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The Impact of Restricting Fixed-Term Contracts on Labor and Skill Demand

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The Impact of Restricting Fixed-Term Contracts on Labor and Skill Demand

Abstract

This paper examines the impact of increasing the relative cost of fixed-term contracts on labor demand as well as the demand for standard measures of human capital and specific skill requirements. We evaluate a 2018 Italian labor law reform that raised the cost of fixed-term contracts while keeping permanent contract costs unchanged. We employ a difference-in-differences research design, leveraging the variation in firms' exposure to the reform resulting from their diverse reliance on fixed-term contracts due to differing reactions to earlier labor market reforms. Using rich data covering the near universe of online job vacancies in Italy, our findings indicate that the increase in hiring costs for temporary contracts led to a decrease in the relative demand for temporary workers and an increase in the demand for permanent workers. This shift in demand was accompanied by upskilling towards workers with higher levels of human capital and specific skill requirements. When offering jobs under permanent contracts, firms increased their demand for workers with a college degree and social skills. At the same time, they reduced their demand for workers with only a high school degree and no work experience. On the other hand, when offering jobs under fixed-term contracts, firms increased their demand for workers with some work experience and social skills. These findings suggest that while restricting fixed-term contracts encouraged the hiring of permanent workers, such reforms might have unintended consequences by raising the hiring standards for job entry, thereby reducing employment opportunities for less qualified workers.

JEL-Codes: J230, J240, J630, K310.

Keywords: hiring costs, employment protection, dual labor markets, skills.

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1 Introduction

Since the 1980s, there has been a noticeable divergence in employment practices across many European countries. This divergence involves the coexistence of fixed-term contracts, providing flexibility to firms in adjusting their workforce’s level and composition, and open-ended contracts, which are subject to stricter firing regulations. As a result of these developments, dual labor markets have emerged, where permanent workers under open-ended contracts enjoy job stability, while temporary workers in the secondary segment under fixed-term contracts face higher employment and earning risks, limited training opportunities, and uncertainty regarding their career prospects (Bentolila et al., 2020; Blanchard and Landier, 2002; Boeri, 2011; Booth et al., 2002). In more recent years, globalization and technological advancements have further exacerbated this divide in employment forms. The emergence of new contract types, such as variable hours contracts and platform work, has contributed to labor market duality, even in countries with low shares of fixed-term contracts (OECD, 2019).¹

To tackle the issue of labor market segmentation, policymakers have introduced reforms in Employment Protection Legislation (EPL) with the intention of finding a balance between providing firms with flexibility and addressing the potential negative consequences for workers’ well-being arising from the uncertainty of these flexible employment arrangements. These reforms include measures such as reducing employment protection for permanent workers by lowering termination costs for open-ended contracts and increasing the costs associated with hiring under fixed-term contracts.

A substantial body of research evaluating these reforms has extensively studied their impact on employment. Specifically, they have examined the effects of: (i) liberalizing fixed-term contracts (e.g., Daruich et al., 2023; García-Pérez et al., 2018); (ii) easing regulations on regular employment through permanent contracts (e.g., Boeri and Garibaldi, 2019; Martins, 2009; Sestito and Viviano, 2018); and (iii) limiting the use of fixed-term contracts (Cahuc et al., 2022; Palladino and Sartori, 2021). While understanding the employment effects of these reforms is crucial, it is essential to consider potential biases caused by general equilibrium effects. This is because changes in EPL that influence employment can also impact unemployment rates and, consequently, the rate at which vacant job positions are filled (Cahuc et al., 2022). Moreover, EPL reforms might also influence the demand for skills, leading to changes not only in the overall level of employment but also in the composition of the workforce. Understanding these adjustments in labor and skill demand is crucial for

¹Throughout the paper, the terms “fixed-term” and “temporary”, and “open-ended” and “permanent” are used interchangeably.

assessing the effectiveness of policy responses and to what extent they can strike a balance between firm flexibility and employment opportunities for the most vulnerable workers they aim to protect.

In this paper, we examine the impact of restricting the use of fixed-term contracts on firms’ labor and skill demand. We exploit a 2018 Italian labor law reform (known as “Decreto Dignità”), which resulted in an increase in the cost of fixed-term contracts while keeping the cost of open-ended contracts unchanged. Our research focuses on evaluating the reform’s effects on labor demand under various contract types, as well as the demand for standard measures of human capital, such as education and experience, and for specific skill requirements. To achieve this, we utilize rich data that covers the near universe of online job vacancies in Italy, allowing us to characterize different skill demands.

To identify the effects of the reform, we employ a difference-in-differences research design, leveraging the variation in firms’ exposure to the reform. This variation is driven by heterogeneous changes in firms’ reliance on fixed-term contracts due to their diverse reactions to earlier labor law reforms. In essence, we compare the changes in labor demand, human capital demand, and skill demand between firms more exposed to the higher costs of hiring workers under fixed-term contracts and their less exposed counterparts. The main concern in this identification strategy is the presence of pre-existing diverging trends in labor and skill demand across firms with varying degrees of exposure to the reform. To address this issue, we use a dynamic specification and demonstrate that there are no differential pre-trends in the outcome variables among the more and less exposed groups. Additionally, we conduct placebo tests and adopt alternative specifications to strengthen the validity of our research design.

With higher hiring costs for temporary contracts, firms may decrease their usage and opt for other flexible employment arrangements like self-employed positions or internships. If this occurs, we would not expect significant changes in skill demand, as firms would retain flexibility without incurring higher screening and labor costs. On the other hand, if other flexible forms of employment are inadequate substitutes for temporary contracts, firms may become more open to increasing their demand for permanent contracts. This could involve converting existing temporary relationships or hiring new employees directly under permanent contracts. In both cases, a greater reliance on permanent contracts would result in higher expected labor costs. Furthermore, stricter conditions to assess temporary workers’ quality before committing to a more stable work relationship entail a higher screening cost. Both these factors may potentially lead firms to adjust by raising skill requirements for workers under both temporary and permanent contracts.

We find that the rise in hiring costs for temporary contracts resulted in an increase in

the relative demand for permanent workers while simultaneously leading to a decrease in the relative demand for temporary workers. These findings indicate a significant level of substitution in firms' demand for permanent over temporary workers, highlighting the reform's success in promoting hiring under permanent contracts.

However, the increase in hiring costs for temporary workers not only influenced the shift from temporary to permanent contracts but also had an impact on the demand for specific skills. Specifically, we find that in job vacancies offering permanent contracts, firms increased their demand for workers with a college degree and social skills, while they reduced their demand for workers with only a high school degree and no work experience. On the other hand, in job vacancies offering fixed-term contracts, firms increased their demand for workers with some work experience and social skills.

These findings suggest that while restricting the use of fixed-term contracts successfully encouraged the hiring of permanent workers, it also resulted in reduced labor market opportunities for individuals with lower levels of education and no prior experience, in favor of more qualified workers, reflecting a broader impact on the composition of the workforce. This indicates that firms seek stronger signals of worker quality when faced with increased screening and labor costs.

This paper is relevant to various strands of the existing literature. Firstly, it contributes to the literature that examines the effects of labor market reforms that alter the EPL provisions of fixed-term contracts. Numerous studies have explored the employment effects of liberalizing (e.g., [Autor and Houseman, 2010](#); [Blanchard and Landier, 2002](#); [Cappellari et al., 2012](#); [Darulich et al., 2023](#); [García-Pérez et al., 2018](#); [Güell and Petrongolo, 2007](#)) and restricting ([Cahuc et al., 2022](#); [Palladino and Sartori, 2021](#)) fixed-term contracts. Our research contributes in two significant ways. First, we provide estimates on the impact of altering the terms of temporary contracts on labor demand, which is less influenced by general equilibrium effects. Second, we offer the first evidence on how reforming fixed-term contracts affects the demand for skills, using detailed measures of both standard human capital requirements (such as education and experience) and specific skill requirements (i.e., cognitive, management, computer, and social skills). While similar studies exist, such as [Cappellari et al. \(2012\)](#), which assesses the impact of easing fixed-term contracts on skill composition defined as the ratio between non-manual and manual workers within a firm, and [Ardito et al. \(2022\)](#), which employs a similar approach at the occupation-sector cell level to evaluate a reform that relaxed employment protection legislation for open-ended contracts using data from the Piedmont Italian region, our use of online vacancy data provides a more comprehensive perspective on the impact of changes in labor market flexibility on the demand for skills, as we can observe specific types of skill requirements mentioned in job postings.

Secondly, this paper is also related to the literature on firms’ hiring strategies and the costs associated with screening workers. Employers value probationary work arrangements as they allow them to assess worker productivity and potential long-term match quality before establishing more stable work relationships. Fixed-term contracts, with their flexible terms and low firing costs, serve as a cost-effective screening tool for firms (Faccini, 2014; Kuhnen and Oyer, 2016; Portugal and Varejão, 2022). In a broader context, screening costs resulting from informational frictions have been shown to influence hiring strategies. For instance, Weinstein (2018) shows that when screening is costly, recruiters prioritize strong signals of worker quality.² Likewise, other studies (Ballance et al., 2020; Shoag and Veuger, 2016) suggest that increased screening costs prompt firms to demand upskilling in terms of education and experience requirements, effectively shifting the responsibility of providing better performance signals to job applicants. Our paper contributes to this area of research by providing evidence of the demand for upskilling by prospective employers in response to higher screening costs associated with fixed-term contracts. Specifically, we demonstrate that as screening through temporary contracts becomes more expensive, firms raise their standards regarding required skills to address uncertainty and protect themselves from adverse selection.

Thirdly, this paper is related to the strand of literature that examines disparities in training opportunities, human capital accumulation, and job content between fixed-term and open-ended contracts (e.g., Bratti et al., 2021; Cabrales et al., 2017). These studies document differences in firm-sponsored on-the-job training in two-tier labor markets, where temporary workers have fewer learning opportunities, leading to training gaps that intensify in countries with higher levels of labor market dualism. Garcia-Louzao et al. (2023) provides empirical evidence that limited training for fixed-term workers hampers their human capital accumulation and, consequently, leads to lower returns to experience compared to permanent workers, as experience gained under temporary contracts is less valuable. Similar gaps are also found in the literature regarding job content. For example, Kahn (2018) argues that more stringent employment protection for permanent workers may induce employers to assign them higher-level skilled tasks, and provides evidence of a cross-country relationship that links larger skill content gaps between permanent and temporary jobs to higher labor market duality. In this paper, we contribute to this body of literature by offering direct evidence, based on within-country variation resulting from a policy reform, on the effects of changing the relative cost of hiring under temporary contracts on the demand for skills for

²Kugler and Saint-Paul (2004) show that, in the presence of adverse selection, high firing costs lead firms to redistribute new employment opportunities from unemployed to employed workers who are less likely to be “lemons” and, therefore, are preferred by firms.

both permanent and temporary hires.

Lastly, this paper is also relevant to a recent and rapidly expanding literature that utilizes highly detailed data from online job vacancies to investigate various labor market issues. These include: (i) variations in job tasks and skill requirements across geographical locations, as well as between and within narrowly defined occupations (Atalay et al., 2023; Deming and Kahn, 2018); (ii) changes in labor and skill demand in response to minimum wage increases (Clemens et al., 2021), computerization (Dillender and Forsythe, 2022), business cycle fluctuations (Modestino et al., 2016; 2020), and severe shocks such as the Great Recession (Hershbein and Kahn, 2018) and the Covid-19 pandemic (Chetty et al., 2020; Forsythe et al., 2020; 2022); (iii) the role of job postings’ semantic content for the process of search and matching (Marinescu and Wolthoff, 2020), also in relation to gender and age discrimination in recruitment (Card et al., 2023; Helleseter et al., 2020; Kuhn et al., 2020; Kuhn and Shen, 2013; 2023); (iv) labor market concentration and its mediating role on the employment effects of minimum wage increases (Azar et al., 2020; 2023); and (v) trends in the demand for AI skills, and the interplay between AI adoption and various firm and labor market outcomes (Acemoglu et al., 2022; Alekseeva et al., 2021; Babina et al., 2022a; 2022b). Our contribution to this body of literature lies in providing evidence on the effects of a policy reform concerning labor contracts on the demand for labor and skills.

The remaining sections of the paper are structured as follows: Section 2 introduces the Italian institutional setting, presenting the reform we study in a quasi-experimental setting, and summarizing the earlier institutional context. Section 3 offers a description of the data sources used in our study. In Section 4, we illustrate the research design. The results of our empirical analysis and robustness checks are presented in Section 5. Finally, Section 6 includes a summary of our findings and concluding remarks.

2 Institutional setting

2.1 The 2018 Italian reform of temporary contracts

In the summer of 2018, the newly formed Italian government enacted a labor market law known as “Decreto Dignità”, aimed at addressing “precarious employment” (Menegotto et al., 2018).³ The law brought significant changes to the cost of temporary contracts while keeping the legal provisions of permanent contracts unchanged. The reform introduced the following modifications to the existing regulatory framework: (i) a reduction in the maximum overall

³*Decree-Law 87/2018* issued in July 2018 and converted into *Law 96/2018* in August. Further details on its roll-out are available in Appendix A.2.

duration of temporary work relationships (from 36 to 24 months), (ii) a decrease in the number of times the termination date can be extended (from 5 to 4 times), (iii) an increase in the social security contribution rate for temporary contract renewals, i.e., non-conversion to a permanent contract (from 1.4 to 1.9% of pre-tax earnings), and (iv) most importantly, the reintroduction of the requirement to specify a “broad” motivation for opting for a fixed-term work relationship (for contracts lasting 12 months or longer). As we will see in the next section, this last point played a crucial role in increasing the relative cost of temporary contracts.

2.2 Brief overview of the Italian employment protection legislation in the 2010s

In this section, we provide a brief overview of the institutional context leading up to the 2018 reform. This context is essential for motivating our identification strategy, as the reforms enacted in the 2010s impacted the gap in EPL between temporary and permanent contracts, as well as the evolution of relative employment levels under these contract types.⁴

After the initial deregulation of temporary contracts around the turn of the millennium, the Italian EPL context remained relatively stable for about a decade. However, starting in 2012, a series of reforms were implemented. Figure 1a displays the timeline of these enactments, indicating the areas they targeted. The top-colored bar above the timeline shows the legislation’s impact on the temporary contract margin, while the bottom bar below represents the effect on the permanent contract margin. Figure 1b illustrates aggregate trends in the relative employment under temporary contracts during the corresponding years. These two figures help identify three distinct periods.

The first period corresponds to the “Fornero labor law”, implemented in June 2012. This reform focused on both margins, tightening the legal provisions of temporary contracts while easing those of permanent contracts by reducing firing costs for larger firms. The objective was to discourage the use of fixed-term contracts while promoting employment under open-ended contracts. Studies evaluating the “Fornero labor law” have reported an increase in permanent hires, particularly among young workers (O’Higgins and Pica, 2020). However, they have also found an increase in permanent worker separations and a decrease in temporary hirings for low-skilled workers (Bottasso et al., 2023). These findings suggest that the reduction in firing costs for permanent workers did not fully compensate for the effects of stricter regulations on temporary contracts. This aligns with the moderate decline in relative employment under temporary contracts observed between 2012 and 2013, reaching its lowest

⁴A more in-depth account of the Italian EPL framework from 1997 until 2017 is provided in Appendix A.1.

point in the decade, as illustrated in Figure 1b.

