence between the groups is to be expected. However, if the effect were purely mechanical, we would not expect the difference to continue to widen steadily over the following years, which suggests that the employment response is not fully explained model construction. This means that despite the mechanical effect, firms that were able to hire the first time they posted a vacancy with the IEFP are growing faster than those that were unable to do so.

For value added per worker, in the period of treatment there are no significant effects, only showing weak significance at t=+2 and then a positive significant effect in the following period, fading afterwards. The effect on average wages shows a negative significant association of -1,7% the year after the treatment, potentially reflecting the initial integration of new hires at lower levels coupled with the increase in workforce described before, before becoming positive and significant from t=+3 onwards (+2.2% and +2.5%). The delayed materialization of productivity gains may reflect the time required for new hires to absorb and apply firm-specific knowledge. This interpretation is further supported by the characteristics of the population covered by public employment services. As documented in the literature, PES participants tend to be lower-skilled workers with weaker labor market attachment (Blasco & Pertold-Gebicka, 2013; Eriksson & Lagerström, 2006) who may require more extensive on-the-job training before becoming fully productive. This could explain both the initial absence of productivity gains and their subsequent emergence as

coefficients increase in magnitude over time. Additionally, after recruitment firms may part with less productive matches, retaining only those that produce gains for the firm. However, the loss of statistical in later periods warrants caution in interpreting the later coefficients as evidence of sustained productivity improvements. The observed increase in value added per worker may reflect primarily a scale effect, driven by the expansion of the workforce, rather than genuine improvements in match quality or productive efficiency. This could mean the productivity gains may be partially mechanical, arising from the dilution of fixed costs across a larger number of workers rather than from an improvement in the quality of the employer-employee match.

The financial performance indicators tell a markedly different story. Total assets per worker show a persistent significant negative association throughout the post-treatment periods, reaching -3.8% at t=+5. EBITDA per worker deteriorates sharply, with coefficients ranging from -5.% to -10.4% across t=+1 to t=+4, although with marginal significance in the relative time period in the period. The operational result per worker follows the same path, showing consistently negative and significant effects. These results suggest that while IEFP recruitment is associated with firm growth and some productivity gains, it also entails high financial costs, at least in the medium term. One possible interpretation is that recruiting new employees generates additional costs that are not offset by the gains in output, compressing margins and reducing asset efficiency per worker. Depending on the PES intervention that led to the recruitment, the financial benefits that resulted in the initial decrease in average wages may not compensate for the additional costs in the following periods. Net profit presents a turning positive and significant association starting two years after treatment until the effect disappears in t=+5. This may display the relationship between initial cost pressures and the delayed materialization of productivity and revenue gains. Finally, no statistically significant effects were found for intangible assets and export capacity per worker, suggesting that the use of public employment services does not appear to be associated with changes in innovation investment or international market participation for this group of firms.

These findings are consistent with a narrative in which (medium and large) firms, rely on IEFP to expand their workforce, with gradual improvements in labor productivity and wages. However, this expansion appears to come at the cost of some short-to-medium financial pressure, as reflected in the persistent deterioration of asset efficiency and profitability measures on a per-worker basis. This pattern may reflect the adjustment costs

inherent in rapid employment growth, and raises the question of whether the financial benefits taken from IEFP recruitment is worth the financial losses and small productivity gains in the long-run and whether the performance of treated firms converges to that of the control group over longer horizons, a question that the available data do not allow us to fully address. As a robustness check, the specificationwas reestimated changing the restriction for more than five employees and the results obtained were similar.

Our findings are consistent with the literature in several dimensions. The positive association between IEFP recruitment and employment growth aligns with the evidence in Blasco and Pertold-Gebicka (2013) and Gautier et al. (2018), who suggest that PES interventions encourage firms to open more vacancies. Moreover, the persistent positive effects observed through the event-study are similar to the findings of Depalo and Viviano (2024), who document that participation in public employment services interventions is associated with permanent hiring. Conversely, the positive and persistent associations between IEFP recruitment is at odds with Lechner et al. (2013), who find that firms tend to decrease in size following participation in employment services interventions.

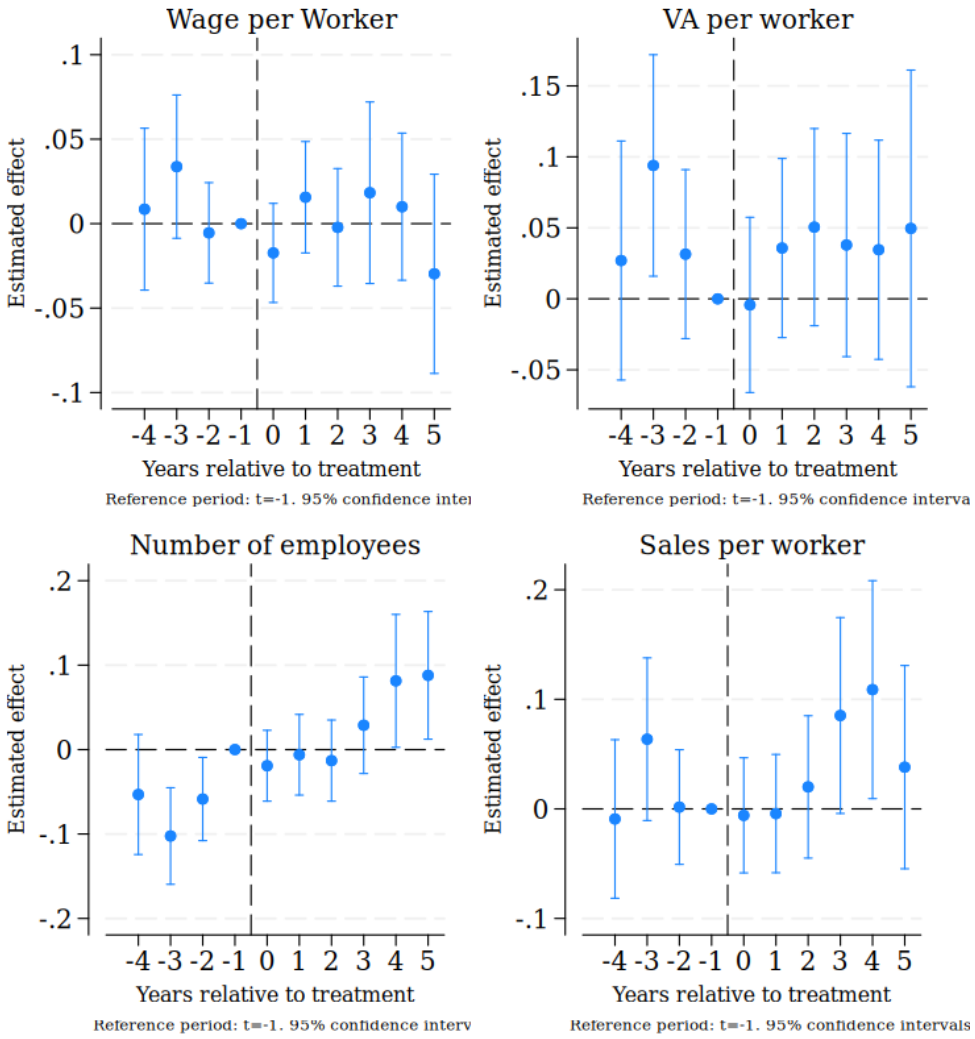
The picture regarding match quality, however, remains mixed. While the positive associations with productivity, sales, wages, and net profit per worker suggest that IEFP recruitment may improve the quality of employer-employee matches, the persistent deterioration in total assets, operational result, and EBITDA per worker raises doubts about whether the overall effect on firm performance is unambiguously positive. In line with Depalo and Viviano (2024), our results also point to a decrease in the capital-to-labor ratio following IEFP recruitment, which raises concerns about the long-run sustainability of the productivity gains observed, particularly given that the positive effects on value added tend to become non-significant in later post-treatment periods. Finally, most of the analyzed literature investigates the impacts of individual interventions, while we assess the overall performance of the Portuguese employment service, which can explain in part the difference in findings.

4.1.1. High-tech vs Low-tech firms

This subsection examines whether the effects documented in the main specification differ across industries. Following the classification proposed by Kampelmann et al. (2020), we group firms into two categories: medium-to-high-tech and knowledge-intensive, and low-tech and less knowledge-intensive sectors, based on the Eurostat (2024) HT/KIS

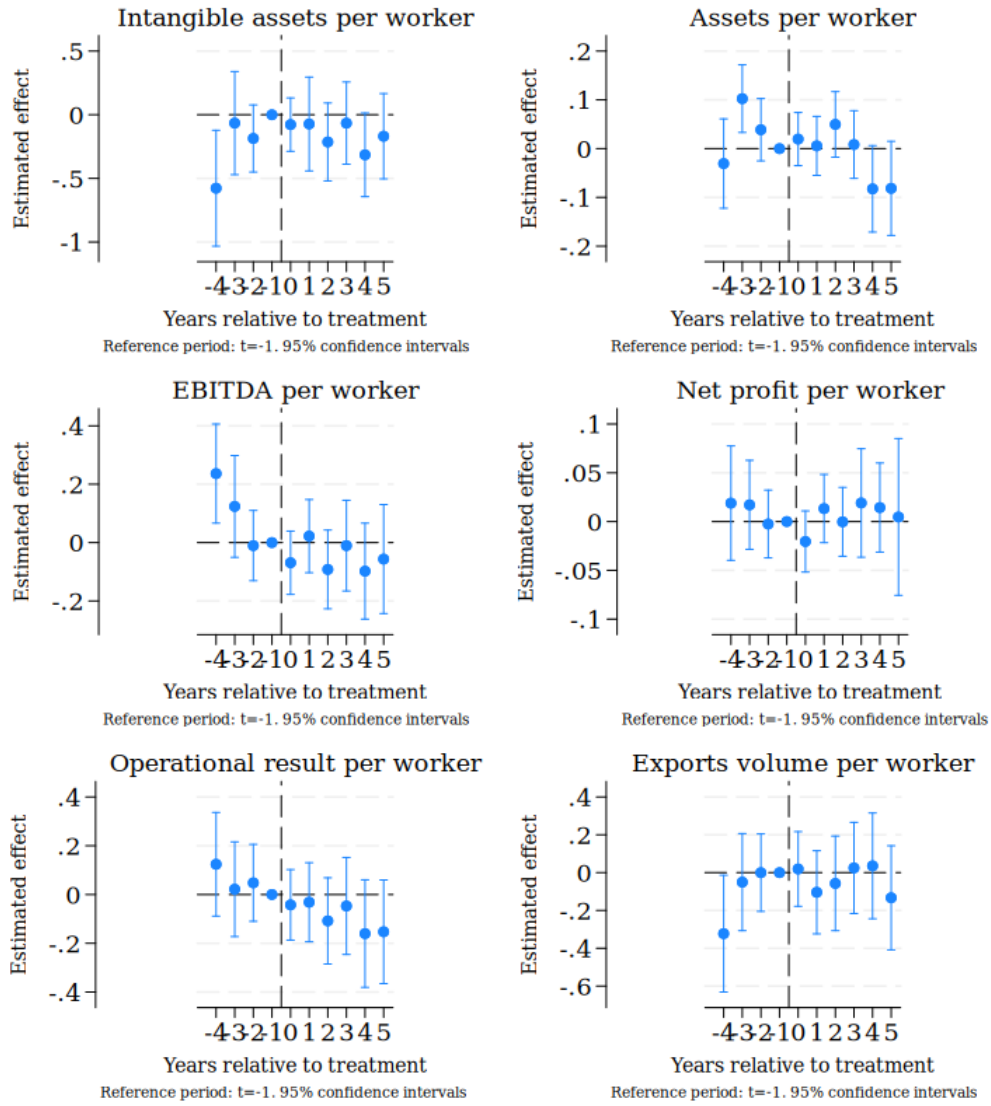
nomenclature.[3] To investigate whether the association between hiring through IEFP and firm outcomes differs according to the firms' technology intensity, we apply (3.2) twice, restricting each time the sample to the high-tech or the low-tech firms.