The second period corresponds to a series of measures collectively known as the “Jobs Act” (JA). The first measure, known as “Decreto Poletti”, was introduced in March 2014 and brought significant changes to the terms of use of temporary contracts. Most notably, it eliminated the mandatory requirement for employers to specify *any* motivation for hiring workers under temporary contracts of *any* duration. This change was unprecedented in Italian labor law. Previously, failure to provide a precise reason for hiring under a temporary contract could lead to potential lawsuits, resulting in the conversion of the contract into a permanent one by the labor court, and considerable costs for the firm. The other two measures of the JA focused on the permanent contract margin. In January 2015, a generous subsidy was introduced to incentivize firms to hire under permanent contracts, which was progressively phased out over the following three years. Then, in March 2015, a new graded security permanent contract was introduced, further easing dismissal regulations for permanent workers, particularly in larger firms, in line with the “Fornero labor law”.

Several studies (Ardito et al., 2023; Boeri and Garibaldi, 2019; Bovini and Viviano, 2018; Sestito and Viviano, 2018) have provided evidence that gross permanent hires and conversions from temporary to permanent contracts saw significant increases due to two main factors: hiring subsidies aimed at all types of firms, which had a more pronounced but short-lived effect mostly driven by smaller firms, and the introduction of the new graded-security permanent contract for larger firms, which had a more gradual impact over time.⁵ However, Di Porto and Tealdi (2022) have shown that the “Decreto Poletti” had a significant negative effect on the probability of conversion to a permanent contract for workers who entered the labor market under the newly introduced more flexible temporary contracts. This resulted in a persistent gap even after the implementation of the other two JA measures.⁶ Overall, the combined medium-term effect of these three measures appears to have had a larger impact on the temporary contract margin, evident in the rapid increase in relative employment under temporary contracts observed between 2013 and 2018 (refer to Figure 1b).

The “Decreto Dignità” policy reform, enacted in 2018, brought a sudden halt to the rising use of temporary contracts. The significant impact of the 2018 reform on the EPL

⁵The effect on permanent hirings and temporary-to-permanent conversions was primarily driven by the hiring subsidies, in contrast to the looser regulations on permanent worker dismissals introduced by the new graded security contract. The impact of the latter is more noticeable at the firing margin, although the effect on permanent worker dismissals was relatively modest and concentrated among larger firms which benefited from it (Boeri and Garibaldi, 2019).

⁶Furthermore, the authors demonstrate that the disparity in the likelihood of conversion between newly hired employees under a more flexible temporary contract and their counterparts who entered with a less flexible one was initially widened by the 2015 hiring subsidy for permanent contracts. They also show that this gap was subsequently diminished by the implementation of the new graded security permanent contract. However, this reduction was not sufficient to completely eliminate the disparity.

gap is evident when observing Figure 2, which displays the temporary-to-permanent ratio of OECD employment protection summary indicators from 2011 to 2019.⁷ Through a substantial and one-sided tightening of temporary contracts, the reform led to a remarkable increase in the temporary-to-permanent indicator ratio, almost doubling from 0.657 to 1.221 in just one year.

Our identification strategy is centered on the notion that, in the lead-up to the “Decreto Dignità”, firms that had grown relatively more reliant on temporary contracts were most exposed to the reform, and thus more affected by it. Consequently, the exposure-to-treatment measure that we construct to identify the reform’s effects, discussed in Section 4, exploits the heterogeneous increase in the propensity among firms operating in different sectors to rely relatively more on temporary contracts for hiring workers.

3 Data

We next describe the two data sources used for the analysis, which provide information on labor demand and skill requirements, and on aggregate stocks of active permanent and temporary work positions.

3.1 Online Job Vacancies Data

The primary data source is a proprietary dataset of online job vacancies (OJVs) obtained from WollyBi, a labor market analytics company of the Burning Glass Technologies (BGT) group.⁸ The dataset comprises information collected from approximately 250 online job boards and employment agencies’ websites through web crawling. The job postings are de-duplicated and transformed into a standardized format suitable for data analysis. The dataset provided to us covers over 6.5 million OJVs from 2014 to 2019, which according to WollyBi captures the near-universe of job vacancies posted online in Italy. To verify the representativeness of the data, we estimated sector-level vacancy rates and compared them with those reported by the Italian National Statistics Office (ISTAT), showing that are reasonably well-aligned (see Figures B.1 and B.2).⁹

⁷The indicators are published in the 2020 version of the Employment Protection Legislation Database by OECD (2020).

⁸WollyBi (www.wollybi.com), previously known as Tabulaex, is powered by Burning Glass Europe, the European division of the Burning Glass Technologies (BGT) group (now Lightcast).

⁹ISTAT defines the vacancy rate as the ratio of the number of posted vacancies to the sum of posted vacancies and filled job positions. Appendix B.1 offers a detailed discussion of the methodology employed to calculate vacancy rates from the WollyBi data and demonstrates the benchmarking exercise conducted to verify the representativeness of the data.

For each online job vacancy (OJV), WollyBi creates approximately 40 standardized fields, including details about the source and date of the job posting, as well as various job characteristics and requirements.¹⁰ Of particular interest are the data on location (at the NUTS-3 level, i.e., province), industry (at the NACE Rev.2 1-digit level), occupation (available up to the ISCO-08 4-digit level), the advertised type of contract, and education and experience requirements.¹¹

For our analysis, we use OJVs posted between the first quarter of 2017 and the fourth quarter of 2019, which accounts for around 4.5 million advertisements. The main estimation sample is limited to job postings with non-missing values for province, industry, and occupation, resulting in a dataset of 3,331,650 ads, constituting 74% of all ads during the specified period.

In addition to the job attributes mentioned earlier, the dataset includes detailed information on skill requirements for each vacancy. Using a proprietary algorithm, WollyBi parsed the text of each OJV and encoded any narrowly defined skills mentioned in the ad into a standardized format; specifically, level 3 of the skills/competences pillar of the ESCO v1 classification.¹² As a result, each vacancy can have none or several of 748 unique standardized English-language text fields such as, for example, “brainstorm ideas”, “perform planning”, “make numerical calculations”, “motivate others”, “manage time”, “ICT debugging tools”, “develop ICT workflow”, and so on.¹³

We use a keyword search routine to map each of these text fields to one of the job skill categories devised by [Deming and Kahn \(2018\)](#). Table 1 displays the keywords and phrases used to derive the 10 job skill categories of their taxonomy, along with the corresponding 4 aggregate job skill categories we develop in this study: cognitive, management, computer, and social skills.

Table 2 presents descriptive statistics on the characteristics and requirements posted in the OJVs, such as the type of contract and, in aggregate terms, education, experience, and

¹⁰We do not observe the specific firms posting the online job vacancies, nor do we have information about the wage offers that may be mentioned in the postings.

¹¹The data contains OJVs from all 107 Italian provinces. We exclude OJVs associated with 4 out of 21 1-digit industry codes that are absent in our secondary dataset (see Section 3.2), i.e., “A - Agriculture, Forestry and Fishing”, “O - Public Administration and Defence; Compulsory Social Security”, “T - Activities of Households as Employers; Undifferentiated Goods- and Services-producing Activities of Households for Own Use” and “U - Activities of Extraterritorial Organizations and Bodies”. For details about the NACE Rev.2 industry taxonomy, see <https://ec.europa.eu/eurostat/ramon/nomenclatures/index.cfm....> Furthermore, we exclude OJVs from 1 out of 10 1-digit occupation codes, i.e., “0 - Armed forces occupations”, of the ISCO-08 occupation taxonomy (<https://www.ilo.org/public/english/bureau/stat/isco/index.htm>).

¹²See https://esco.ec.europa.eu/en/classification/skill_main.

¹³In our main estimation sample, 80.88% of the ads include at least one specific skill requirement, with an average of 8.47 and a median of 6 specific skill requirements among ads that include any.

specific skill requirements.¹⁴ To compute these summary statistics, we follow the approach used by [Hershbein and Kahn \(2018\)](#) and weigh each OJV by the size of the labor force in the province where the ad was posted (fixed at the level of 2017, the last pre-reform year). The table reveals that almost all job postings (95%) provide information on the type of contract offered. In the WollyBi data, we observe four contract types: temporary contracts, which are offered in the majority of vacancies (45% of all OJVs), followed by permanent contracts (24%), self-employed positions (17%), and internships (10%).¹⁵ Nearly all job postings (99.8%) specify an education requirement, with the majority (68%) asking for only up to secondary education (i.e., high school), while the remaining 32% require post-secondary education levels (i.e., college). A slightly lower share of job postings mentions an experience requirement (62%). Out of all the ads in the sample, a significant portion (18%) requires no experience (up to 1 year). Another 33% requires some experience (from 1 to 4 years), while only 11% requires substantial experience (4 years and above). When weighting job ads by the size of the labor force in the province where they were posted, we find that 85% of them report at least one of the four non-mutually exclusive aggregate specific skill requirements. Out of the total number of vacancies, 49% require cognitive skills, 39% management skills, 44% computer skills, and 71% social skills.

Table 3 presents a comparison of job requirement distributions across contract types. The proportion of vacancies stating an education requirement does not vary across contract types. However, job ads for temporary contracts and self-employed positions heavily favor lower levels of education, with approximately 70% demanding up to secondary education. Those offering permanent contracts exhibit a similar but slightly less pronounced pattern, with around 60% requiring up to secondary education. For internships, education requirements are evenly distributed. Regarding stated experience requirements, there is a difference of about 4-7 percentage points between permanent contracts and self-employed positions (about 59%) compared to temporary contracts and internships (63-66%). Permanent contract job ads tend to post higher experience requirements, with 16% asking for 4 or more years of experience, as opposed to 12% and 31% requiring 0-1 and 1-4 years, re-

¹⁴Similar descriptive statistics obtained from detailed job requirements are presented in Tables B.1 and B.2.

¹⁵In the Italian legal framework, permanent contracts also include apprenticeships, which are specific permanent contracts designed for youth training and employment. In our dataset, we are unable to distinguish them from the rest of permanent contracts. However, their occurrence is likely to be minimal, as they constitute only around 3-4% of permanent employment [INPS \(2022\)](#). Additionally, unlike apprenticeships, which involve formal employment contracts, internships involve short-term training programs (up to 6 months) in collaboration with a firm. These programs are governed by training projects jointly developed with relevant schools or universities. Lastly, self-employed workers are independent individuals who conduct their profession autonomously, without being subordinate to any employer. Instead, they offer their services to one or multiple clients as independent contractors.

spectively. In contrast, internships ask for lower experience levels, with 40% requiring 0-1 years of experience, compared to 17% and 7% requiring 1-4 and 4 or more years, respectively. Temporary contracts and self-employed positions lie in-between: 17% of temporary contracts require 0-1 years of experience, 41% require 1-4 years, and 8% require 4 or more years. For self-employed positions, the corresponding shares are 20%, 27%, and 12%. Lastly, job ads for permanent contracts and internships have at least one specific skill requirement at rates higher than 90%, while this occurs at lower rates for self-employed positions (88%) and temporary contracts (80%). This pattern remains consistent when examining each macro job skill category. Furthermore, the ranking of specific skill requirements seen in Table 2 remains unchanged across contract types.

3.2 ASIA dataset

To create the exposure-to-treatment measure as described in Section 4, we utilize aggregate data derived from the Italian Statistical Register of Active Enterprises (ASIA).¹⁶ This dataset, maintained by the Italian National Institute of Statistics (ISTAT), covers all Italian firms and combines various administrative sources such as social security, tax authority, and chamber of commerce data.¹⁷ Among its comprehensive information, the dataset includes detailed data on firms' stocks of active work positions for both employees and non-employees (such as self-employed workers and external personnel) within a specific period.

For our study, it is crucial that the ASIA dataset distinguishes between employees on temporary contracts and those on permanent contracts. While we do not have access to the entire dataset, ISTAT provided us with yearly aggregate data on the total stock of (i) temporary employees, (ii) permanent employees, and (iii) other non-employees, broken down by province (NUTS-3) and NACE Rev.2 1-digit sector for the years 2011 to 2017. We use this information to calculate the nationwide sector-level relative employment annual growth rates in 2017 and 2013, as well as the sector shares of total province-level employment in 2011.

¹⁶This does not include information concerning the 1-digit industry codes A, O, T, and U of the NACE Rev.2 industry classification, as this data was not accessible to us.

¹⁷For further details, see [Consalvi et al. \(2008\)](#).

4 Research Design

4.1 Measuring exposure to the reform

The aim of this analysis is to estimate the causal effect of the increased cost of hiring under temporary contracts, resulting from the 2018 Italian labor reform known as “Decreto Dignità”, on labor demand and skill requirements. As the 2018 reform applied to all firms in Italy, we use a measure of exposure-to-treatment that leverages variations in relative employment under temporary contracts (compared to permanent contracts) across sectors and provinces over time.¹⁸ We adopt the approach suggested by [Hershbein and Kahn \(2018\)](#) and adapt it to the Italian context, where two previous labor law reforms led to a decline and subsequent significant rise in relative employment under temporary contracts.¹⁹

As explained in detail in Section 2.2, the first reform we consider is the “Fornero labor law” of 2012. This reform simultaneously restricted the use of temporary contracts while easing regulations for permanent ones, resulting in a decrease in relative employment under temporary contracts. We designate 2013 as the *trough* year, as it marked the lowest point in relative employment following the implementation of the “Fornero labor law”. The second reform is the “Jobs Act” of 2014-2015. This reform first relaxed regulations for temporary contracts and later for permanent contracts, leading to a significant increase in relative employment under temporary contracts. This upward trend continued until 2018, the year the “Decreto Dignità” reform was introduced. Therefore, we define 2017 as the *peak* year, which was the last year before the 2018 reform.

We calculate the nationwide sector-level annual growth rates of relative employment at the trough and peak years and project them at the province level using predetermined province-specific sectoral employment shares. This process is illustrated in Equation (1) below:

$$\Delta \widehat{RE}_{p,t} = \sum_{s=1}^S \frac{e_{p,s,2011}}{e_{p,2011}} \left[\ln(RE_{s,t}) - \ln(RE_{s,t-1}) \right] \quad (1)$$

¹⁸Relative employment under temporary contracts (referred to as “relative employment” hereafter) is defined as the ratio of aggregate stocks of active temporary and permanent contracts from the ASIA dataset (see also Section 3.2).

¹⁹[Hershbein and Kahn \(2018\)](#) calculate an exposure measure that captures geographical variation in the severity of the Great Recession’s employment losses to identify its impact on skill demand. Their approach leverages sectoral heterogeneity in employment declines by calculating the difference between locally projected annual growth rates of sector-level employment during the years surrounding the recession. Specifically, to determine these growth rates, they consider the years 2006 (the *peak*, when employment was at its highest level before the recession) and 2009 (the *trough*, when it reached its lowest level during the recession).

where $RE_{s,t}$ denotes the nationwide relative employment under temporary contracts in sector s and year t (where $t \in \{2013, 2017\}$, representing the trough and peak years, respectively), and $\frac{e_{p,s,2011}}{e_{p,2011}}$ are the shares of employment in each sector s out of total employment in a given province p (computed in 2011, which is the last year before the series of reforms that occurred in the 2010s). To derive our exposure-to-treatment measure, we then take the difference between the two projections in the trough and peak years, as shown in Equation (2):

$$exposure_p = \widehat{\Delta RE}_{p,2017} - \widehat{\Delta RE}_{p,2013} \quad (2)$$

Figure 3 shows the nationwide sector-level annual growth in relative employment at the trough and peak years, which corresponds to the term in square brackets in Equation (1). In almost all sectors, we observe a negative growth in relative employment between 2012 and 2013, followed by a positive growth in relative employment between 2016 and 2017, with variations across sectors. Figures 4a and 4b display the projections of these nationwide sector-level annual growth rates in relative employment at the province level in the trough year ($\widehat{\Delta RE}_{p,2013}$) and in the peak year ($\widehat{\Delta RE}_{p,2017}$), respectively. The exposure-to-treatment measure is the difference between these two year-specific local projections, and it exhibits substantial variation across provinces, as shown in Figure 4c.