Figure 1. Event-study: effects on high-tech firms



[3] Given that our data are coded according to the Portuguese CAE Revision 3 classification, we map the two-digit Classification of Economic Activities (CAE) codes to their NACE equivalents, and assign firms to each category accordingly.

Figure 2. Event-study: effects on high-tech firms



Figures 1 and 2 show the evolution of the effects of IEFP recruitment for high-tech firms, and Figures 3 and 4 for firms considered low-tech. This subsample is composed predominantly of low-tech firms, accounting for around 106,000 observations, with high-tech firms representing a smaller share of around 28,000 observations (depending on the outcome specification; for example, for the intangible assets specification, the number of observations is significantly lower for both subgroups). This composition likely explains why the main specification (Table 4) displays patterns similar to those found for low-tech firms, rather than reflecting a balanced picture based on the two subsamples.

Figure 3. Event-study: effects on low-tech firms

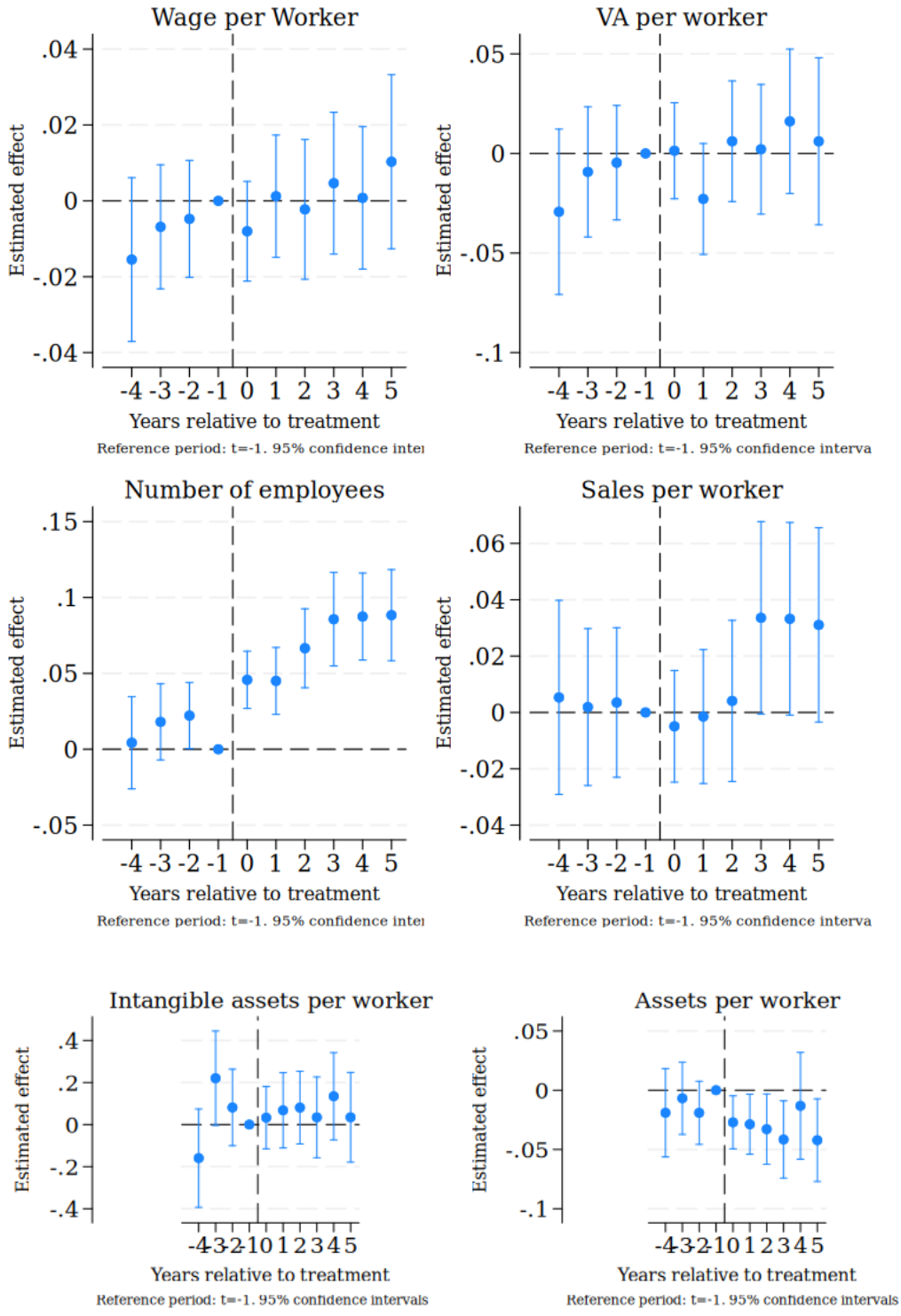
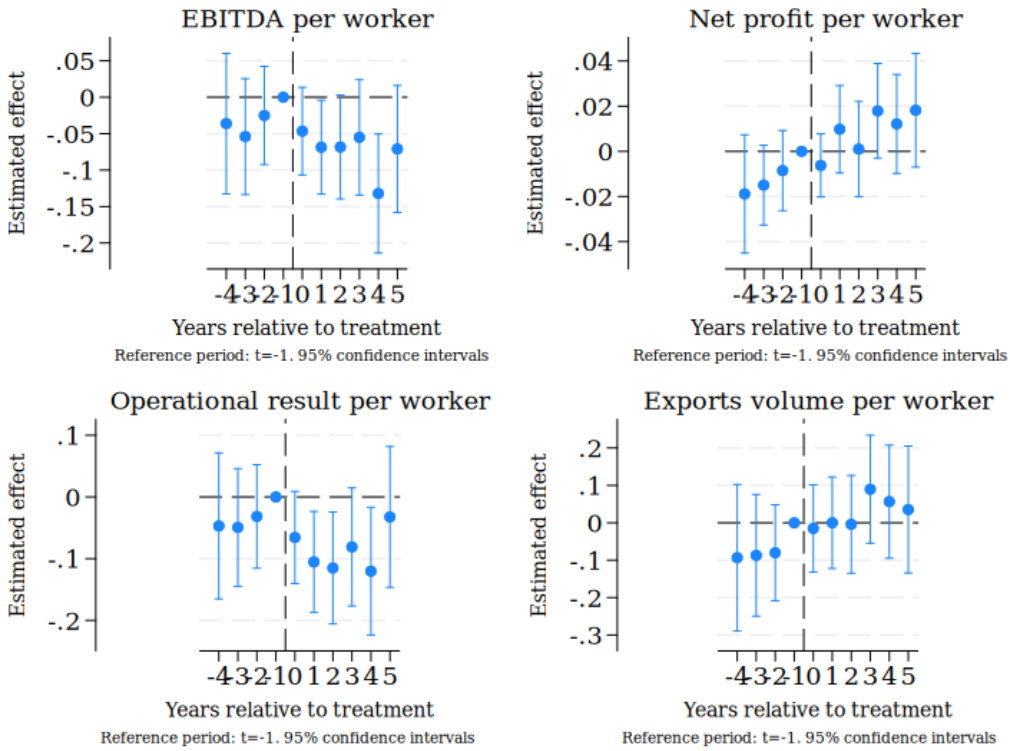


Figure 4. Event-study: effects on low-tech firms



For high-tech firms, several outcomes show significant pre-treatment coefficients: employment at t=-3, and t=-2; intangible assets, export volume, and EBITDA at t=-4; total assets, and value added at t=-3, raising some concern about the parallel trends assumption for this subsample. This is visible in the graphs as a downward pre-treatment trajectory, alongside large isolated swings for the other affected outcomes mentioned. For post-treatment periods, the effects are only significant in later relative time periods for employment (t=+4 and t=+5) and for sales, showing some weak significance at t=+3 and significance at t=+4, as demonstrated by the rising trajectories only clearly distinct from 0 in later periods. For the rest of the outcomes, no significant effects were found at any period, visible by the lack of a trajectory in post-treatment periods. The possible main reason for the lack of results could be the small number of observation that does not allow identification. However, we can question whether the employment and sales gains in later post-treatment periods, alongside the absence of corresponding effects on productivity and financial measures, are consistent with a scenario where workers hired through IEFP, face a steeper learning curve before contributing to firm performance, as discussed by Hitt et al. (2001).

Given that PES usually covers lower-skilled workers, the higher skill requirements typical of high-tech environments may limit their immediate productive contribution.

For low-tech firms, pre-treatment coefficients are close to zero and mostly insignificant, except for employment at t=-2, but immediately followed by a stronger and persistent post-treatment pattern. Employment growth shows the clearest result. Recruiting through IEFP leads to a positive and significant association across all post-treatment periods, visible in the continuous upward trajectory. Although sales appear to display a positive association in later periods, it is only marginally significant at t=+3. Total assets exhibit a significant negative association from the year of treatment and through most of the post-treatment horizon, except for t=+4. EBITDA and operational result, although not significant in every period, show a consistent decline in their graphs. The rest of the outcomes show no significant effects across the post-treatment time window. Therefore, while hiring through IEFP is connected to some employment growth, it is also associated with some financial pressures, pointing to potential difficulties in deploying the additional labor force. Firms that can effectively use their human resources are more likely to grow (Hatch & Dyer, 2004). Taken together, these findings suggest that the effects of recruiting via IEFP are, as expected, heterogeneous across sectors, with the pattern of results differing substantially between high-tech and low-tech firms. Both sectors exhibit employment growth (although only in later periods for high-tech firms) without broad improvements in firm performance in either case in the horizon considered.

4.1.2. Effects according to firm size

To examine whether the association between hiring through IEFP and firm outcomes differs according to firm size we estimate the specification (3.2.) separately for each size category by restricting the sample accordingly (eliminating the restriction concerning the number of employees). Firm size is defined based on the number of employees in the period before the treatment, to capture more accurately the firm's dimension, and follows the same four categories used in the sample description: micro firms (5 or fewer employees), small firms (6–9 employees), medium firms (10–49 employees), and large firms (50 or more employees).

Figure 5. Event-study: effects on micro firms

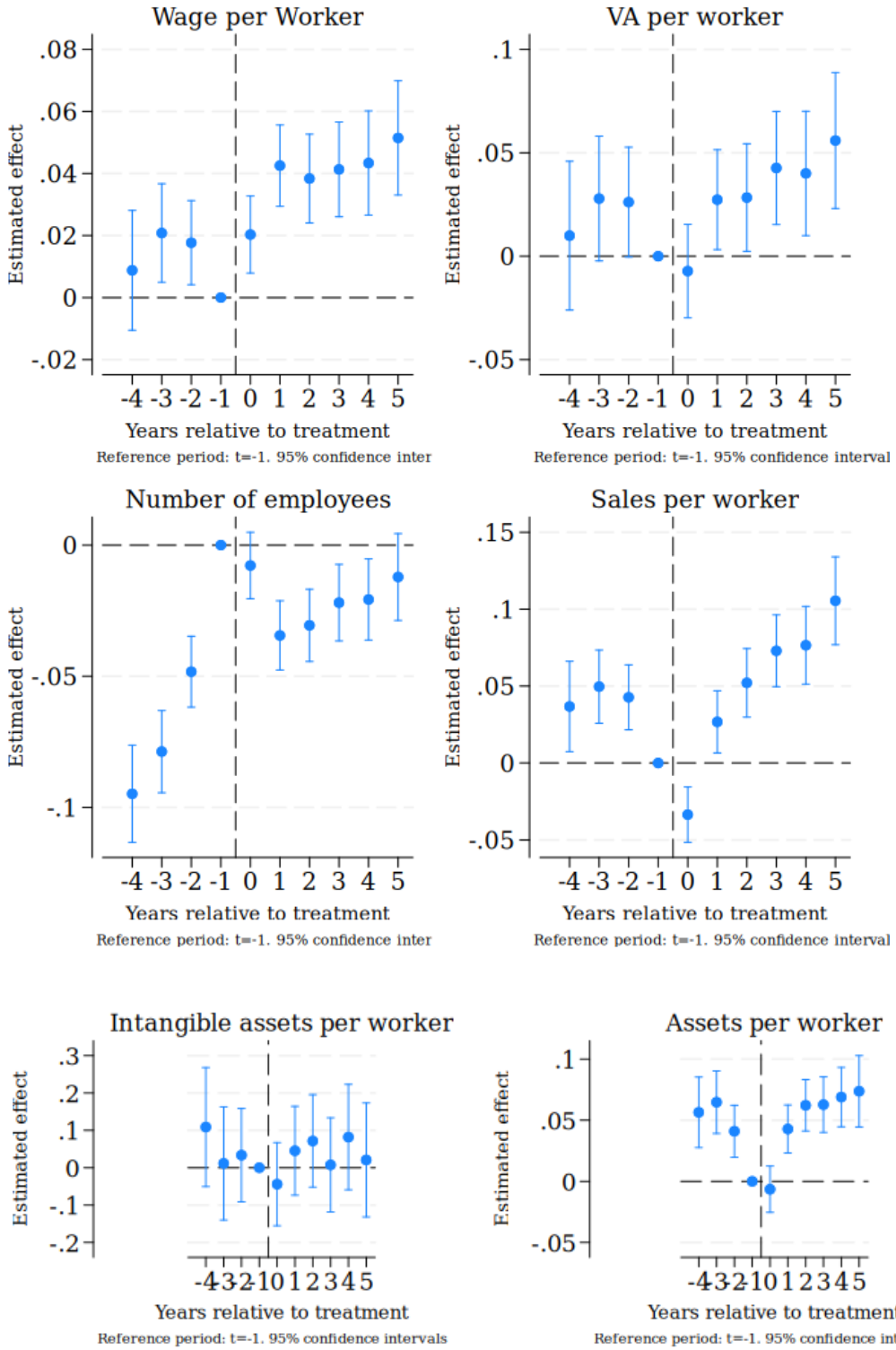
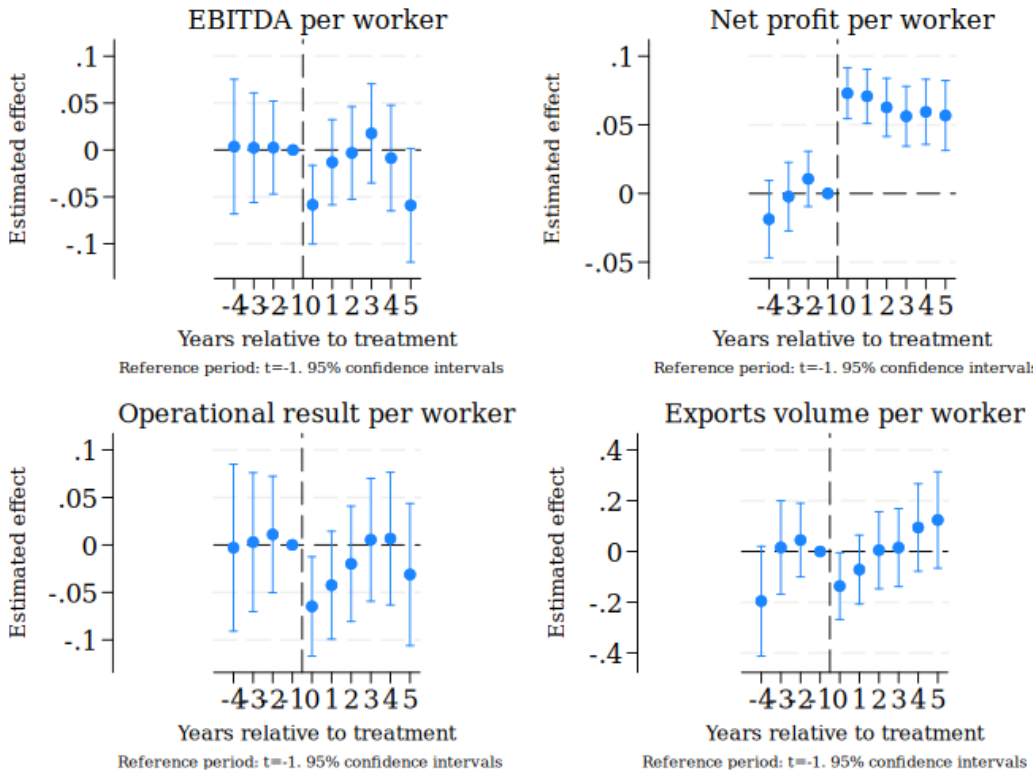


Figure 6. Event-study: Effects on micro firms



For micro firms, the pre-treatment period shows clear evidence of diverging trajectories between treated and control firms well before treatment, and that continue into the post-treatment periods for wages, number of employees, sales, and total assets. Net profit and value added per worker show a positive association with hiring through IEFP starting at t=0 and t=+1, respectively, and remaining so until t=+5, which suggests that recruiting through this channel may be beneficial for micro firms' performance, although given the pre-trends found for other outcomes, these results should be interpreted with caution. For EBITDA, operational result, and export volume, relying on IEFP yields a negative association at the moment of hiring, fading immediately after. The continuity across the treatment year for employment, sales, and total assets constitutes a clear violation of parallel trends, justifying the restriction of the main specification to firms with ten or more employees.