In summary, our measure is a Bartik-type exposure-to-treatment measure that leverages two factors: (i) the nationwide sectoral adjustments over time of relative employment under temporary contracts in response to the two earlier reforms, and (ii) predetermined sectoral employment distributions within provinces to project the nationwide sector-level exposure at the province level. This measure effectively captures which local areas (i.e., provinces) had become relatively more reliant on temporary contracts in the lead-up to the 2018 “Decreto Dignità” reform due to previous labor market reforms.

This measure offers three significant advantages. Firstly, by projecting nationwide sector-level exposure at the local level, the proposed Bartik measure avoids directly measuring local relative employment under temporary contracts, which could be influenced by local labor demand shocks and thus be potentially endogenous. Secondly, in the specific institutional context of this study, using the change in relative employment growth at peak and trough is more suitable than static measures, as it allows to grasp dynamic reactions to previous labor law changes that determine the exposure to the 2018 reform. Thirdly, this measure allows to obtain a location-specific measure of exposure, by weighing the relevant between- and within-sector identifying variation through province-by-sector predetermined employment shares, thereby maximizing variation and enhancing precision.

4.2 Specification

We begin by grouping the job postings into 13,383 province-sector-occupation (pso) cells over 12 calendar quarters (t) from 2017Q1 to 2019Q4. This results in an unbalanced panel dataset with 109,861 province-sector-occupation-quarter ($psot$) cells, each containing an average of about 30 job postings.²⁰ From this dataset, we create the following cell-level indicators related to labor demand and job requirements. Firstly, we define the demand for employment under a certain contractual arrangement as the share of all job postings in a cell that offer such a contract. Secondly, we calculate the demand for various human capital and specific skill requirements by computing the share of job postings in a cell that mention these requirements, both overall and separately for each advertised contract type. These indicators represent the outcomes of interest (Y_{psot}).

We estimate the following reduced-form difference-in-differences regression model:

$$Y_{psot} = \alpha_p + \beta_s + \gamma_o + \delta_t + \rho * \mathbb{1}[exposure_p \geq p50] * post_t + \epsilon_{psot} \quad (3)$$

where $\mathbb{1}[exposure_p \geq p50]$ is a discrete exposure-to-treatment indicator. This indicator takes value 1 when $exposure_p$ is above its median value, and 0 otherwise. Additionally, $post_t$ is a dummy variable representing the treatment period. It equals 1 when $t \geq 2018Q3$, which is the calendar quarter in which the “Decreto Dignità” was implemented, and 0 otherwise.

The specification also includes a comprehensive set of fixed effects to account for unobserved factors in each of the four dimensions that constitute our cells. Given that our identification strategy relies on the interaction of (i) national changes in relative employment growth by sector and (ii) province-level predetermined sector composition, it is crucial to include fixed effects at the province (α_p) and sector (β_s) levels. This ensures that any systematic differences between provinces and sectors with higher exposure-to-treatment, such as variations in technology adoption affecting labor demand and skill requirements, are properly controlled for, thereby strengthening the identification through our exposure-to-treatment measure. We also include occupation (γ_o) fixed effects to address unobserved confounders at the occupation level and calendar quarter (δ_t) fixed effects to account for secular trends that may be related to the demand for labor and skills. Standard errors are clustered at the province level (p) to address potential serial correlation within local units.

Moreover, in line with [Hershbein and Kahn \(2018\)](#), we consider the size of the labor force and the number of job vacancies in each labor market cell by weighting each cell-level obser-

²⁰Detailed descriptive statistics on the number of (unweighted) online job vacancies posted per cell are provided in Table [B.3](#).

vation. This weighting is done by taking the product of the cell’s total labor force, which is fixed at the last pre-reform year (2017), and the cell’s share of advertisements within each calendar quarter.

We also estimate a specification where the discrete exposure-to-treatment indicator, $\mathbb{1}[exposure_p \geq p50]$, is interacted with multiple pre- and post-reform time dummies, as shown in Equation (4) below:

$$Y_{psot} = \alpha_p + \beta_s + \gamma_o + \delta_t + \sum_{\tau \neq 2017Q1} \theta_\tau * \mathbb{1}[exposure_p \geq p50] * \mathbb{1}[t = \tau] + \epsilon_{psot} \quad (4)$$

The coefficients of interest in this specification, $\{\theta_\tau\}_{\tau \neq 2017Q1}$, capture the difference in outcomes at each quarter between areas with high and low exposure to the reform compared to 2017Q1 (the first quarter of the estimation sample, for which we set $\theta_{2017Q1} = 0$). This specification allows us to study the intensity and persistence of the estimated treatment effects at different points in time.²¹

Additionally, by estimating this specification, we can test the validity of the parallel trends identifying assumption. The identification would be threatened in the presence of pre-existing diverging trends in labor and skill demand across provinces with different industry mixes and, thus, different degrees of exposure to the reform. In Section 5, we investigate this assumption and find no evidence of underlying differential pre-trends in the outcome variables across exposure groups. Furthermore, to assess the robustness of the results, we perform a placebo test to check that exposure-to-treatment does not predict labor and skill demand changes before the reform. In addition, we also examine the sensitivity of the results to other ways of defining the exposure-to-treatment indicator, and to the inclusion of sector by calendar quarter fixed effects.

5 Results

To assess the impact of the increased cost of hiring with temporary contracts, we employ the difference-in-differences (DiD) approach outlined in Equations (3) and (4). This methodology allows us to compare outcomes before and after the policy change between two groups: firms in local areas more exposed to temporary contracts before the reform (“treatment group”) and their less exposed counterparts (“control group”). Section 5.1 presents the reform’s effects on

²¹It is important to note that treatment is simultaneously applied to all treated units and represents an absorbing state. Therefore, we do not need to apply the recently developed estimators for staggered difference-in-differences (for a comprehensive review, see [de Chaisemartin and D’Haultfoeuille \(2022\)](#)).

labor demand by contract type. Expanding the analysis, Section 5.2 investigates firms' demand for standard human capital measures such as education and experience. Furthermore, Section 5.3 delves into detailed skill requirements.

5.1 Reform effects on labor demand by contract type

We begin by examining the effect on labor demand under four distinct contract types: (i) permanent contracts, (ii) temporary contracts, (iii) self-employment positions, and (iv) internships. The outcome variables are defined as the proportions of job vacancies offering each of these contract types.

Firms' adjustments in hiring intentions across these contract types represent a potential avenue for labor demand adaptation in response to the reform. Higher costs associated with temporary contract hiring may lead to a greater reliance on alternative flexible employment arrangements, like self-employed positions or internships, or potentially shift demand towards permanent workers. The estimates from Equation (3) are presented in Table 4, indicating that firms more exposed to higher temporary worker hiring costs increased their demand for permanent employees (by 5.9 percentage points) and simultaneously reduced their demand for temporary workers (by 5.3 percentage points). Conversely, there is no discernible impact on the demand for self-employed workers or interns. These findings suggest that the reform induced a labor demand shift from temporary to permanent contracts.

Figure 5 displays the coefficient estimates from the dynamic specification of Equation (4) and their corresponding 95% confidence intervals for all four outcome variables. These coefficients capture the treatment effect of the reform during each calendar quarter τ compared to the baseline in 2017Q1.²²

In the top-left graph of Figure 5, it is evident that, before the reform, there exists no significant divergence in firms' propensity to advertise permanent job vacancies, supporting the validity of the parallel trends assumption. However, after the reform's implementation in 2018Q3, an observable shift is witnessed. Firms located in areas more affected by the higher hiring costs of temporary contracts exhibited an increased demand for permanent workers. By the last quarter of 2019 (2019Q4), the probability of posting a vacancy for a permanent worker increased by 8.6 percentage points, relative to the reference point in 2017Q1. This estimated increase corresponds to a 32% rise in the demand for permanent workers, relative to the average share of advertised permanent job positions before the reform (.268).

²²We set the reference period as the first calendar quarter (2017Q1) to ensure that the estimates remain unaffected by potential anticipation effects. Despite the reform being officially implemented in July 2018, public discussions about the policy change were ongoing from January 2018 due to the election campaign and the general election held in March 2018 (for additional details on the roll-out of the reform, refer to Appendix A.2).

The top-right graph of Figure 5 also illustrates the absence of differential pre-existing trends and that, subsequent to the reform, firms in areas more influenced by it reduced their demand for temporary workers. By the last quarter, the probability of posting a job vacancy with a temporary contract decreased by 6.2 percentage points, resulting in a 13% decline relative to the average pre-reform share of advertised temporary positions (.482).

The two graphs at the bottom of Figure 5 further validate the findings presented in Table 4, indicating that the reform did not impact the demand for workers under alternative flexible contract types, such as self-employment and internships. Overall, these results suggest a substitution effect, where temporary contracts were replaced by permanent contracts to a significant extent. This is consistent with a differential adjustment in hiring patterns in response to the sharp and significant increase in the relative cost of temporary contracts.

5.1.1 Robustness

To assess the robustness of the above findings, we perform several sensitivity analyses. First, we conduct a placebo test by shifting the time frame to the period between 2016Q1 and 2017Q4, prior to the implementation of the reform in 2018Q3. However, we maintain the reference period for the dynamic specification estimates at 2017Q1. Figure C.1 illustrates that no treatment effects are observed in these placebo regressions, further supporting the credibility of the research design and the results presented in Figure 5.

We proceed with two additional checks to assess the results' robustness against alternative definitions of the exposure-to-treatment indicator variable. In our baseline results, the indicator variable is defined using the median ($p50$) as the threshold. We first investigate the sensitivity of the results to various thresholds. As depicted in Figure C.7, the baseline results remain stable when considering cutoffs such as $p35$, $p40$, $p45$, $p55$, $p60$ or $p65$.

Additionally, we consider an alternative definition of the indicator variable, where it equals 1 if $exposure_p$ falls within the top tercile of its distribution, and 0 if it lies within the bottom tercile. This approach involves excluding observations for provinces with medium tercile exposure. Figure C.8 shows that the estimates remain largely consistent even with this alternative definition of the exposure-to-treatment indicator variable.

Furthermore, we extend our analysis by estimating a variation of the Equation (4) specification that includes sector by calendar quarter fixed effects. These fixed effects capture sector-specific changes over time that might influence labor demand. The resulting coefficient estimates, as shown in Figure C.9, exhibit considerable stability, suggesting that time-varying sector-level unobservable factors are unlikely to impact our findings.

5.2 Reform effects on education and experience requirements

We next study the impact of higher costs associated with hiring under temporary contracts on the demand for conventional measures of human capital, namely educational level and years of experience. We evaluate the reform’s effects across all job vacancies and distinguish the impact by contract type. Figure 6 presents the estimates from the dynamic specification in Equation (4).²³

In Figure 6a, we find that firms more exposed to the reform exhibited an increased demand for workers with college degrees (post-secondary education) and a decreased demand for high school graduates (upper-secondary education), relative to their less exposed counterparts. Focusing on contract types, Figure 6b indicates that these effects are primarily due to shifts in educational requirements for permanent job vacancies right after the reform’s announcement in 2018Q2. This indicates that firms, in response to increased costs of hiring temporary workers, are willing to hire more permanent workers, but require higher educational qualifications.

Figure 7a reveals a general rise in the demand for workers with some experience (1-4 years) due to the reform, coupled with a decline in demand for individuals with extensive experience (4+ years) or no experience (0-1 years), although the latter effect is not precisely estimated.²⁴ By examining contract types, Figure 7b highlights that the decrease in demand for workers with no experience is mostly driven by job positions offering permanent contracts. On the other hand, the increase in demand for workers with some experience is mainly driven by positions offering temporary contracts.

Our findings indicate that the increased hiring costs for temporary workers not only shifted the demand from temporary to permanent workers but also led to changes in the required levels of human capital. Specifically, firms offering permanent contracts are more likely to require a college degree and less likely to offer such contracts to individuals with no experience. Meanwhile, firms offering temporary contracts are more likely to require some prior work experience. Overall, our results suggest that while the reform promoted the hiring of permanent workers, it also limited the labor market opportunities for less educated and inexperienced workers.

²³Given the evidence indicating that the reform did not impact the demand for workers under self-employed and internship contracts, our subsequent analyses concentrate solely on permanent and temporary contracts.

²⁴The decrease in the demand for highly experienced workers might be due to firms’ reluctance to hire this category of workers because of their higher labor costs. Another plausible explanation pertains to the potential conversions from temporary to permanent contracts within firms, which may involve high-experienced workers.

5.3 Reform effects on the demand for skills

In this section, we expand our analysis beyond conventional measures of human capital and delve into the effects of the reform on skill demand. Building upon the classification outlined in Section 3.1 (also depicted in Table 1), we examine the following skill categories: cognitive, management, computer, and social skills.

5.3.1 Share of job vacancies requiring a specific skill

As a first approach, we define the dependent variable for each skill category as the proportion of job vacancies within a cell that demand that particular skill. Similar to our approach for education and experience requirements, we investigate the reform’s effects for all vacancies and separately for vacancies offering temporary or permanent positions.

When considering all vacancies, Figure 8a illustrates that firms facing greater exposure to the higher costs of hiring temporary workers increased their demand for social skills in comparison to their less exposed counterparts. Social skills pertain to communication, organization, and supportive tasks. However, we do not observe any significant effect of the reform on the demand for cognitive, management, or computer skills. Focusing on contract types, Figure 8b reveals that the increase in the demand for social skills is apparent in both temporary and permanent job vacancies. The magnitude of these effects is roughly comparable (around 2.2 percentage points), although the relative increase is more pronounced among temporary job vacancies (+3.9% from a pre-reform average of .576) compared to permanent job vacancies (+2.8% from .795). These findings suggest that the reform led firms with higher exposure to raise the requirements for social skills for entry jobs, particularly for temporary hires that had become relatively more costly.

5.3.2 Relative importance of specific skill categories

We also explore two alternative dependent variables based on the relative importance of each skill category. Drawing on the approach suggested by Alabdulkareem et al. (2018), these alternative measures allow us to examine the impact of the reform on the reallocation of the demand for skills both within and between labor markets.

The first measure, denoted as $RI_{c,t}^{W(k)}$ in Equation (5), captures the relative importance of a skill category k required *within* a labor market defined by cell c at time t , where there are K skill categories, including cognitive, management, computer, and social skills.

$$RI_{c,t}^{W(k)} = \frac{\sum_{v \in (c,t)} s_v^k}{\sum_{k \in K} \sum_{v \in (c,t)} s_v^k} \quad (5)$$

It is worth noting that each job vacancy might require one or multiple specific skills of category k (s_v^k denotes the count per vacancy v), while cells correspond to province-sector-occupation (*pso*) groups as introduced in Section 4. As outlined in Equation (5), the relative importance of a skill category k is calculated as the ratio of the number of specific skills of category k required in vacancies within cell c at time t to the total number of specific skills across all categories demanded in all vacancies posted in cell c at time t . Essentially, this measure traces the prevalence of a particular skill category relative to all categories of skills demanded *within* a given labor market over time. Its range lies between 0 and 1.