Figure 7. Event-study: effects on small firms

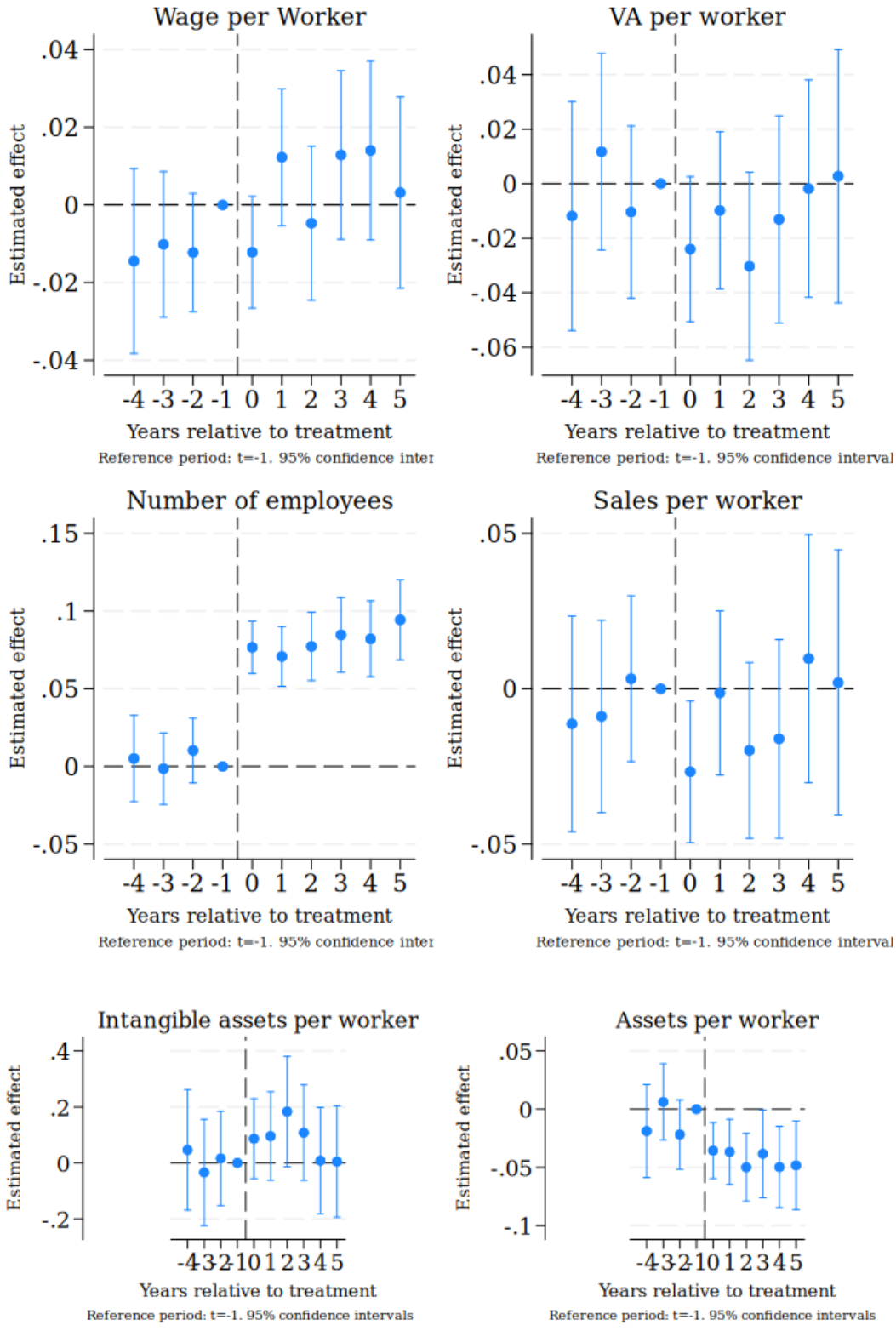
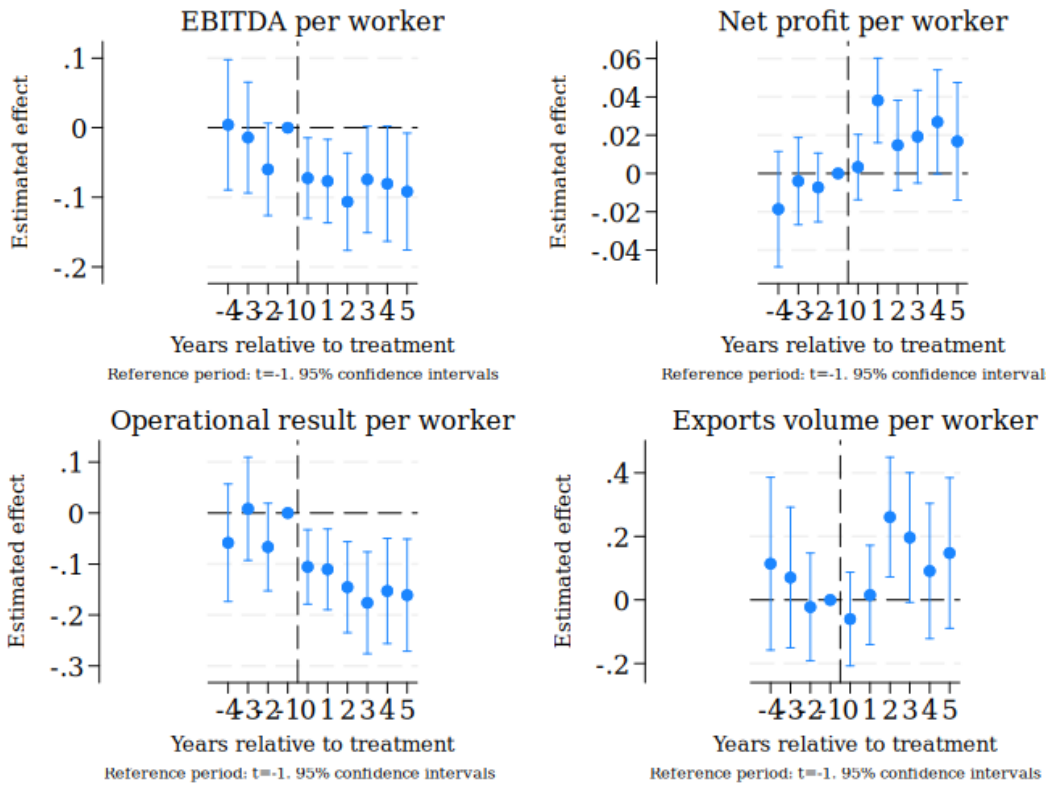


Figure 8. Event-study: effects on small firms



For small firms, pre-treatment coefficients are mostly insignificant and near zero, providing reasonable support for parallel trends. From the graphics, we can see that recruiting through IEFP is associated with a persistent positive association with employment, visible from the upward trajectory in the post-treatment periods; unlike Blasco and Pertold-Gebicka (2013), we found no association with higher turnover following the treatment. Conversely, it is related to a significant negative impact on total assets, EBITDA, and operational result starting at t=0, and continuing throughout the relative time window. For wages, value added, and intangible assets, no effect stands out. For net profit and export volume, we note a single positive effect at t=+1 and t=+2, respectively. The results suggest that small firms may procure IEFP to increase their labor force, although it yields significant financial pressure, casting doubt on whether hiring through IEFP is the most appropriate recruitment channel for smaller firms.

Figure 9. Event-study: effects on medium firms

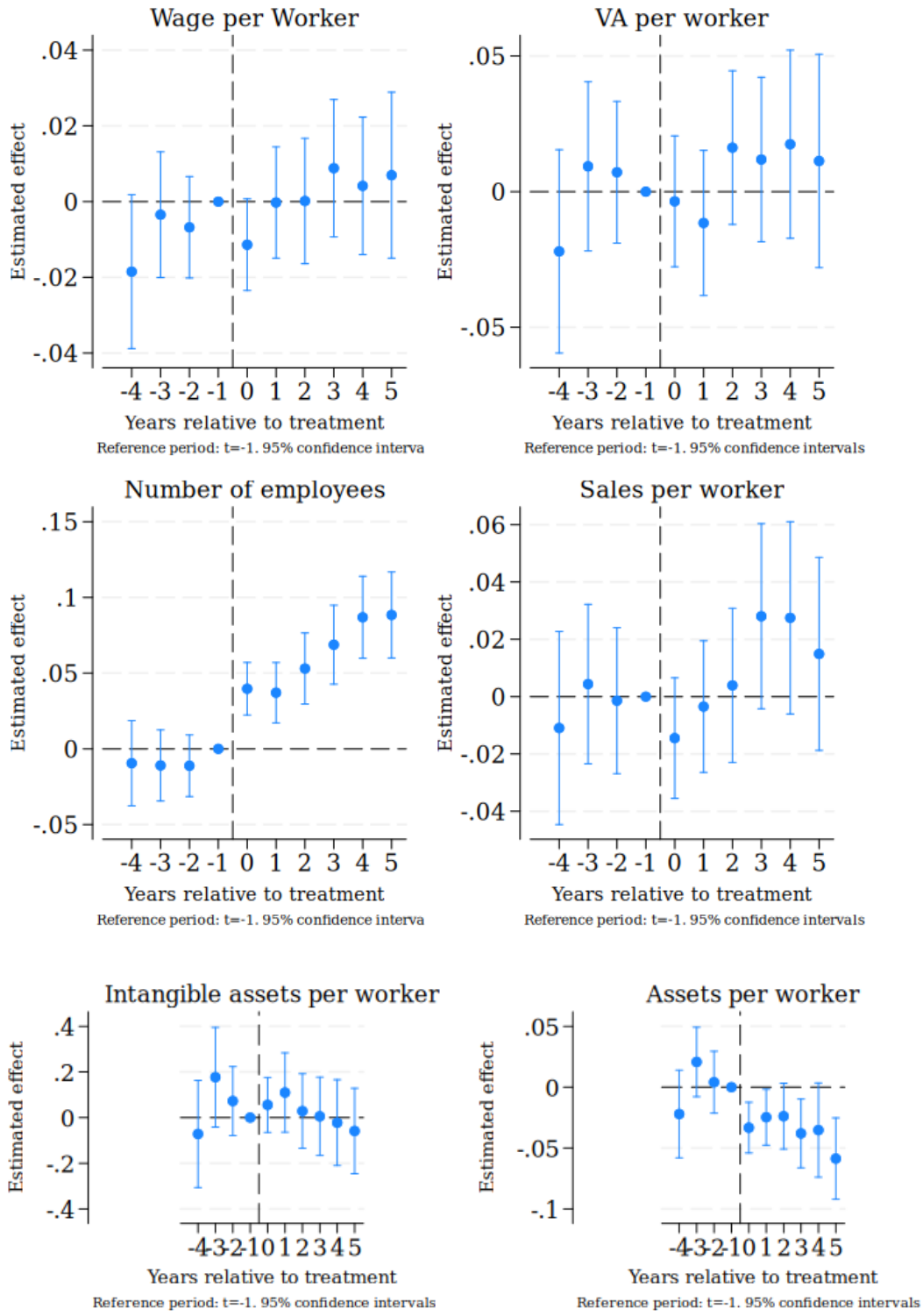
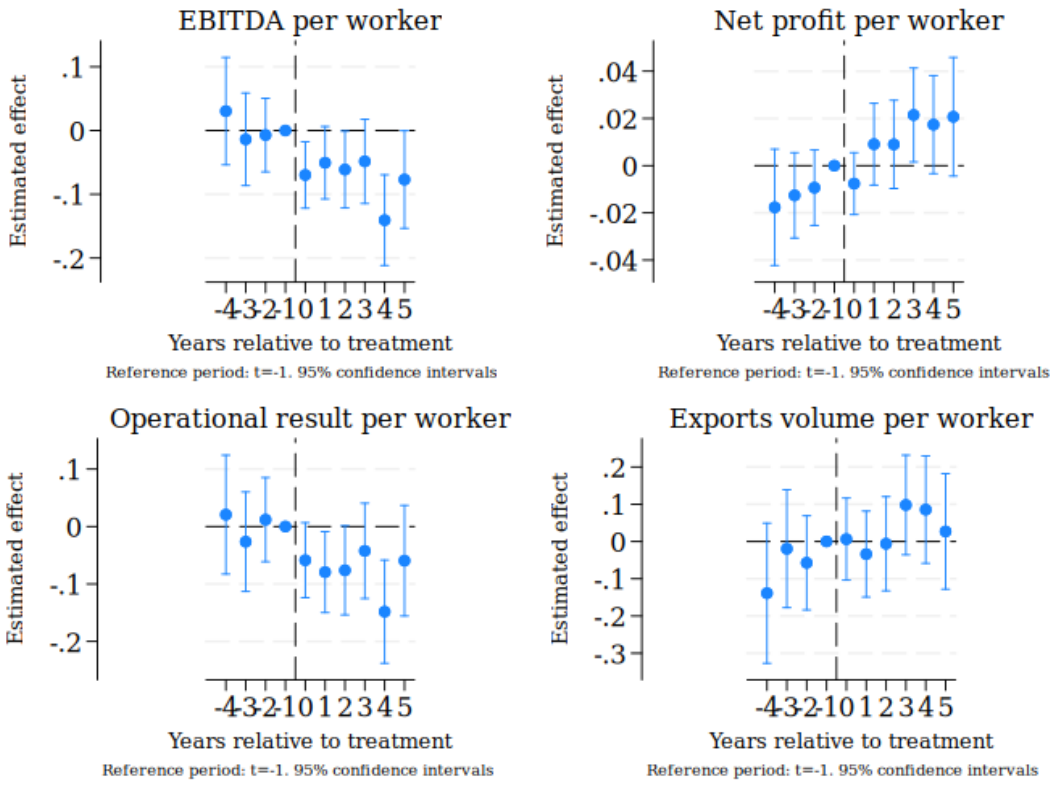


Figure 10. Event-study: effects on medium firms



For medium firms, pre-treatment coefficients are insignificant and close to zero, offering reasonable support for parallel trends. The results follow a similar pattern found for smaller firms. We find a positive association between recruiting through public employment services and the number of employees, demonstrated by the clear upward trajectory of the estimates. This recruitment channel also appears to create some financial pressures for medium firms, reflected in the estimates' decline across post-treatment periods for total assets, EBITDA, and operational result. Net profit turns significantly positive at t=+3. Wages, value added, sales, intangible assets, and export volume show no consistent significant association across the post-treatment periods, with no visible trends. These findings contrast with Lombardi et al. (2018), who observed that medium firms that hired unemployed workers followed an ALMP, registered improvement in profits and value added. One reason could be that medium firms are still adapting their internal processes to efficiently deploy a larger workforce.

Figure 11. Event-study: effects on large firms

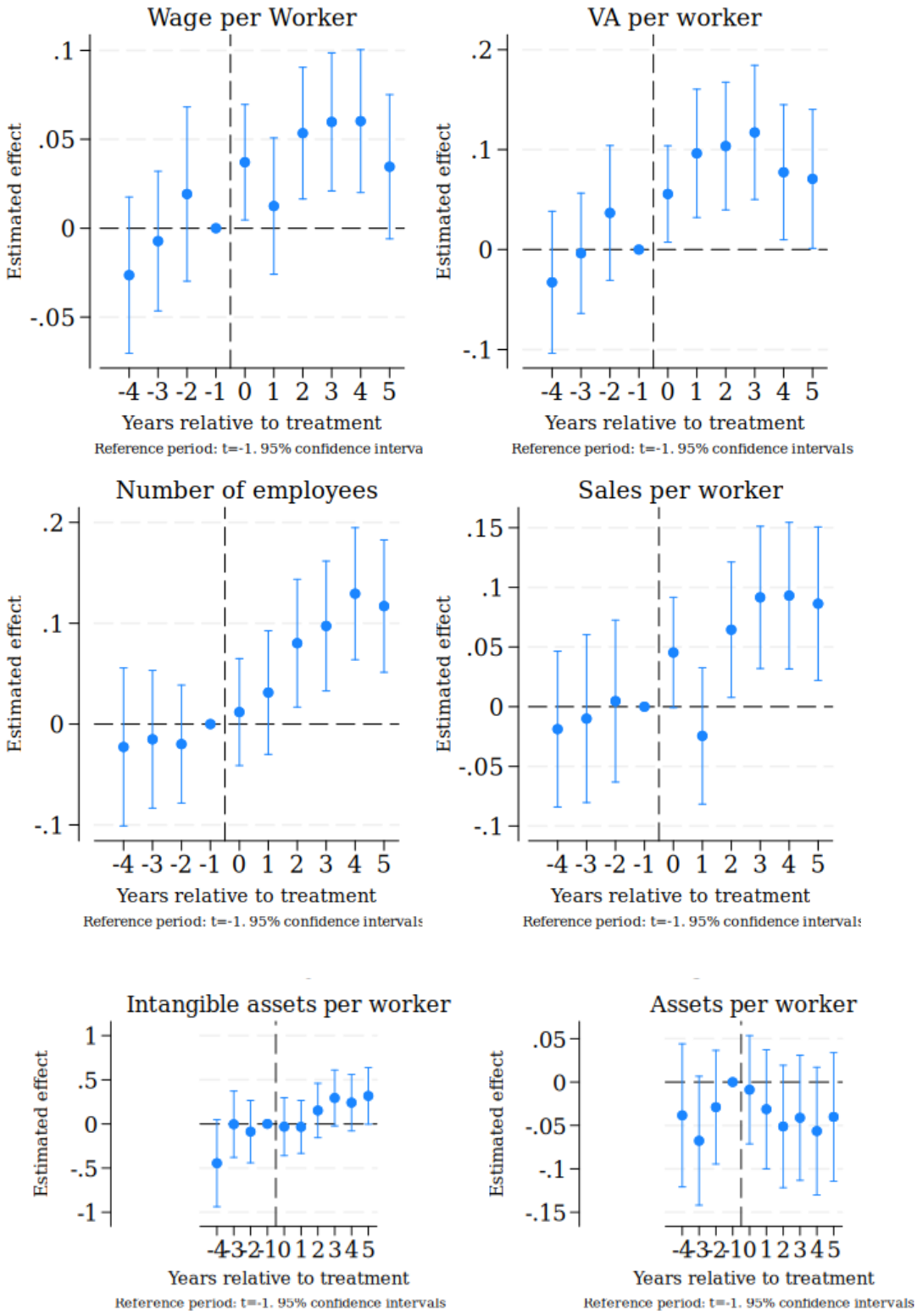
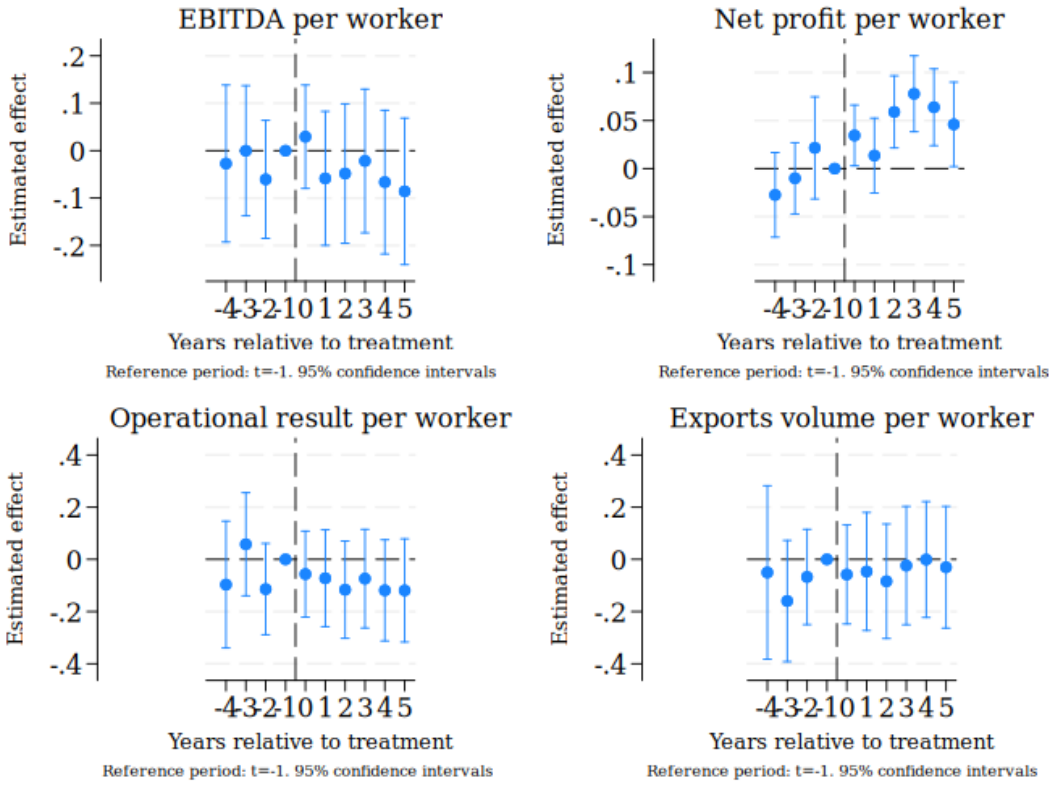


Figure 12. Event-study: effects on large firms



For larger firms, the pre-treatment coefficients also offer reasonable support for parallel trends, as estimates are small and insignificant. In contrast to the previous subsamples, recruiting through IEFP is associated with effects across a different set of outcomes. The graphs show a positive association with wages, value added, number of employees, sales, and net profit. For wages, value added, and net profit, this positive effect emerges at t=0, while for employment and sales, it appears with a two-period delay. All five outcomes display an increasing, persistent trend over the post-treatment horizon, suggesting that recruiting through IEFP is associated with sustained improvements in these measures for larger firms. Intangible assets, total assets, EBITDA, operational result, and export volume show no significant association at any period. Contrary to Blasco and Pertold-Gebicka (2013), who found no significant effects for large firms, our results suggest that they may benefit the most from using IEFP as a recruitment channel. The delayed positive trajectory may be consistent with a learning-by-doing process, whereby workers recruited through IEFP gradually accumulate the firm-specific skills needed to contribute effectively in more complex organizational environments. Given their organizational structure, larger firms may also be better equipped to place IEFP workers in suitable roles and to provide the training needed

to raise their productivity. It is also worth noting that firms, and particularly larger ones, are likely to rely on several recruitment channels simultaneously, using each to target workers with different characteristics. Given IEFP's typical applicant pool, firms using this channel may be specifically seeking workers for roles associated with lower qualification requirements, rather than using it as their primary channel for all types of hiring. Future research could help disentangle how the choice of recruitment channel interacts with the characteristics of the workers hired.