The second measure, denoted as $RI_{c,t}^{B(k)}$ in Equation (6), captures the relative importance of a skill category k *between* different labor markets. It is a normalized version of $RI_{c,t}^{W(k)}$, utilizing the prevalence of skill category k in *all* labor markets $c \in C$ at t , where C denotes the total number of cells (markets).

$$RI_{c,t}^{B(k)} = \frac{\sum_{v \in (c,t)} s_v^k / \sum_{k \in K} \sum_{v \in (c,t)} s_v^k}{\sum_{c \in C} \sum_{v \in (c,t)} s_v^k / \sum_{c \in C} \sum_{k \in K} \sum_{v \in (c,t)} s_v^k} \quad (6)$$

This measure captures how much a specific skill category k is either more or less prominent in the vacancies in cell c at time t compared to the overall labor market. If a skill category k is more in demand in market c compared to the overall market, this measure will be greater than 1; conversely, if it is less in demand in market c compared to the overall market, the metric will be below 1.

Our findings reveal a significant reallocation of the demand for social skills both within and between labor markets. Figure 9a illustrates that firms more exposed to the higher costs of hiring temporary workers increased their demand for social skills relative to other skill categories. This indicates that social skills became relatively more prevalent within more exposed markets. Furthermore, Figure 10a indicates that firms in these markets began demanding social skills more compared to all markets collectively. No substantial shifts in reallocation were found for other skill categories like cognitive, management, and computer skills. Moreover, these effects are driven predominantly from the reallocation of skill demand within temporary job postings (Figures 9b and 10b). Consequently, our results suggest that firms with greater exposure to the reform reacted by attaching greater significance to social

skills, particularly when considering temporary hires.²⁵

6 Conclusion

Over the past decade, several countries have taken measures to address the persistent disparity in employment protection between permanent and fixed-term employment arrangements, which led to a dual labor market. These efforts involve relaxing regulations on permanent contracts and, more recently, imposing restrictions on the utilization of fixed-term contracts.

In this paper, we study the impact of increasing the cost of fixed-term contracts on labor demand and skill requirements, exploiting a 2018 Italian labor law reform known as the “Decreto Dignità”. To identify the reform’s effects, we leverage the variation in firms’ exposure to fixed-term contracts driven by diverse responses to previous labor market reforms enacted from 2012 to 2017. This variation implies that some employers had become more reliant on temporary contracts for hiring workers.

Drawing on online job vacancy data, our analysis shows that the increase in the cost of hiring under fixed-term contracts led to a decline in the relative demand for temporary workers and a simultaneous increase in the relative demand for permanent workers. Additionally, this shift in labor demand was accompanied by upskilling. For permanent job openings, firms raised their demand for more educated workers and those possessing social skills, while they showed more reluctance to hire workers with no prior experience. For temporary job opening, firms increased their demand for workers with some work experience, and similarly to permanent jobs, those possessing social skills.

Our findings demonstrate that firms more reliant on fixed-term contracts, who were thus affected more by the reform’s increased hiring costs, responded by significantly reducing their reliance on temporary contracts, opting instead to commit to hiring more permanent workers. However, due to increased screening costs and the loss of flexibility, firms adjusted their labor demand for both contract types, favoring candidates with higher human capital. This move compensated for the lack of quality signals from workers and safeguarded against adverse selection.

The evidence provided in this study contributes valuable insights to ongoing policy discussions, underscoring that labor law reforms seeking to limit the use of fixed-term contracts, or other forms of flexible employment, can have unintended consequences. Such reforms may shift entry-level job standards in terms of skill requirements, potentially worsening employ-

²⁵We have also conducted placebo tests for the outcomes analyzed in Sections 5.2 and 5.3. As depicted in Figures C.2 to C.6, these tests further confirm the robustness of the results. Moreover, we performed additional sensitivity tests to confirm the robustness of the findings presented in those sections to alternative definitions of the exposure-to-treatment indicator variable; these results can be provided upon request.

ment prospects for less skilled workers and affecting the effectiveness of job search and matching processes.

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Tables

Table 1: Job skills taxonomy from [Deming and Kahn \(2018\)](#) and own aggregation

4 Aggregate Job Skills (own aggregation)	10 Job Skills (D&K 2018)	Keywords and Phrases
<i>COGNITIVE</i>	Cognitive	Problem solving, research, analytical, critical thinking, math, statistics
<i>COGNITIVE</i>	Writing	Writing
<i>MANAGEMENT</i>	Project Management	Project management
<i>MANAGEMENT</i>	People Management	Supervisory, leadership, management (not project), mentoring, staff
<i>MANAGEMENT</i>	Financial	Budgeting, accounting, finance, cost
<i>COMPUTER</i>	Computer (general)	Computer, spreadsheets, common software (e.g., Microsoft Excel, PowerPoint)
<i>COMPUTER</i>	Software (specific)	Programming language or specialized software (e.g., Java, SQL, Python)
<i>SOCIAL</i>	Social	Communication, teamwork, collaboration, negotiation, presentation
<i>SOCIAL</i>	Character	Organized, detail oriented, multitasking, time management, meeting deadlines, energetic
<i>SOCIAL</i>	Customer Service	Customer, sales, client, patient

Note: The table illustrates our job skill taxonomy, which is constructed by searching for specific keywords and phrases within the standardized text field associated to each vacancy. Each vacancy may contain multiple standardized text fields, each of which is assigned to a unique, mutually exclusive skill category. The right column lists the keywords and phrases used by [Deming and Kahn \(2018, Table 1\)](#) to derive their 10 job skill categories (center column). We map those 10 categories into 4 aggregate job skill categories (left column).

Table 2: Descriptive statistics on requirements posted in online job vacancies (OJVs)
(aggregate job requirements)

	Mean	(Std. Dev.)
<i>Contract</i>		
Contract type stated	0.949	(0.219)
Permanent contract	0.241	(0.428)
Temporary contract	0.447	(0.497)
Self-employed position	0.165	(0.371)
Internship	0.096	(0.294)
<i>Education</i>		
Education requirement stated	0.998	(0.046)
High School: Up to Secondary	0.676	(0.468)
College: Post-Secondary	0.322	(0.467)
<i>Experience</i>		
Experience requirement stated	0.616	(0.486)
None: 0-1 years	0.178	(0.383)
Some: 1-4 years	0.330	(0.470)
High: ≥ 4 years	0.108	(0.310)
<i>Specific skills</i>		
Specific skill requirements stated	0.851	(0.356)
Cognitive	0.493	(0.500)
Management	0.392	(0.488)
Computer	0.444	(0.497)
Social	0.706	(0.456)
N of online job vacancies (OJVs)	3,331,650	

Note: The table reports descriptive statistics on aggregate job requirements—i.e., contract type, and human capital and specific skills requirements—mentioned in the online job vacancies contained in the WollyBi data. Vacancies are weighted by the size of the labor force of the province in which the ad was posted (fixed at the level of 2017, the last pre-reform year). Analogous descriptive statistics obtained using disaggregate job requirements are reported in Table [B.1](#).

Table 3: Descriptive statistics on requirements posted in online job vacancies (OJVs)
(aggregate job requirements), by contract

	Permanent		Temporary		Self Employment		Internship	
	Mean	(Std. Dev.)	Mean	(Std. Dev.)	Mean	(Std. Dev.)	Mean	(Std. Dev.)
<i>Education</i>								
Education requirement stated	0.998	(0.045)	0.998	(0.046)	0.998	(0.047)	0.999	(0.036)
High School: Up to Secondary	0.589	(0.492)	0.751	(0.432)	0.684	(0.465)	0.502	(0.500)
College: Post-Secondary	0.409	(0.492)	0.247	(0.431)	0.314	(0.464)	0.497	(0.500)
<i>Experience</i>								
Experience requirement stated	0.589	(0.492)	0.660	(0.474)	0.594	(0.491)	0.633	(0.482)
None: 0-1 years	0.116	(0.320)	0.168	(0.374)	0.204	(0.403)	0.394	(0.489)
Some: 1-4 years	0.312	(0.463)	0.409	(0.492)	0.269	(0.443)	0.172	(0.377)
High: ≥ 4 years	0.162	(0.368)	0.083	(0.277)	0.121	(0.327)	0.067	(0.250)
<i>Specific skills</i>								
Specific skill requirements stated	0.923	(0.266)	0.802	(0.398)	0.881	(0.323)	0.912	(0.284)
Cognitive	0.618	(0.486)	0.404	(0.491)	0.568	(0.495)	0.521	(0.500)
Management	0.518	(0.500)	0.317	(0.465)	0.410	(0.492)	0.416	(0.493)
Computer	0.592	(0.491)	0.350	(0.477)	0.476	(0.499)	0.518	(0.500)
Social	0.806	(0.396)	0.632	(0.482)	0.750	(0.433)	0.813	(0.390)
N of online job vacancies (OJVs)	804,332		1,489,804		549,927		318,638	

Note: The table reports descriptive statistics on aggregate job requirements—i.e., human capital and specific skills requirements—mentioned in the online job vacancies contained in the WollyBi data, by type of contract. Vacancies are weighted by the size of the labor force of the province in which the ad was posted (fixed at the level of 2017, the last pre-reform year). Analogous descriptive statistics obtained using disaggregate job requirements are reported in Table B.2.

Table 4: Reform effects on Labor Demand (Baseline Specification)

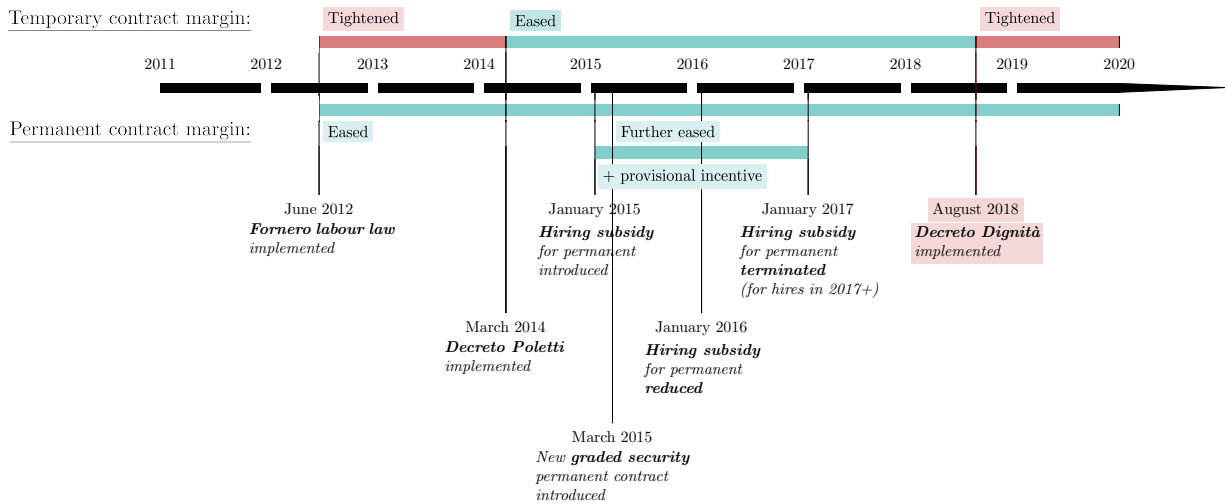
	Share of all vacancies			
	Permanent	Temporary	Self-employment	Internship
$\mathbb{1}[exposure_p \geq p50] * post_t$	0.059*** (0.015)	-0.053*** (0.017)	-0.005 (0.006)	-0.004 (0.003)
Province FE	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes	Yes
Calendar quarter FE	Yes	Yes	Yes	Yes
Observations	109,861	109,861	109,861	109,861

Note: The table presents estimates from the baseline model in Equation (3) indicating the impact of the reform on the share of total vacancies offering job positions under permanent contracts (first column), temporary contracts (second column), self-employment (third column), and internships (fourth column). The outcome shares are calculated within each province-sector-occupation-calendar quarter cell, which serves as the unit of analysis. The treatment indicator, denoted as $\mathbb{1}[exposure_p \geq p50]$, takes a value of 1 for provinces with high exposure to the reform (defined as $exposure_p \geq p50$) and 0 for provinces with low exposure ($exposure_p < p50$). The variable $post_t$ is a dummy for the treatment period, equal to 1 when $t \geq 2018Q3$, the calendar quarter when the “Decreto Dignità” was introduced, and 0 otherwise. Standard errors are clustered at the province level p . In the regression analysis, each observation at the cell level is weighted by the total labor force in the corresponding province-sector-occupation cell in 2017, multiplied by the cell’s share of ads within each calendar quarter.

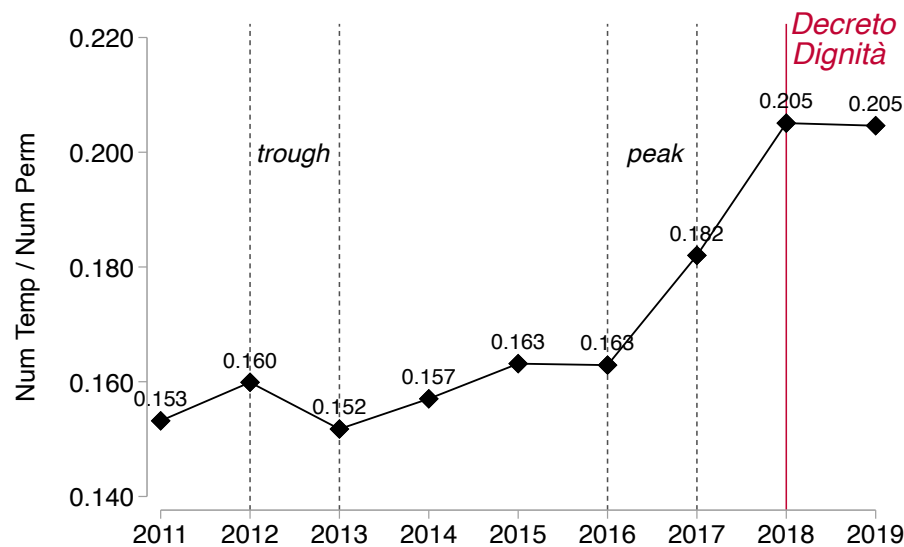
Figures

Figure 1: Trends in the EPL and Relative Employment under Temporary contracts

a. Timeline of EPL policies affecting Temporary and Permanent contracts

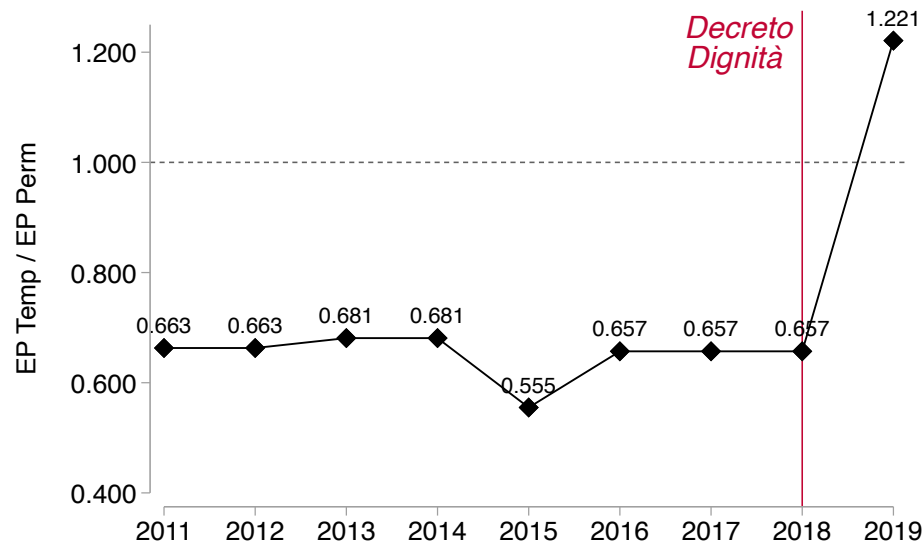


b. Aggregate trends in Relative Employment under Temporary contracts (Ratio of Temporary-to-Permanent employment stocks)



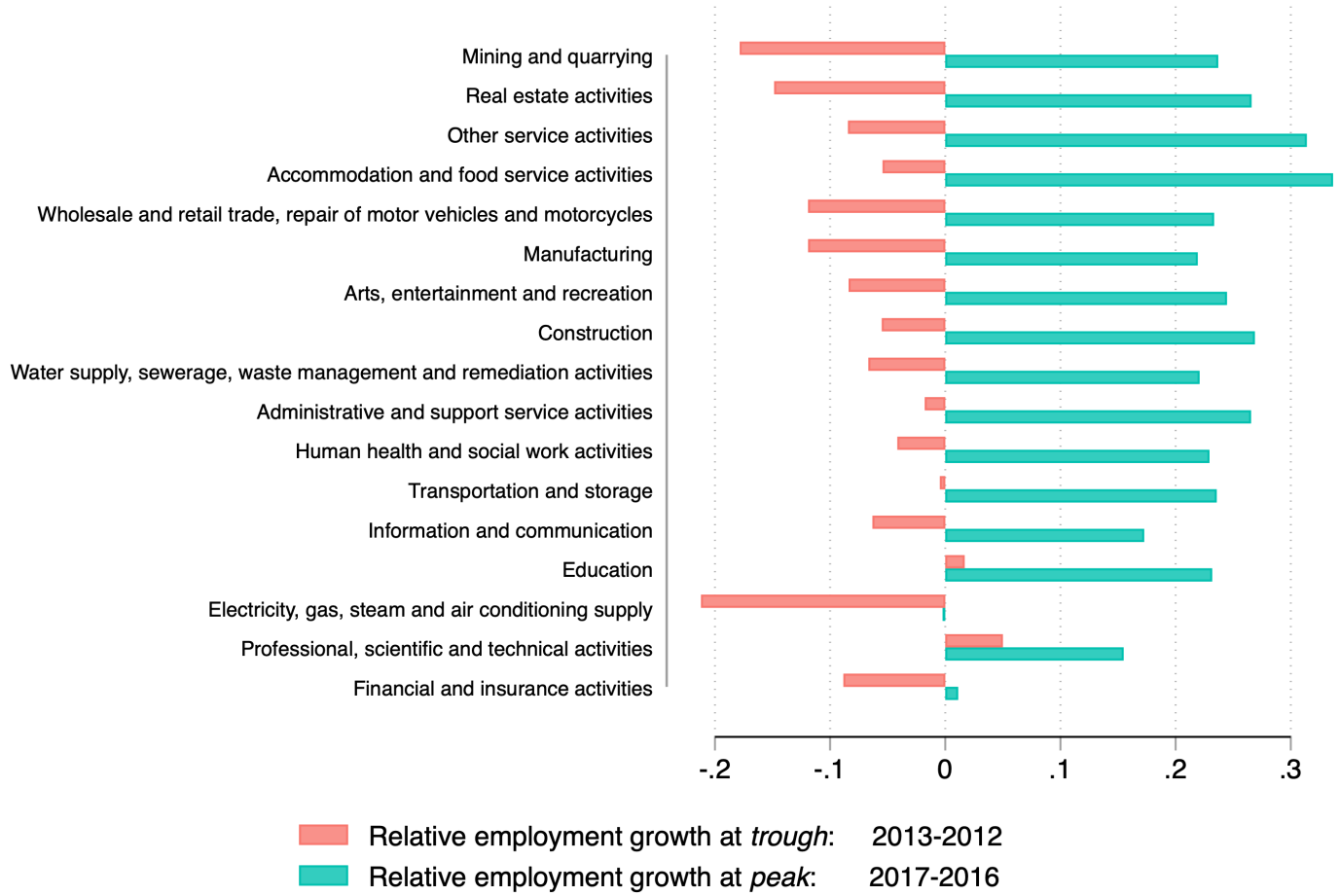
Note: The top graph presents a timeline summarizing the changes in Italian Employment Protection Legislation (EPL) that occurred during the 2010s. The figure outlines the impact of each reform on EPL for both temporary contracts (indicated by the top colored bar above the timeline) and permanent contracts (represented by the bottom colored bar). Tightening and easing interventions are depicted in red and green, respectively. The bottom graph displays aggregate trends at the nationwide level over the same period of the relative employment under temporary contracts. This measure is calculated as the ratio of temporary and permanent employment stocks using ISTAT's Labor Force Survey. The dashed vertical lines mark the *trough* and *peak* years discussed in Section Section 4.1.

Figure 2: Employment Protection Gap between
(Ratio of Temporary-to-Permanent OECD EPL indicators)



Note: The figure depicts trends in the ratio of the OECD's Employment Protection Legislation (EPL) indicators for temporary and permanent contracts. This ratio reflects the relative employment protection of temporary contracts in comparison to permanent contracts, thereby representing the EPL gap between the two contract types. The dashed horizontal line signifies the point at which both contract types have an equivalent level of employment protection. The indicators used are obtained from the 2020 version of the Employment Protection Legislation Database published by the OECD. The methodology employed to derive these indicators is extensively described on the following webpage: <https://www.oecd-ilibrary.org/sites/1686c758-en/1/3/3/index.html?itemId=/content/publication/1686c758...>

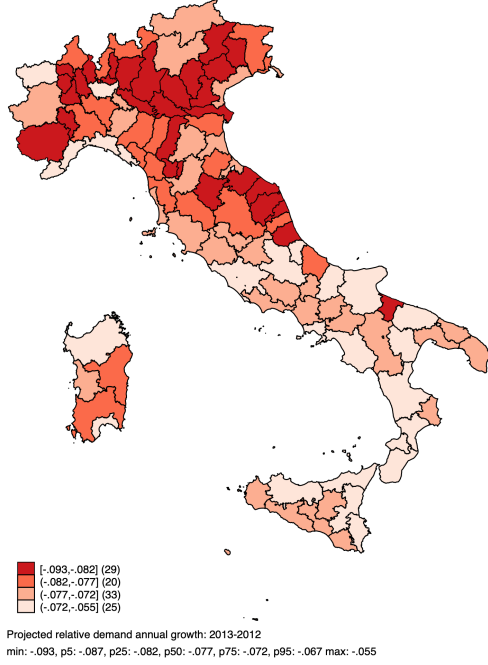
Figure 3: Nationwide sector-level annual growth rates of relative employment at *trough* and *peak*



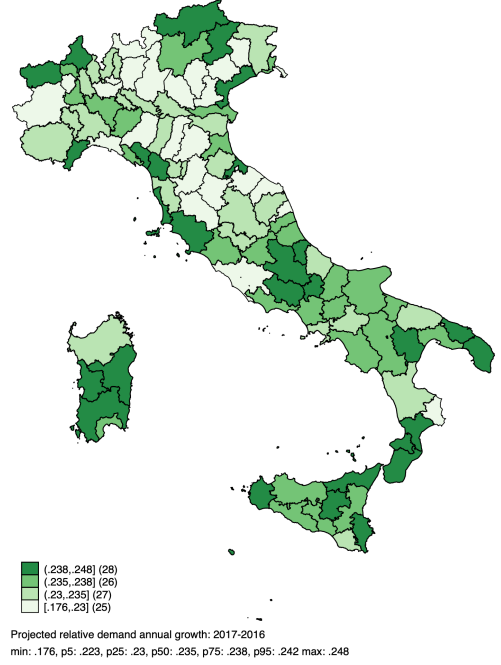
Note: The figure shows nationwide sector-level annual growth rates in relative employment under temporary contracts — $[(\ln(RE_{s,t}) - \ln(RE_{s,t-1}))]$ in Equation (1) — computed at *trough* ($t = 2013$, red bars) and *peak* ($t = 2017$, green bars) years. For each 1-digit sector s and year t , relative employment ($RE_{s,t}$) is calculated as the ratio of aggregate stocks of active temporary and permanent contracts from the ASIA dataset.

Figure 4: Projected relative employment growth (and change in growth): *trough* (2013) and *peak* (2017) (projection weights: province-specific sectoral shares of total employment in 2011)

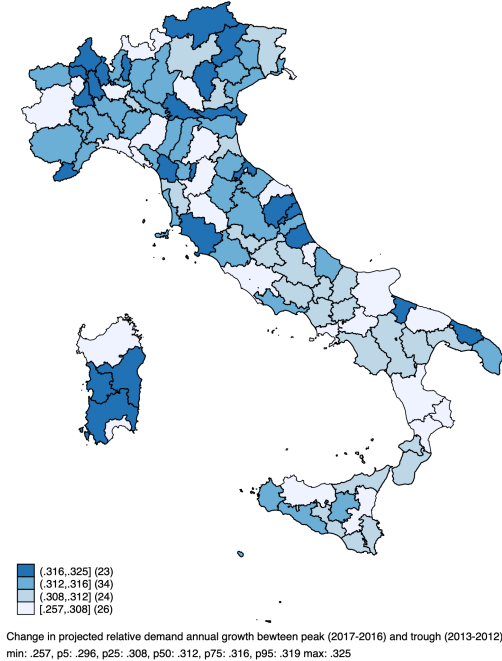
a. Projected relative employment growth at trough: 2013-2012 ($\Delta \widehat{RE}_{p,2013}$)



b. Projected relative employment growth at peak: 2017-2016 ($\Delta \widehat{RE}_{p,2017}$)

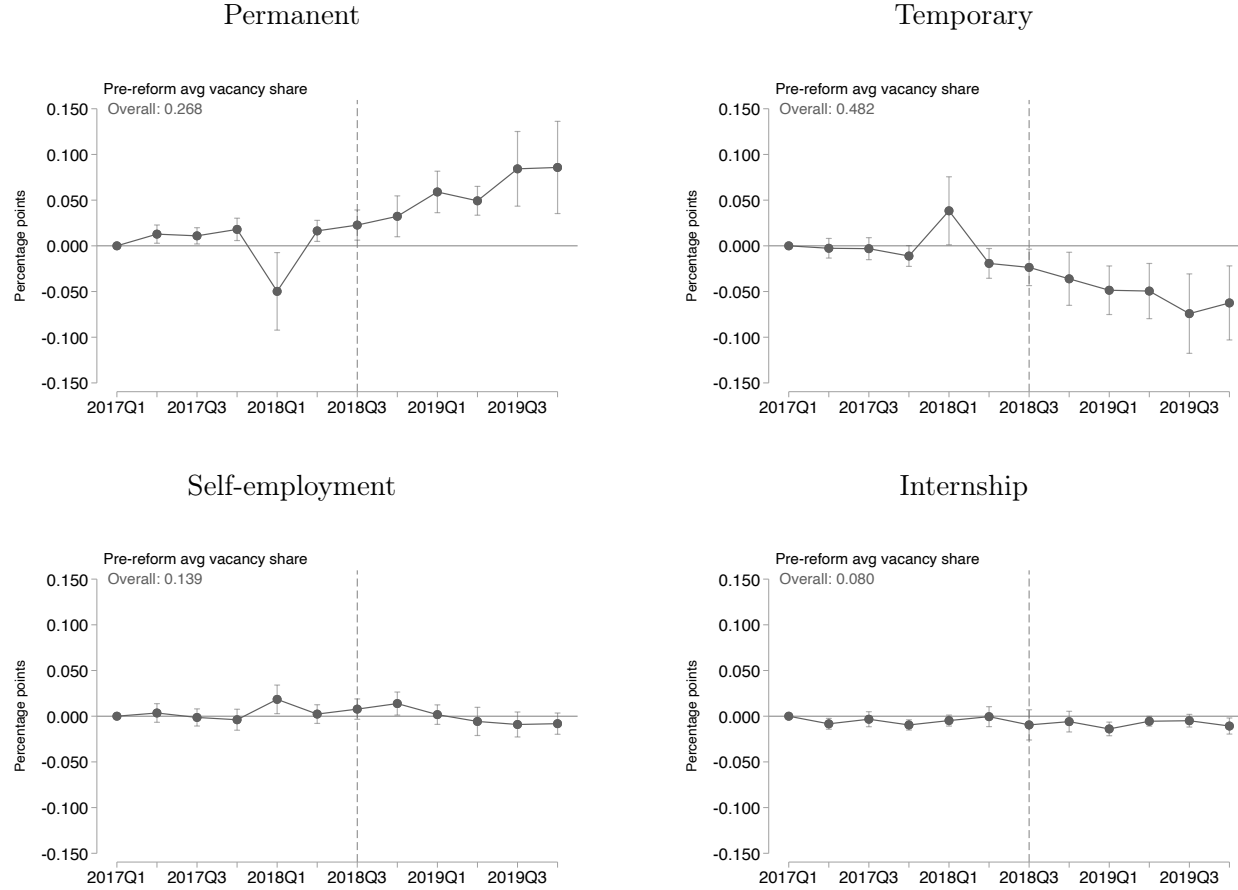


c. Change in proj. rel. employment annual growth between peak and trough
 $exposure_p = \Delta \widehat{RE}_{p,2017} - \Delta \widehat{RE}_{p,2013}$



Note: The top-left (panel a, in red) and top-right (panel b, in green) figures respectively display the variation in province-level projections of annual growth in relative employment at the *trough* ($\Delta \widehat{RE}_{p,2013}$) and *peak* ($\Delta \widehat{RE}_{p,2017}$) periods. These projections are computed as outlined in Equation (1), with projection weights determined by province-specific sectoral employment shares of total employment in 2011. The bottom-left figure (panel c, in blue) illustrates the variations in $exposure_p$, which is defined as the difference between the two above projections, as presented in Equation (2). All computations utilize aggregate data from the ASIA dataset.