In summary, the dimension analysis reveals that the effects of recruiting through IEFP are highly heterogeneous across firm size categories. Micro firms show broad positive associations between IEFP recruitment and firm performance, including labor costs, productivity, sales, and profitability, though pre-trend concerns require cautious interpretation. Small firms actively use IEFP as a channel for workforce expansion, yet it is also correlated with persistent financial pressure, suggesting that scaling the labor force does not immediately translate into efficiency gains. For medium firms, the associations are more limited, concentrated in employment growth and accompanied by deteriorating financial performance indicators, consistent with an organizational adjustment process that has yet to yield compensating productivity returns. Large firms present the most striking contrast between the static and dynamic estimates, with a delayed but broad-based positive trajectory suggesting that organizational capacity may play a decisive role in whether firms can effectively leverage IEFP recruitment.

4.1.3. Impacts of overqualification

Figure 13 presents the average share of overqualified workers and of workers matched at the required level among individuals hired through IEFP job offers, over the period 2013–2022. The share of overqualified workers remains relatively stable throughout the period, fluctuating between 16% and 20%, while the share of workers matched at the required level hovers around 50%. This pattern shows that, persistently, roughly one in five job offers filled through IEFP is occupied by an overqualified worker, showing no indication of decline. This suggests that overqualification occurs even when a public employment service intermediates the match, demonstrating the relevance of studying its consequences for firm performance in this specific institutional context.

Figura 13. Evaluating the magnitude of mismatch

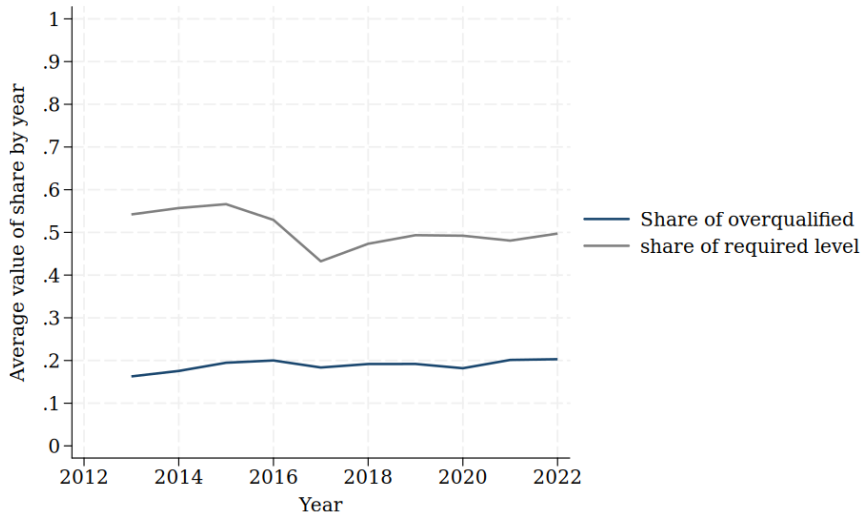


Table 5 presents the results of specification (3.3.), examining whether hiring overqualified workers is associated with differential firm outcomes beteen treated and control firms. Given the three sets of interaction, the parallel trends assumption cannot be assessed by solely inspecting the significance of pre-treatment coefficients. Formal tests of the combined pre-treatment effects support this assumption. As robustness check, the specification was re-estimated using the cumulative number of vacancies posted, rather than those filled with overqualified workers, and the results are similar to those reported here (Table B.1., Appendix).

The coefficients of the the triple interaction, are small and largely insignificant across outcomes. Since the dependent variables are expressed in logarithms, coefficients approximate percentage changes relative to the baseline period t=-5. Average wages show a weak significant negative effect in some of the post-treatment periods. Net profit reveals a small negative significant association with recruiting through IEFP, but negligible in economic terms. For the remaining outcomes, the triple interaction coefficients are small and statistically insignificant at every period. The level term (k x Treated) shows significant pre-treatment coefficients for several variables of interest (value added, employment, total assets, EBITDA) indicating that treated and control firms already diverged relative to t=-5 before the first overqualified hire. In the post-treatment period, this term shows a positive association with employment and net profit, and a negative association with export volume. Overall, recruiting overqualified workers through IEFP, shows no evidence of heterogenous

effects depending on the number of overqualified workers recruited between firms that hired initially a bigger proportion of overqualified and those that did not.

Table 5. Event-study: effects of hiring overqualified workers

	(1) Wages	(2) VA	(3) Number of Employees	(4) Sales	(5) Intangible Assets	(6) Total Assets	(7) EBITDA	(8) Net Profit	(9) Operational Result	(10) Export Volume
-4 x Treated	0.00229	0.02495*	-0.04195***	0.03490***	0.08702	0.05321***	0.11959***	-0.00089	0.09180**	-0.06690
	(0.00836)	(0.01442)	(0.01229)	(0.01348)	(0.07445)	(0.01455)	(0.03056)	(0.00892)	(0.03914)	(0.06279)
-3 x Treated	0.01550**	0.03871***	-0.03171***	0.03437***	0.13174*	0.04071***	0.06121**	0.01157	0.04228	-0.07202
	(0.00769)	(0.01278)	(0.01061)	(0.01239)	(0.06733)	(0.01252)	(0.02810)	(0.00799)	(0.03568)	(0.05780)
-2 x Treated	0.00433	0.01754	0.00719	0.02055*	0.06085	0.01931*	0.04625*	0.00367	0.01714	-0.07583
	(0.00677)	(0.01136)	(0.00947)	(0.01115)	(0.06199)	(0.01088)	(0.02398)	(0.00717)	(0.03077)	(0.04757)
-1 x Treated	-0.00404	0.00276	0.04876***	0.00673	0.00532	-0.00727	0.02407	0.00106	0.01513	-0.07620*
	(0.00589)	(0.01027)	(0.00801)	(0.01003)	(0.05666)	(0.00928)	(0.02129)	(0.00629)	(0.02640)	(0.03939)
0 x Treated	0.00331	0.01053	0.03141***	0.02328***	-0.03700	0.00137	0.02834	0.01079**	-0.01251	-0.07472**
	(0.00508)	(0.00871)	(0.00682)	(0.00883)	(0.04788)	(0.00797)	(0.01801)	(0.00536)	(0.02296)	(0.03374)
1 x Treated	0.00267	0.01219	0.02419***	0.01177	-0.04453	-0.00162	0.02352	0.01111**	0.00605	-0.07751**
	(0.00498)	(0.00839)	(0.00643)	(0.00843)	(0.04601)	(0.00757)	(0.01750)	(0.00535)	(0.02221)	(0.03197)
2 x Treated	-0.00123	0.00511	0.01002	0.00823	0.04288	0.00262	0.02320	0.00696	-0.00037	-0.04488
	(0.00498)	(0.00820)	(0.00621)	(0.00826)	(0.04381)	(0.00739)	(0.01662)	(0.00517)	(0.02105)	(0.03040)
3 x Treated	0.00076	0.00685	-0.00174	0.01126	0.04200	0.01275*	0.03034*	0.00504	0.01563	-0.04954*
	(0.00477)	(0.00782)	(0.00614)	(0.00802)	(0.04023)	(0.00759)	(0.01613)	(0.00502)	(0.02023)	(0.03003)
4 x Treated	0.00394	0.00847	0.00252	0.01256	0.02132	0.00257	0.02616*	0.00760	-0.01381	-0.06053**
	(0.00449)	(0.00762)	(0.00570)	(0.00771)	(0.03541)	(0.00653)	(0.01523)	(0.00473)	(0.01962)	(0.02783)
-4 x Treat. x Hires	-0.02447	-0.00944	-0.04814	0.08220*	0.62505**	0.08538	0.27125*	-0.03307	0.27624**	0.05970
	(0.02454)	(0.03260)	(0.05824)	(0.04601)	(0.26574)	(0.07897)	(0.15997)	(0.02832)	(0.12982)	(0.10911)
-3 x Treat. x Hires	-0.01965**	-0.05120	0.01378	-0.02727	0.06762	0.01430	0.04899	-0.01786*	-0.02125	-0.11448
	(0.00943)	(0.03269)	(0.02001)	(0.02684)	(0.11527)	(0.02323)	(0.11447)	(0.01000)	(0.12008)	(0.11906)
-2 x Treat. x Hires	-0.00232	-0.01579	-0.01436*	0.01262*	0.01613	0.00940	-0.02978	-0.00276	-0.01451	-0.04393
	(0.00617)	(0.01011)	(0.00784)	(0.00728)	(0.04541)	(0.00732)	(0.03141)	(0.00652)	(0.03845)	(0.05266)
-1 x Treat. x Hires	-0.00028	-0.00782	-0.00696	-0.00087	0.01072	-0.00201	-0.02206	-0.00088	0.00216	-0.06058*
	(0.00397)	(0.00653)	(0.00454)	(0.00513)	(0.02870)	(0.00675)	(0.01524)	(0.00385)	(0.01970)	(0.03347)
0 x Treat. x Hires	-0.00014	-0.00006	0.00002	-0.00021	-0.00039	0.00010	0.00029	-0.00018	0.00012	-0.00041
	(0.00010)	(0.00017)	(0.00061)	(0.00019)	(0.00149)	(0.00053)	(0.00072)	(0.00011)	(0.00076)	(0.00162)
1 x Treat. x Hires	-0.00012*	-0.00006	0.00013	0.00000	-0.00077	-0.00002	-0.00036	-0.00016**	-0.00051	0.00053
	(0.00007)	(0.00012)	(0.00038)	(0.00017)	(0.00100)	(0.00032)	(0.00059)	(0.00008)	(0.00075)	(0.00100)
2 x Treat. x Hires	-0.00006	0.00001	0.00015	0.00003	-0.00055	-0.00010	0.00027	-0.00010	0.00046	0.00018
	(0.00007)	(0.00009)	(0.00029)	(0.00015)	(0.00075)	(0.00024)	(0.00037)	(0.00006)	(0.00051)	(0.00077)
3 x Treat. x Hires	-0.00010*	-0.00010	0.00007	-0.00003	0.00002	-0.00002	-0.00004	-0.00010*	-0.00045	0.00060
	(0.00005)	(0.00008)	(0.00020)	(0.00008)	(0.00055)	(0.00018)	(0.00029)	(0.00005)	(0.00032)	(0.00049)
4 x Treat. x Hires	-0.00005	-0.00007	0.00004	0.00002	-0.00004	0.00003	0.00015	-0.00008**	0.00044	0.00053
	(0.00003)	(0.00007)	(0.00016)	(0.00007)	(0.00038)	(0.00013)	(0.00027)	(0.00004)	(0.00053)	(0.00039)
5 x Treat. x Hires	-0.00006*	0.00001	0.00008	0.00002	0.00065	0.00014	0.00025	-0.00008**	0.00022	0.00047
	(0.00003)	(0.00005)	(0.00014)	(0.00006)	(0.00058)	(0.00012)	(0.00021)	(0.00004)	(0.00033)	(0.00034)
Base Period	t=-5	t=-5	t=-5	t=-5	t=-5	t=-5	t=-5	t=-5	t=-5	t=-5
Lag DV	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	140,753	138,031	141,014	140,280	52,153	140,953	117,455	139,793	108,521	63,216
Adjusted R^2	0.805	0.746	0.933	0.904	0.852	0.933	0.770	0.790	0.721	0.851