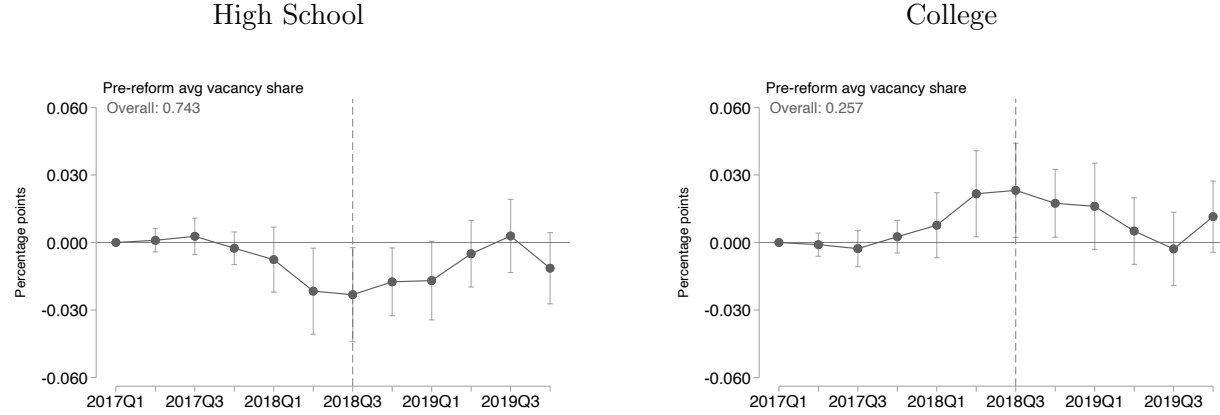
Figure 5: Labor demand by contract type



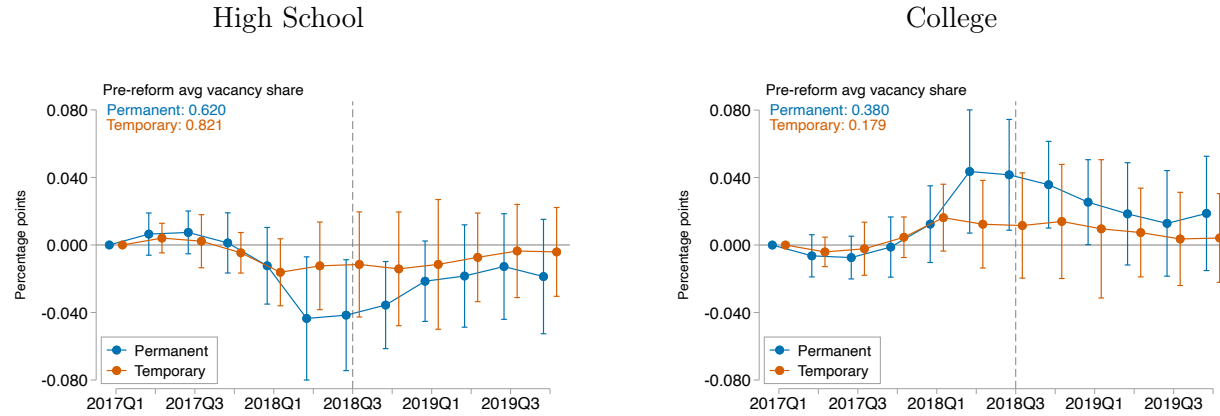
Note: The figure shows the impact of the reform on the share of total vacancies offering job positions under permanent contracts (top left), temporary contracts (top right), self-employment (bottom left), and internships (bottom right). The outcome shares are calculated within each province-sector-occupation-calendar quarter cell, which serves as the unit of analysis. The treatment indicator takes a value of 1 for provinces with high exposure to the reform (defined as $exposure_p \geq p50$) and 0 for provinces with low exposure ($exposure_p < p50$). The estimated treatment effect coefficients $\{\theta_\tau\}_{\tau \neq 2017Q1}$ from Equation (4) are plotted alongside their corresponding 95% confidence intervals. These coefficients capture the differences in outcomes at each quarter between provinces with high and low exposure to the reform relative to the reference period 2017Q1. The reform was implemented in 2018Q3. Standard errors are clustered at the province level p . In the regressions analysis, each cell-level observation is weighted by the total labor force in the corresponding province-sector-occupation cell for 2017, multiplied by the cell's share of ads within each calendar quarter.

Figure 6: Education requirements

a. *All vacancies*

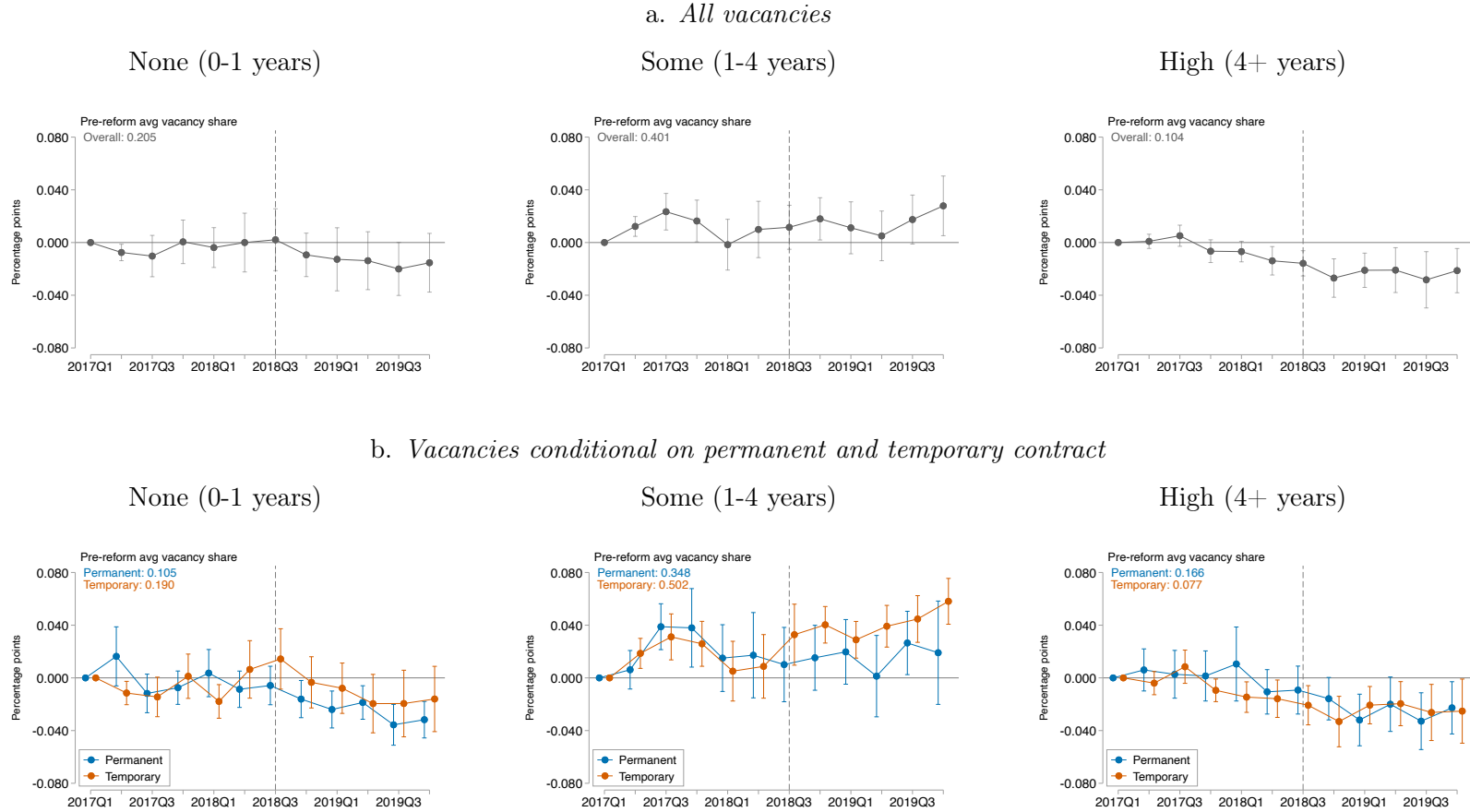


b. *Vacancies conditional on permanent and temporary contract*



Note: The figure shows the impact of the reform on the share of vacancies (out of total job vacancies in panel a, and out of permanent or temporary job vacancies in panel b) requiring up to secondary education (high school in the left column), and post-secondary education (college degree in the right column). The outcome shares are calculated within each province-sector-occupation-calendar quarter cell, which serves as the unit of analysis. The treatment indicator takes a value of 1 for provinces with high exposure to the reform (defined as $exposure_p \geq p50$) and 0 for provinces with low exposure ($exposure_p < p50$). The estimated treatment effect coefficients $\{\theta_\tau\}_{\tau \neq 2017Q1}$ from Equation (4) are plotted alongside their corresponding 95% confidence intervals. These coefficients capture the differences in outcomes at each quarter between provinces with high and low exposure to the reform relative to the reference period 2017Q1. The reform was implemented in 2018Q3. Standard errors are clustered at the province level p . In the regressions analysis, each cell-level observation is weighted by the total labor force in the corresponding province-sector-occupation cell for 2017, multiplied by the cell's share of ads within each calendar quarter.

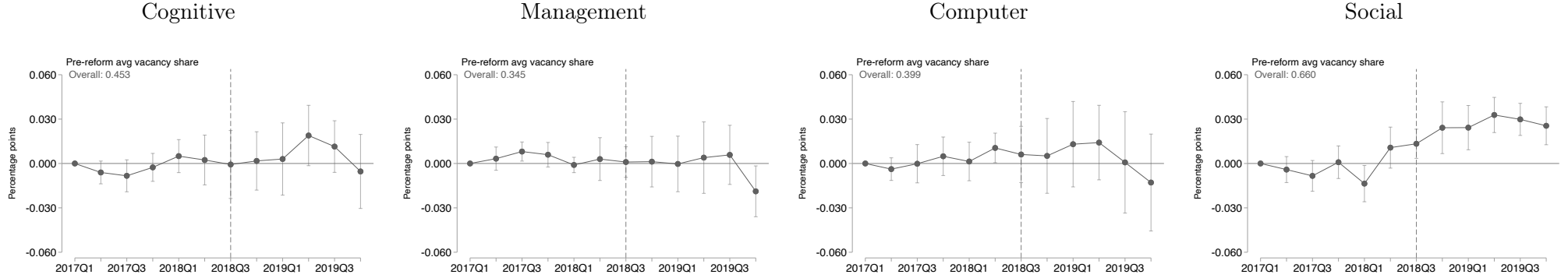
Figure 7: Experience requirements



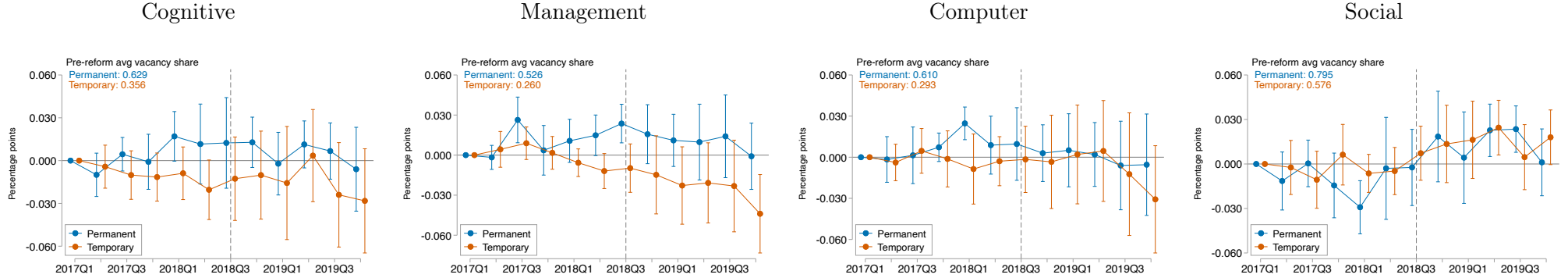
Note: The figure shows the impact of the reform on the share of vacancies (out of total job vacancies in panel a, and out of permanent or temporary job vacancies in panel b) requiring no experience (0-1 years in the left column), some experience (1-4 years in the center column) and high experience (4+ year in the right column). The outcome shares are calculated within each province-sector-occupation-calendar quarter cell, which serves as the unit of analysis. The treatment indicator takes a value of 1 for provinces with high exposure to the reform (defined as $exposure_p \geq p50$) and 0 for provinces with low exposure ($exposure_p < p50$). The estimated treatment effect coefficients $\{\theta_\tau\}_{\tau \neq 2017Q1}$ from Equation (4) are plotted alongside their corresponding 95% confidence intervals. These coefficients capture the differences in outcomes at each quarter between provinces with high and low exposure to the reform relative to the reference period 2017Q1. The reform was implemented in 2018Q3. Standard errors are clustered at the province level p . In the regressions analysis, each cell-level observation is weighted by the total labor force in the corresponding province-sector-occupation cell for 2017, multiplied by the cell's share of ads within each calendar quarter.