Notes: All dependent variables are expressed on a per-worker basis. Standard errors clustered at the firm level are reported in parentheses. All specifications include firm and year fixed effects and the lagged dependent variable as a control. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. The k × Offers panel is omitted.

Some explanation may account for this pattern of results. First, most instances of overqualification occur involuntarily (McGuinness, 2006), which may generate job dissatisfaction and lower effort (Liu et al., 2015; Song et al., 2026), limiting any productivity gains for the firm. Second, workers recruited through IEFP may have faced broader labor

market difficulties, meaning their formal overqualification may not reflect real overqualification (Allen & van der Velden, 2001; Green & McIntosh, 2007), with their actual skill level may matching their job requirements, leaving no genuine surplus of skills to translate into performance gains. This would understate the true number of overqualified individuals in the sample and could explain the absence of results, consistent with McGowan and Andrews (2015), who found significant effects only once a measure of overskilling was incorporated. Third, firms may use overqualified hires primarily to scale operations, rather than giving opportunities to use this additional human capital, limiting the potential gains (Song et al., 2026). Fourth, since treatment is defined by the share of overqualified hires only in the firm's first use of IEFP, both treated and control firms can accumulate overqualified workers in later years, blurring the distinction between groups over time. This contamination would attenuate any differential effect and could explain the null result for the triple interaction. Finally, as Crook et al. (2011) suggest, the post-treatment window may simply be too short for these effects to materialize, since some firms may have only three post-treatment periods, the minimum given the sample restrictions.

5. Conclusion

This dissertation studies two related questions about the firm-level consequences of recruiting through Portugal's public employment services. The first concerned the role of IEFP on firm growth, whether firms through the several IEFP interventions perform differently from those that do not. The second concerns the role of overqualified workers, whether hiring individuals whose qualifications surpass the ones demanded by the job they were hired for has measurable consequences for firm performance.

Drawing on a sample of medium and large firms that use IEFP as a recruitment channel, we empoy a difference-in-differences and event-study design to compare firms with at least on filled vacancy, and that have hired more overqualified workers, through this channel to those thatwere unable to hire any worker through it. The results suggest that recruiting through IEFP is, overall, associated with persistent employment growth and gradual improvements in labor productivity and wages, consistent with labor market intermediation facilitating employment. Positive effects are also observed for sales and net profit per worker. However, is also accompanied by the deterioration of asset efficiency and other profitability measures such as operational result and EBITDA per worker, raising questions about whether the financial cost associated with recruiting via IEFP is compensated by productivity gains in the long-term. The heterogeneity analysis showed that larger firms seem to benefit the most from hiring through these services, exhibiting a delayed but broad positive trajectory for most outcomes, while smaller firms exhibit a consistent increase in the number of employees with detrimental effects for most profit measures. Hiring overqualified workers has no heterogenous effects between treated and control firms, only showing some marginal effects on employment, sales, net profit and export volume.

5.1. Limitations and future research

This study is subject to several limitations that should be considered when interpreting the results. Our measure of education mismatch is based solely on comparing individuals' level of education with the minimum level required by the job offers, abstracting from other workers skills that may affet job suitability and potentially leading to misleading results (McGuiness and Wooden, 2009). Secondly, although we use FE to control for invariant firms observed and non-observed chaaracteristics, we are unable to use the workers characteristics which may be reflected in our results. Third, while the empirical strategy drew on differences-

in-differences and event-study designs, the results are reported as conditional associations rather than causal estimates. The staggered timing of IEFP recruitment means that the two-way fixed effects estimator combines comparisons across multiple treatment-timing cohorts, introducing bias when already-treated cohorts can serve as controls for later-treated firms and treatment effects evolve over time (Goodman-Bacon, 2021; Sun & Abraham, 2021). Heterogeneity robust estimators, such as those proposed by Callaway and Sant'Anna (2021), are an important addition for future research. Fourth, for both parts of the study, the treatment is constructed based on the first year of hiring, which may not accurately reflect the ongoing qualifications profile of IEFP hires and IEFP overqualified hires in subsequent years, which raises concerns about the contamination of the control groups identified. Finally, we restrict the study to medium and large firms with ten or more employees, limiting the extension of the findings since micro and small firms constitute the vast majority of the Portuguese firms, although some exploratory evidence is provided in the heterogeneity analysis.

Various venues for future research emerge from this study. The main suggestion, is the utilization of a heterogeneity-robust estimator more adequate for staggered treatment. Second, incorporating workers' characteristics such as their qualification level, unemployment duration, or prior work experience could allow a more precise examination of the mechanisms through which PES recruitment affects firm outcomes, and in particular whether match quality at the worker level mediates the firm-level effects documented here. Finally, while during our time horizon some measures incentivizing registration by graduates have been implemented, for example, Professional Interships and *Ativar.pt*, more recent policies, namely *Emprego+Talento,* implemented in 2024, could help increase the sample of highly qualified workers in the sample, and subsequently potentially overqualified workers, warranting a new analysis. Future research could exploit these measures as exogenous shocks to obtain a causal interpretation of the results, comparing firms that potentially benefited from the incentives with those that did not, in order to identify the resulting differences and recover causal estimates.

Appendix

Table A 1. Event-study estimates: initial sample

	(1) Wages	(2) VA	(3) Number of Employees	(4) Sales	(5) Intangible Assets	(6) Total Assets	(7) EBITDA	(8) Net Profit	(9) Operational Result	(10) Exports Volume
t=-4	0.006	0.007	-0.066***	0.025***	-0.049	0.036***	0.031	-0.009	0.006	-0.087
	(0.006)	(0.011)	(0.007)	(0.009)	(0.054)	(0.010)	(0.022)	(0.008)	(0.028)	(0.056)
t=-3	0.016***	0.029***	-0.057***	0.034***	0.041	0.050***	0.008	-0.000	0.015	-0.025
	(0.005)	(0.009)	(0.006)	(0.008)	(0.049)	(0.008)	(0.019)	(0.007)	(0.023)	(0.047)
t=-2	0.013***	0.021**	-0.037***	0.034***	0.011	0.031***	-0.002	0.007	-0.003	-0.030
	(0.004)	(0.008)	(0.005)	(0.007)	(0.039)	(0.007)	(0.016)	(0.006)	(0.020)	(0.037)
t=0	0.012***	0.002	-0.010**	-0.014**	0.018	0.001	-0.040***	0.039***	-0.048***	-0.048
	(0.004)	(0.007)	(0.005)	(0.006)	(0.034)	(0.007)	(0.014)	(0.006)	(0.017)	(0.034)
t=+1	0.021***	0.016**	-0.020***	0.020***	0.041	0.032***	-0.011	0.031***	-0.038**	-0.002
	(0.005)	(0.008)	(0.005)	(0.007)	(0.040)	(0.007)	(0.015)	(0.006)	(0.019)	(0.036)
t=+2	0.033***	0.027***	-0.001	0.057***	0.087**	0.043***	-0.014	0.043***	-0.034*	0.071*
	(0.005)	(0.009)	(0.005)	(0.008)	(0.041)	(0.007)	(0.016)	(0.007)	(0.020)	(0.039)
t=+3	0.039***	0.034***	0.009*	0.075***	0.078*	0.046***	0.008	0.046***	-0.019	0.077*
	(0.005)	(0.009)	(0.005)	(0.008)	(0.042)	(0.008)	(0.017)	(0.007)	(0.022)	(0.041)
t=+4	0.037***	0.031***	0.015***	0.080***	0.089*	0.043***	-0.025	0.044***	-0.042*	0.098**
	(0.006)	(0.010)	(0.006)	(0.009)	(0.047)	(0.009)	(0.018)	(0.007)	(0.023)	(0.043)
t=+5	0.033***	0.036***	0.021***	0.089***	0.069	0.044***	-0.035*	0.037***	-0.031	0.109**
	(0.006)	(0.011)	(0.006)	(0.009)	(0.047)	(0.009)	(0.019)	(0.008)	(0.024)	(0.047)
Base Period	t=-1	t=-1	t=-1	t=-1	t=-1	t=-1	t=-1	t=-1	t=-1	t=-1
Lag DV	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	449,785	426,643	453,856	447,711	104,844	453,084	332,585	429,667	304,667	120,016
Adjusted R^2	0.722	0.670	0.942	0.849	0.874	0.900	0.697	0.736	0.656	0.828

Notes: All dependent variables are expressed on a per-worker basis. Standard errors clustered at the firm level are reported in parentheses. All specifications include firm and year fixed effects and the lagged dependent variable as a control. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A 2. Event-study estimates for firms with more than five employees

	(1) Wages	(2) VA	(3) Number of Employees	(4) Sales	(5) Intangible Assets	(6) Total Assets	(7) EBITDA	(8) Net Profit	(9) Operational Result	(10) Export Volume
t=-4	-0.015**	-0.015	-0.005	-0.014	-0.126	-0.022*	0.016	-0.015	-0.021	-0.053
	(0.008)	(0.014)	(0.010)	(0.012)	(0.082)	(0.013)	(0.030)	(0.009)	(0.037)	(0.072)
t=-3	-0.006	0.011	-0.008	-0.003	0.066	0.007	-0.012	-0.006	-0.004	-0.018
	(0.006)	(0.012)	(0.008)	(0.010)	(0.072)	(0.011)	(0.026)	(0.007)	(0.032)	(0.058)
t=-2	-0.008*	0.001	-0.001	-0.001	0.021	-0.011	-0.035	-0.007	-0.033	-0.046
	(0.005)	(0.010)	(0.007)	(0.009)	(0.056)	(0.010)	(0.021)	(0.006)	(0.027)	(0.046)
t=0	-0.009**	-0.008	0.053***	-0.016**	0.047	-0.031***	-0.062***	-0.001	-0.076***	-0.022
	(0.005)	(0.009)	(0.006)	(0.008)	(0.046)	(0.008)	(0.019)	(0.005)	(0.024)	(0.041)
t=+1	-0.009*	-0.011	0.042***	-0.029***	0.045	-0.033***	-0.074***	0.004	-0.100***	0.007
	(0.006)	(0.010)	(0.007)	(0.009)	(0.056)	(0.009)	(0.020)	(0.007)	(0.026)	(0.044)
t=+2	0.004	0.002	0.065***	0.004	0.095	-0.040***	-0.068***	0.017**	-0.101***	0.061
	(0.006)	(0.011)	(0.008)	(0.009)	(0.058)	(0.010)	(0.022)	(0.007)	(0.028)	(0.047)
t=+3	0.018***	0.013	0.081***	0.024**	0.115*	-0.038***	-0.058**	0.032***	-0.104***	0.100**
	(0.007)	(0.011)	(0.008)	(0.010)	(0.059)	(0.011)	(0.023)	(0.008)	(0.029)	(0.050)
t=+4	0.021***	0.009	0.094***	0.039***	0.077	-0.039***	-0.095***	0.033***	-0.142***	0.078
	(0.007)	(0.012)	(0.008)	(0.011)	(0.064)	(0.012)	(0.024)	(0.008)	(0.031)	(0.051)
t=+5	0.010	0.006	0.098***	0.031***	0.071	-0.042***	-0.076***	0.026***	-0.097***	0.074
	(0.007)	(0.013)	(0.009)	(0.012)	(0.063)	(0.011)	(0.025)	(0.009)	(0.032)	(0.055)
Base Period	t=-1	t=-1	t=-1	t=-1	t=-1	t=-1	t=-1	t=-1	t=-1	t=-1
Lag DV	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	262,463	254,496	263,451	261,365	77,252	263,285	208,951	258,423	192,532	92,625
Adjusted R^2	0.772	0.716	0.939	0.881	0.867	0.919	0.738	0.762	0.692	0.839

Notes: We lower the threshold for the number of employees in the period before the treatment to 5. All dependent variables are expressed on a per-worker basis. Standard errors clustered at the firm level are reported in parentheses. All specifications include firm and year fixed effects and the lagged dependent variable as a control. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A 3. Robustness test overqualification study by using the total offers

	(1) Wages	(2) VA	(3) Number of Employees	(4) Sales	(5) Intangible Assets	(6) Total Assets	(7) EBITDA	(8) Net Profit	(9) Operational Result	(10) Exports Volume
-4 x Treated	0.00277	0.02499*	-0.04142***	0.03462**	0.08551	0.05209***	0.11923***	-0.00050	0.09174**	-0.06773
	(0.00838)	(0.01447)	(0.01230)	(0.01354)	(0.07488)	(0.01457)	(0.03065)	(0.00895)	(0.03925)	(0.06326)
-3 x Treated	0.01557**	0.03839***	-0.03189***	0.03384***	0.12359*	0.04061***	0.06097**	0.01168	0.04317	-0.07256
	(0.00774)	(0.01283)	(0.01066)	(0.01243)	(0.06738)	(0.01258)	(0.02821)	(0.00805)	(0.03583)	(0.05832)
-2 x Treated	0.00446	0.01736	0.00715	0.02050*	0.05212	0.01907*	0.04798**	0.00376	0.02010	-0.07869
	(0.00683)	(0.01142)	(0.00955)	(0.01121)	(0.06247)	(0.01096)	(0.02414)	(0.00723)	(0.03099)	(0.04825)
-1 x Treated	-0.00393	0.00358	0.04927***	0.00738	-0.00397	-0.00756	0.02568	0.00116	0.01927	-0.08099**
	(0.00593)	(0.01037)	(0.00805)	(0.01010)	(0.05705)	(0.00934)	(0.02141)	(0.00633)	(0.02662)	(0.03961)
0 x Treated	0.00339	0.01026	0.03175***	0.02338***	-0.03758	0.00141	0.02815	0.01095**	-0.01212	-0.07382**
	(0.00509)	(0.00873)	(0.00685)	(0.00885)	(0.04803)	(0.00799)	(0.01805)	(0.00537)	(0.02302)	(0.03389)
1 x Treated	0.00274	0.01206	0.02435***	0.01189	-0.04459	-0.00158	0.02357	0.01124**	0.00654	-0.07650**
	(0.00499)	(0.00841)	(0.00645)	(0.00845)	(0.04615)	(0.00759)	(0.01752)	(0.00537)	(0.02225)	(0.03208)
2 x Treated	-0.00117	0.00495	0.00994	0.00828	0.04368	0.00280	0.02318	0.00703	-0.00008	-0.04462
	(0.00499)	(0.00822)	(0.00623)	(0.00828)	(0.04396)	(0.00742)	(0.01664)	(0.00519)	(0.02109)	(0.03055)
3 x Treated	0.00071	0.00679	-0.00173	0.01128	0.04215	0.01291*	0.03072*	0.00498	0.01654	-0.04924
	(0.00478)	(0.00784)	(0.00615)	(0.00803)	(0.04036)	(0.00762)	(0.01616)	(0.00504)	(0.02028)	(0.03013)
4 x Treated	0.00391	0.00846	0.00254	0.01258	0.02162	0.00267	0.02620*	0.00760	-0.01370	-0.06054**
	(0.00450)	(0.00763)	(0.00571)	(0.00772)	(0.03554)	(0.00654)	(0.01525)	(0.00474)	(0.01965)	(0.02795)
-4 x Treat. x Offers	-0.00692	0.00788	-0.02043	0.02930*	0.13330	0.03805	0.10078**	-0.01225	0.11151**	0.04802
	(0.00955)	(0.01388)	(0.01737)	(0.01627)	(0.11860)	(0.02498)	(0.05123)	(0.01013)	(0.04888)	(0.03525)
-3 x Treat. x Offers	-0.00280	-0.00709	0.00781	-0.00228	0.02563	0.00269	0.00980	-0.00213	-0.00765	-0.05085
	(0.00303)	(0.01315)	(0.00607)	(0.00966)	(0.04714)	(0.00710)	(0.02259)	(0.00300)	(0.02555)	(0.04979)
-2 x Treat. x Offers	-0.00143	-0.00374	-0.00237	0.00339	0.05404*	0.00547	-0.01497	-0.00036	-0.01483	-0.00239
	(0.00257)	(0.00454)	(0.00344)	(0.00335)	(0.03114)	(0.00390)	(0.01449)	(0.00252)	(0.01707)	(0.02298)
-1 x Treat. x Offers	-0.00073	-0.00376	-0.00266	-0.00200	0.01554	-0.00152	-0.00887*	-0.00083	-0.01238	-0.00210
	(0.00156)	(0.00271)	(0.00165)	(0.00183)	(0.01197)	(0.00243)	(0.00503)	(0.00162)	(0.00779)	(0.00922)
0 x Treat. x Offers	-0.00008	-0.00005	-0.00001	-0.00015	-0.00018	0.00010	0.00019	-0.00015**	0.00005	-0.00043
	(0.00006)	(0.00010)	(0.00041)	(0.00010)	(0.00067)	(0.00031)	(0.00040)	(0.00007)	(0.00042)	(0.00061)
1 x Treat. x Offers	-0.00008*	-0.00005	0.00007	-0.00002	-0.00040	0.00002	-0.00019	-0.00013***	-0.00031	0.00012
	(0.00004)	(0.00006)	(0.00026)	(0.00010)	(0.00046)	(0.00019)	(0.00030)	(0.00004)	(0.00042)	(0.00039)
2 x Treat. x Offers	-0.00004	0.00000	0.00010	0.00001	-0.00038	-0.00004	0.00016	-0.00008**	0.00023	0.00008
	(0.00004)	(0.00006)	(0.00021)	(0.00009)	(0.00037)	(0.00015)	(0.00020)	(0.00004)	(0.00029)	(0.00034)
3 x Treat. x Offers	-0.00005*	-0.00006	0.00004	-0.00002	-0.00001	0.00000	-0.00006	-0.00006**	-0.00034*	0.00029
	(0.00003)	(0.00004)	(0.00014)	(0.00004)	(0.00025)	(0.00011)	(0.00016)	(0.00003)	(0.00020)	(0.00022)
4 x Treat. x Offers	-0.00003	-0.00004	0.00001	0.00001	-0.00003	0.00004	0.00010	-0.00006**	0.00022	0.00028
	(0.00002)	(0.00004)	(0.00011)	(0.00004)	(0.00019)	(0.00008)	(0.00015)	(0.00002)	(0.00030)	(0.00019)
5 x Treat. x Offers	-0.00004**	-0.00000	0.00004	0.00001	0.00036	0.00009	0.00014	-0.00006***	0.00010	0.00024
	(0.00002)	(0.00003)	(0.00010)	(0.00003)	(0.00034)	(0.00008)	(0.00012)	(0.00002)	(0.00019)	(0.00018)
Base Period	t=-5	t=-5	t=-5	t=-5	t=-5	t=-5	t=-5	t=-5	t=-5	t=-5
Lag DV	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	140,753	138,031	141,014	140,280	52,153	140,953	117,455	139,793	108,521	63,216
Adjusted R^2	0.805	0.746	0.933	0.904	0.852	0.933	0.770	0.790	0.721	0.851

Notes: All dependent variables are expressed on a per-worker basis. Standard errors clustered at the firm level are reported in parentheses. All specifications include firm and year fixed effects and the lagged dependent variable as a control. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively. The k x Offers is omitted.

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Annex

Figure B 1. Evolution of outcomes variables: treated vs control group