Figure 8: Specific skill requirements

a. *All vacancies*

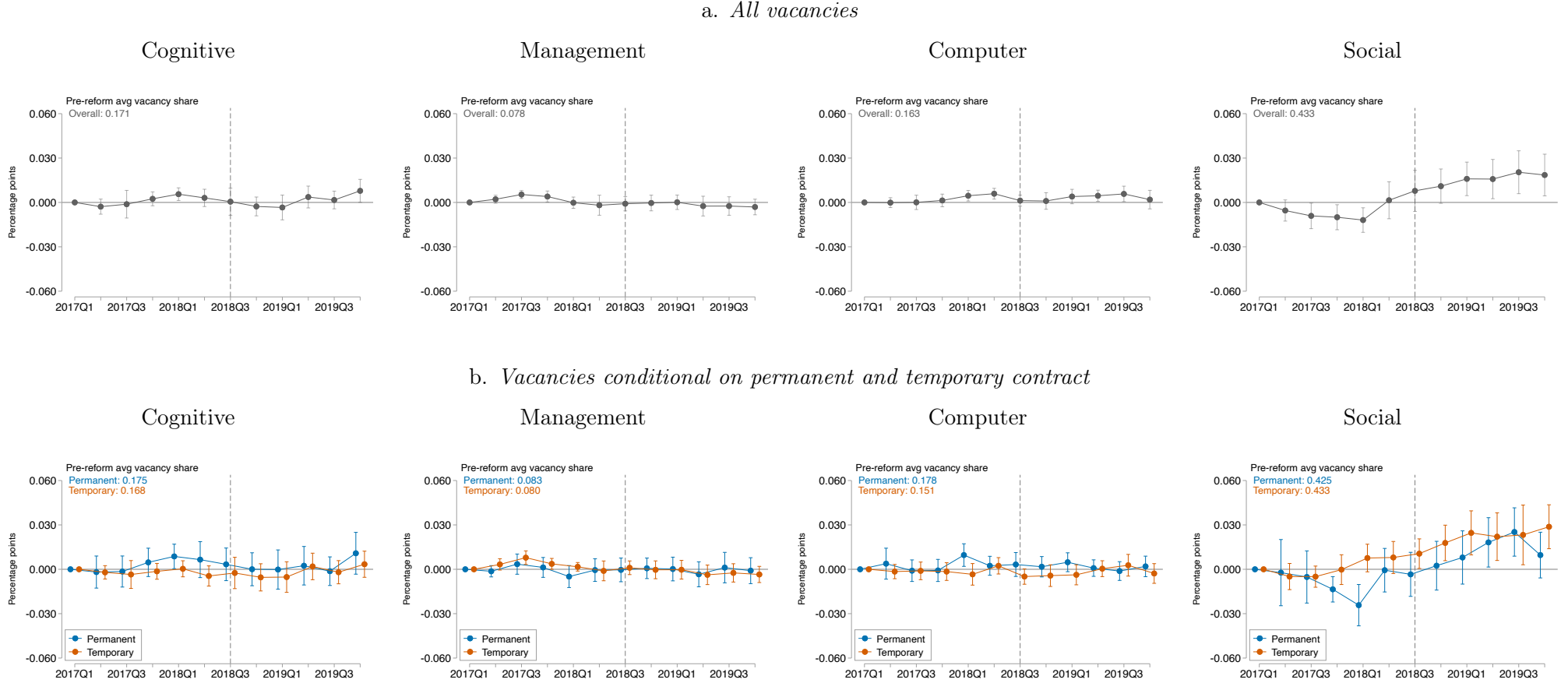


b. *Vacancies conditional on permanent and temporary contract*



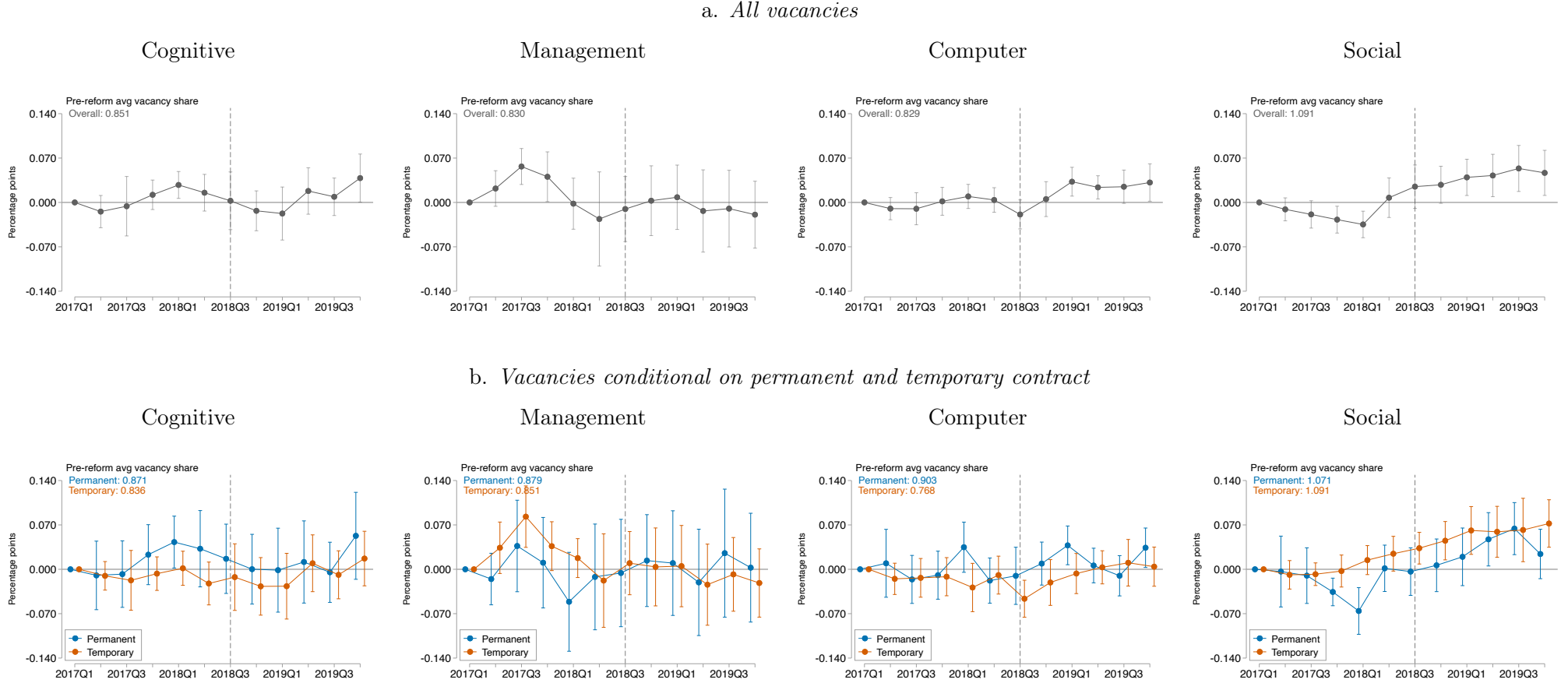
Note: The figure shows the impact of the reform on the share of vacancies (out of total job vacancies in panel a, and out of permanent or temporary job vacancies in panel b) requiring cognitive (first column), management (second column), computer (third column), and social (fourth column) skills. The outcome shares are calculated within each province-sector-occupation-calendar quarter cell, which serves as the unit of analysis. The treatment indicator takes a value of 1 for provinces with high exposure to the reform (defined as $exposure_p \geq p50$) and 0 for provinces with low exposure ($exposure_p < p50$). The estimated treatment effect coefficients $\{\theta_\tau\}_{\tau \neq 2017Q1}$ from Equation (4) are plotted alongside their corresponding 95% confidence intervals. These coefficients capture the differences in outcomes at each quarter between provinces with high and low exposure to the reform relative to the reference period 2017Q1. The reform was implemented in 2018Q3. Standard errors are clustered at the province level p . In the regressions analysis, each cell-level observation is weighted by the total labor force in the corresponding province-sector-occupation cell for 2017, multiplied by the cell's share of ads within each calendar quarter.

Figure 9: Specific skill requirements, *within*-cells relative importance



Note: The figure shows the impact of the reform on the *within*-cells relative importance indicators (for total job vacancies in panel a, and for permanent or temporary job vacancies in panel b) for cognitive (first column), management (second column), computer (third column), and social (fourth column) skills. The within-cells relative importance indicators are calculated within each province-sector-occupation-calendar quarter cell, which serves as the unit of analysis. The treatment indicator takes a value of 1 for provinces with high exposure to the reform (defined as $exposure_p \geq p50$) and 0 for provinces with low exposure ($exposure_p < p50$). The estimated treatment effect coefficients $\{\theta_\tau\}_{\tau \neq 2017Q1}$ from Equation (4) are plotted alongside their corresponding 95% confidence intervals. These coefficients capture the differences in outcomes at each quarter between provinces with high and low exposure to the reform relative to the reference period 2017Q1. The reform was implemented in 2018Q3. Standard errors are clustered at the province level p . In the regressions analysis, each cell-level observation is weighted by the total labor force in the corresponding province-sector-occupation cell for 2017, multiplied by the cell's share of ads within each calendar quarter.

Figure 10: Specific skill requirements, *between-cells* relative importance



Note: The figure shows the impact of the reform on the *between-cells* relative importance indicators (for total job vacancies in panel a, and for permanent or temporary job vacancies in panel b) for cognitive (first column), management (second column), computer (third column), and social (fourth column) skills. The *between-cells* relative importance indicators are calculated within each province-sector-occupation-calendar quarter cell, which serves as the unit of analysis. The treatment indicator takes a value of 1 for provinces with high exposure to the reform (defined as $exposure_p \geq p50$) and 0 for provinces with low exposure ($exposure_p < p50$). The estimated treatment effect coefficients $\{\theta_\tau\}_{\tau \neq 2017Q1}$ from Equation (4) are plotted alongside their corresponding 95% confidence intervals. These coefficients capture the differences in outcomes at each quarter between provinces with high and low exposure to the reform relative to the reference period 2017Q1. The reform was implemented in 2018Q3. Standard errors are clustered at the province level p . In the regressions analysis, each cell-level observation is weighted by the total labor force in the corresponding province-sector-occupation cell for 2017, multiplied by the cell's share of ads within each calendar quarter.

A Duality in the Italian employment protection legislation (EPL): an overview of the institutional context

A.1 EPL reforms during 1997-2017

The Italian labor market exhibits a dual structure where permanent employees enjoy higher levels of employment protection compared to temporary workers. The roots of this duality can be traced back to the turn of the new millennium, when legislative actions such as *Law 196/1997* (commonly known as the “Treu Package”) and *Legislative Decree 368/2001* were introduced.²⁶ The first one initially liberalised temporary contracts, while the second one later considerably reduced the legal constraints on their usage. As a result, the extent of employment protection for temporary contracts decreased remarkably, while that for permanent contracts remained largely unchanged (Boeri, 2011; Cappellari et al., 2012; Daruich et al., 2023). For this reason, these types of reforms in EPL, similar to those pursued by other European governments in recent decades, have been termed *partial* EPL reforms (Boeri, 2011).²⁷

Following a period of relative inaction, the 2010s witnessed the implementation of various interventions by different governments. These interventions generally aimed to take a more comprehensive and structural approach to EPL reform. In what follows, we delve into two major policy interventions that were put into effect between 2012 and 2017. These interventions differed in their scope and objectives, resulting in varying impacts on the dual EPL framework, as illustrated in the timeline depicted in Figure 1a.

²⁶The “Treu Package” included a reduction in penalties for violations of regulations regarding the conversion of temporary contracts into permanent ones. It also legalized temporary work agencies and introduced liberalized apprenticeship contracts, as well as regulations for “atypical” contracts. On the other hand, *Legislative Decree 368/2001* replaced the detailed list of specific reasons that firms were required to choose from in order to justify their use of fixed-term work relationships. Instead, it introduced a unique general reason having “technical, organizational, production, or replacement nature.” This change effectively relieved firms of the obligation to provide specific justifications for their use of temporary contracts.

²⁷Based on aggregate firm data from 8 collective bargaining agreements (CCNLs), which experienced a staggered implementation of the 2001 reform, Cappellari et al. (2012) did not find any significant impact of the fixed-term contract reform on the growth rate of such contracts. Instead, they found evidence pointing to increased use of external personnel. Furthermore, their findings revealed not only a substitution effect between temporary and permanent contracts ($\eta \approx 1$), but also a higher level of substitution between different types of temporary contracts ($\eta = 1.4$). They argued that this finding could be attributed to increased uncertainty surrounding the use of temporary contracts following the removal of the requirement for “specific reasons” to justify their usage. However, using a similar research design but employing matched employer-employee data with information from 121 CCNLs, Daruich et al. (2023) arrived at a different conclusion. Their study indicated that the reform resulted in an increase in the prevalence of temporary contracts. Additionally, they found that the reform led to significant wage decreases for temporary workers (both incumbent and new entrants), while not significantly affecting the earnings of permanent workers, thereby exacerbating earning disparities between these two types of workers.

The first of these, *Law 92/2012* (referred to as the “Fornero labor law”), which was implemented in June 2012, marked a notable departure from the policies of the previous decade (Massagli, 2018). This reform aimed to discourage the use of “non-standard” employment arrangements other than permanent contracts (such as temporary contracts, apprenticeships, and collaborations) by introducing various measures that made these alternatives more burdensome.²⁸ However, it is important to note that this reform also affected permanent contracts. Prior to this, Italy already had firing costs for permanent contracts contingent on firm size, with substantially higher costs for firms employing more than 15 employees.²⁹ The reform also made changes to the firing cost structure at the 15-employee threshold, significantly reducing it.³⁰

The second set of interventions is part of a broader set of policies collectively known as the “Jobs Act”. Here, we focus on three key measures within this framework that influenced the EPL gap. Similar to the prior reform, these measures affected both temporary and permanent contract margins, albeit not entirely in the same direction. The first of these interventions consisted of *Decree-Law 34/2014*, referred to as the “Decreto Poletti”, issued in March 2014 and later converted into *Law 78/2014* in May. This decree marked a sharp reversal in the regulations governing temporary contracts by drastically relaxing the legal constraints on their usage. This relaxation even surpassed the provisions introduced in the 2001 reform, for instance, by eliminating the requirement to provide *any* justification for

²⁸Regarding temporary contracts, the reform introduced several changes: (i) it raised existing social security contributions by 1.4% of pre-tax earnings, which could be redeemed if the contract was converted to permanent; (ii) it placed a cap of 36 months on the maximum duration of temporary contracts; (iii) it eliminated the requirement to specify a “generic” motivation for initial fixed-term contracts lasting less than 12 months, while also prohibiting the renewal of such “free” contracts; (iv) it extended the minimum time interval between contract renewals (from 10 to 60 days for contracts lasting less than 6 months and from 20 to 90 days for longer contracts). Additionally, the reform introduced restrictions on apprenticeships and imposed more bureaucratic requirements or even abolished other “atypical” contracts, such as collaboration contracts.

²⁹In Italy, permanent workers can be fired based on either misconduct (*giusta causa o giustificato motivo soggettivo*) or due to the firm’s re-organizational needs (*giustificato motivo oggettivo*). Prior to the “Fornero labor law”, if a dismissal of a permanent worker was deemed “unfair” by a court, firms with more than 15 employees, governed by Article 18 of *Law 300/1970* (“Statuto dei lavoratori”) at that time, had two options: reinstate the worker and compensate them for the lost earnings between the termination and the court’s decision, or pay them a severance payment equal to 15 months of salary. This exposed larger firms to considerably higher potential costs compared to those with fewer than 15 employees, who could choose between reinstatement (without compensation) and a severance payment ranging from 2.5 to 14 months based on seniority (Schivardi and Torrini, 2008; Hijzen et al., 2017; Bratti et al., 2021).

³⁰Regarding unfairly dismissed permanent workers in firms exceeding the threshold, the reform introduced several changes: (i) it decreased the monetary compensation owed to the worker and diversified it based on the specific circumstances of the case, (ii) it reduced the uncertainty related to the duration and costs of legal proceedings and expedited the legal process, and (iii) in certain instances, it restricted workers’ options to either reinstatement or receiving compensation (Bratti et al., 2021).

using temporary contracts of *any* duration.³¹

Conversely, the subsequent two “Jobs Act” policies, implemented shortly thereafter, intervened on the permanent contracts margin. These measures built upon the framework laid by the analogous margin-specific intervention in the “Fornero labor law” and reshaped it even further. Specifically, through *Legislative Decree 23/2015*, the government introduced a *new graded security permanent contract* for all newly hired permanent employees in firms of *all* sizes, effective from March 7th, 2015. This novel contract featured several key changes: (i) severance payments that increased with tenure, (ii) a phase-out of mandatory worker reinstatement in case of unfair dismissal that had been in force under the previous regime, and (iii) an additional reduction in judicial discretion in the legal process surrounding dismissals (Boeri and Garibaldi, 2019; Bovini and Viviano, 2018; Sestito and Viviano, 2018). The introduction of the graded security contract, therefore, significantly relaxed the regulations concerning firing costs for newly-hired permanent workers in firms with over 15 employees.

Additionally, in an attempt to further stimulate and accelerate the creation of permanent jobs, a few months prior to introducing the new graded security permanent contract, the government launched a highly generous but provisional (3-year duration) *hiring subsidy* in the *2015 Budget Law*, in the form of a rebate of employer-paid social security contributions. This subsidy applied to all new permanent hires made between January and December of 2015, under the condition that the hired individuals had not been employed under a permanent contract during the prior 6 months. The subsidy was proportional to gross wages but had an annual cap of 8,060 euros. As such, it was relatively more generous to small firms. Under analogous conditions, the subsidy was extended by the *2016 Budget Law*. However, the cap was more than halved (to 3,250 euros annually) and the subsidy’s overall duration shortened to 2 years. As a result, from 2017 onwards, all new permanent hires did not receive any rebate in social security contributions at all, while those that had previously benefited from the provisional hiring subsidy ceased to do so starting from January 1st, 2018.

A.2 Timing of the roll-out of the 2018 “Decreto Dignità” reform

January-February 2018

The election campaign reaches its climax as political parties publish their electoral programs. On January 18th, the 5 Star Movement, the party leading in the polls and advocating for the enactment of the temporary contracts reform once a government is formed, releases its program. Within the section titled “Labor Contracts”, the program states (emphasis in bold

³¹Furthermore, the “Decreto Poletti” (i) increased the maximum number of renewals from 1 to 5, (ii) reverted to the shorter time intervals between renewals as were in place before the “Fornero labor law”, (iii) and introduced a 20% cap on the share of temporary workforce within a firm.

as per the original text): “We envisage both the **reintroduction of causality for fixed-term contracts** and the provision, in cases of termination, extension or renewal of the fixed-term employment relationship, of an **additional allowance proportionate to the total remuneration**, due to the worker for the entire duration of the fixed-term employment contract. This allowance is aimed at making the fixed-term contract more favorable to the worker.”.³²

March-May 2018

The general election takes place on March 4th. The 5 Star Movement emerges as the political party with the largest share of votes in both branches of parliament (32.68% in the Chamber of Deputies and 32.22% in the Senate of the Republic).³³ As it secures a relative majority in parliament, the party is expected to lead efforts in forming a government. However, it needs to find coalition partner(s) in order to form a parliamentary majority capable of winning a vote of confidence supporting a new government. Negotiations are initiated to build such a majority. Following a prolonged period of considerable uncertainty (89 days, the longest in the history of the Italian Republic), the 5 Star Movement forms a majority coalition with the Northern League. This coalition leads to the establishment of a new government under the leadership of Prime Minister Giuseppe Conte on June 1st.

June 2018

Shortly after the new government assumed office, Luigi Di Maio, the leader of the 5 Star Movement and new Minister of Labor, grants his inaugural interview on June 19th to the national economic newspaper “Il Sole 24 Ore”. In this interview, he introduces the “Decreto Dignità”, largely confirming the details that were outlined in the January electoral program.³⁴

July-August 2018

The “Decreto Dignità” stands as the first legislative action of the new government. It enters into law upon the issuance of *Decree-Law 87/2018* on July 12th and subsequently undergoes conversion, with minor adjustments, into *Law 96/2018* on August 9th.

³²The original document can be retrieved at: <https://www.bollettinoadapt.it/wp-content/uploads/2018/02/Lavoro.pdf>.

³³Official election data from the Home Office available at: <https://elezionistorico.interno.gov.it/index.php?tpel=C&dtel=04/03/2018&tpa=I&tpe=A&lev0=0&levsut0=0&es0=S&ms=S>.

³⁴The interview is available at: https://www.ilsole24ore.com/art/parla-maio-piu-incentivi-lavoro-stabile-e-stretta-contratti-termini-AEzFsT8E?refresh_ce=1.

B Data Appendix

B.1 Sector-level benchmarking of online job vacancy data

Online job vacancy data have been used in recent economic research. However, a major concern is how accurately they reflect job openings across the entire economy, given that certain sectors and occupations might be less likely to advertise vacancies online. To tackle this concern, various studies utilizing BGT data for the US (e.g, Carnevale et al., 2014; Hershbein and Kahn, 2018; Deming and Kahn, 2018; Acemoglu et al., 2022; Chetty et al., 2020) have assessed their representativeness by comparing them against nationally representative sources like the Job Openings and Labor Market Turnover Survey (JOLTS) by the Bureau of Labor Statistics (BLS) and the Help Wanted Online (HWOL) index from the Conference Board. These studies have consistently found that aggregate figures (e.g., by sector or occupation) for the number of job vacancies derived from BGT data closely align with those obtained from these alternative sources.

We conduct a similar benchmarking exercise to assess the representativeness of the WollyBi data by comparing them with official statistics by ISTAT, the Italian National Statistics Office. ISTAT collects data on job vacancies from two sources: VELA, a survey on job vacancies and hours worked for firms with 1-499 employees (*Rilevazione trimestrale sui posti vacanti e le ore lavorate*), and LES, a census on various labor market indicators in larger firms (*Rilevazione mensile sull'occupazione, gli orari di lavoro, le retribuzioni e il costo del lavoro nelle Grandi Imprese*). These two sources are combined to produce official statistics on job vacancies which are publicly available via Eurostat, the statistical office of the European Union.³⁵

Unlike the US, for Italy ISTAT does not publicly disclose specific figures regarding the *number* of job vacancies. Instead, it provides quarterly statistics on the job *vacancy rate*, defined as follows:³⁶

$$\text{vacancy rate}_t = \frac{\# \text{vacancies}_t}{\# \text{vacancies}_t + \# \text{filled jobs}_t} \quad (7)$$

where t represents calendar quarters, and the number of vacancies and filled jobs refer to dependent workers. These vacancy rate figures have been available starting from 2016. However, these rates are released on a 1-digit NACE Rev.2 sector level, but not by occupation.³⁷

³⁵The metadata of the official statistics dataset used for the benchmarking exercise are available at the following web page: https://ec.europa.eu/eurostat/cache/metadata/EN/jvs_esqrs_it.htm.

³⁶The ISTAT definition of the vacancy rate is stated in the document accessible at the following web page https://www.istat.it/it/files//2022/06/EN_labour_market_Q1_2022.pdf.

³⁷Specifically, the release covers the non-agricultural private sector (NACE sections B to S). The statistic

To establish a basis for comparison, we derive an estimate of the vacancy rate using the WollyBi data. To accomplish this, we use additional data on filled jobs for dependent workers from ISTAT’s Statistical Register of Active Enterprises (ASIA). This data is available at the 1-digit sector by year level via ISTAT’s website.³⁸ The estimation of vacancy rates involves three steps: first, we calculate the average number of vacancies (excluding self-employed positions) for each sector within a given month using the WollyBi data; second, we compute year-level averages of these sector averages; third, we utilize the aggregates derived from the first two steps in conjunction with ASIA’s aggregate data to compute sector-by-year vacancy rates by applying the definition provided in Equation (7). In order to compare these estimates with ISTAT’s sector-by-quarter vacancy rates, we compute year-level averages of the latter.

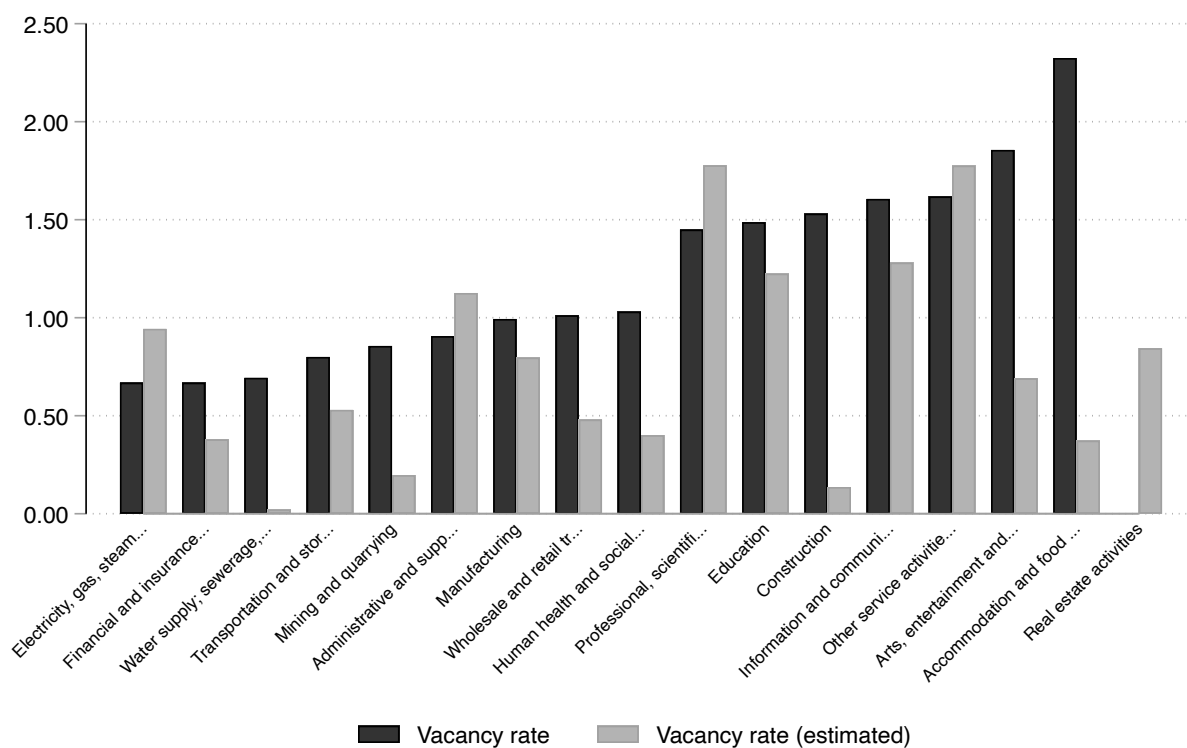
Figure B.1 presents average vacancy rates across the 2016-2019 period, by sector. The black bars represent ISTAT’s vacancy rates, while the grey bars refer to the estimated rates. Overall, the figure reveals a substantial overlap between the two sets of statistics, particularly evident in sectors such as manufacturing and services associated with professional activities, administrative tasks, education, and ICT. However, it is evident that sectors like water supply and waste management, mining and quarrying, construction, hospitality, and arts and entertainment seem to be considerably underrepresented in the WollyBi data.

Figure B.2 presents a comparison of trends between the two sets of statistics over the years 2016-2019. Generally, these trends appear to be consistent, although there is an exception for professional activities, education, and other services, where the estimated vacancy rates experience a noticeable spike in 2019, potentially due to noise in the year-level estimation.

for section L, “Real estate activities”, is confidential.

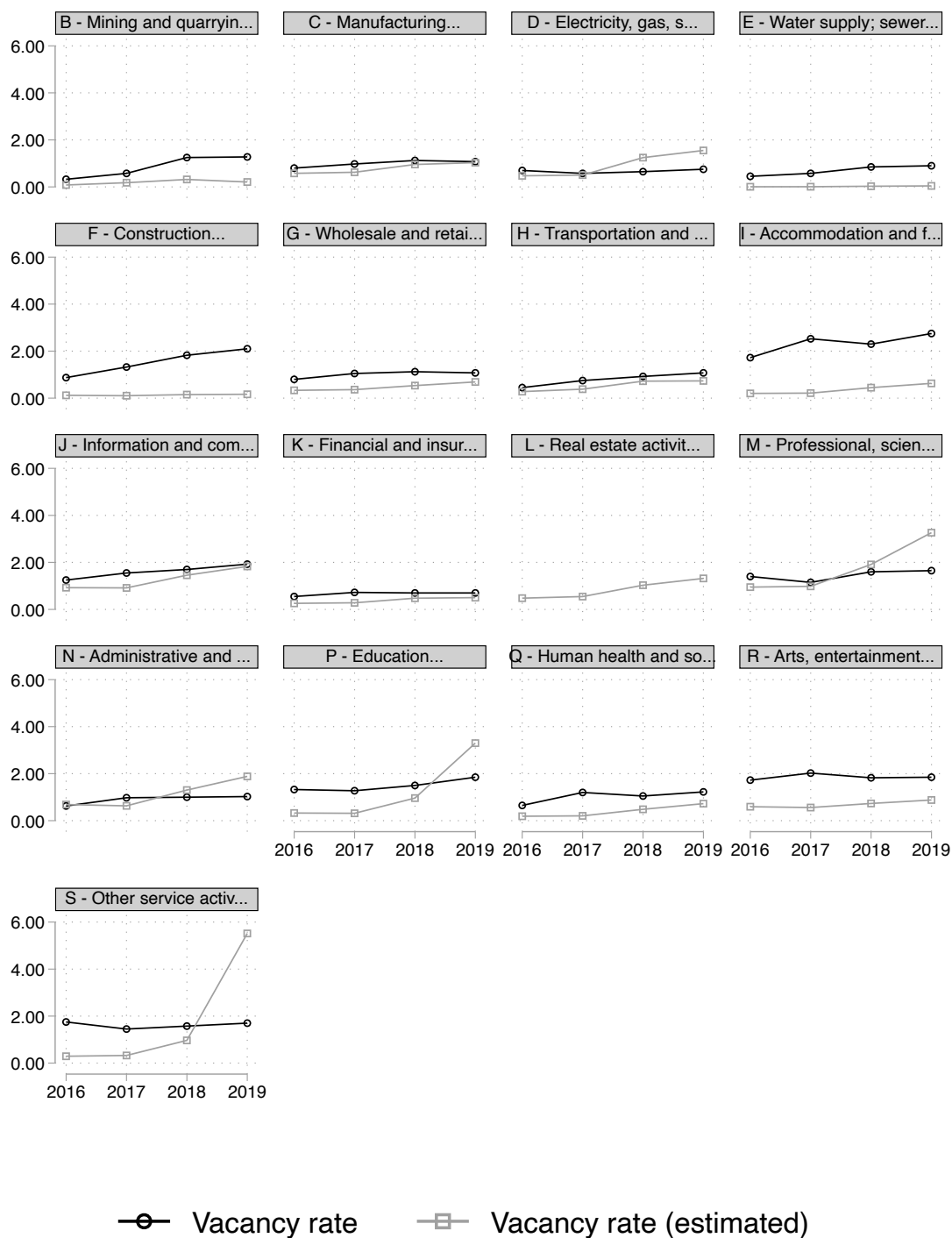
³⁸The aggregate data can be downloaded from the following web page: <http://dati.istat.it/?lang=en>.

Figure B.1: Comparison of ISTAT's and estimated vacancy rates, by sector
(averages over the 2016-2019)



Note: Black bars represent the vacancy rate published by ISTAT via Eurostat, by 1-digit sector. Grey bars are estimated vacancy rates based on WollyBi data (for vacancies) and ASIA data (for filled positions).

Figure B.2: Comparison of ISTAT's and estimated vacancy rates, by sector
(trends over the years 2016-2019)



Note: Black lines represent the vacancy rate published by ISTAT via Eurostat, by 1-digit sector and year. Grey lines are estimated vacancy rates based on WollyBi data (for vacancies) and ASIA data (for filled positions).

B.2 Further descriptive statistics

Table B.1: Descriptive statistics on requirements posted in online job vacancies (OJVs)
(disaggregate job requirements)

	Mean	(Std. Dev.)
<i>Contract</i>		
Contract type stated	0.949	(0.219)
Permanent contract	0.241	(0.428)
Temporary contract	0.447	(0.497)
Self-employed position	0.165	(0.371)
Internship	0.096	(0.294)
<i>Education</i>		
Education requirement stated	0.998	(0.046)
Primary	0.020	(0.139)
Lower Secondary	0.001	(0.033)
Upper Secondary	0.655	(0.475)
Post-Secondary (Non-Tertiary)	0.087	(0.281)
Tertiary (Short-cycle)	0.006	(0.077)
Tertiary (Bachelor)	0.156	(0.362)
Tertiary (Master's')	0.068	(0.251)
Tertiary (PhD)	0.006	(0.078)
<i>Experience</i>		
Experience requirement stated	0.616	(0.486)
0 years	0.144	(0.351)
<=1 years	0.034	(0.180)
1-2 years	0.145	(0.353)
2-4 years	0.184	(0.388)
4-6 years	0.033	(0.178)
6-8 years	0.006	(0.075)
8-10 years	0.009	(0.097)
>=10 years	0.060	(0.238)
<i>Specific skills</i>		
Specific skill requirements stated	0.851	(0.356)
Cognitive	0.492	(0.500)
Social	0.566	(0.496)
Character	0.521	(0.500)
Writing	0.013	(0.113)
Customer	0.392	(0.488)
Project management	0.190	(0.393)
People management	0.233	(0.422)
Financial	0.117	(0.322)
Computer	0.369	(0.483)
Software	0.225	(0.418)
N of online job vacancies (OJVs)	3,331,650	

Note: The table reports descriptive statistics on disaggregate job requirements—i.e., contract type, and human capital and specific skills requirements—mentioned in the online job vacancies contained in the WollyBi data. Vacancies are weighted by the size of the labor force of the province in which the ad was posted (fixed at the level of 2017, the last pre-reform year).

Table B.2: Descriptive statistics on requirements posted in online job vacancies (OJVs)
(disaggregate job requirements), by contract

	Permanent		Temporary		Self Employment		Internship	
	Mean	(Std. Dev.)	Mean	(Std. Dev.)	Mean	(Std. Dev.)	Mean	(Std. Dev.)
<i>Education</i>								
Education requirement stated	0.998	(0.045)	0.998	(0.046)	0.998	(0.047)	0.999	(0.036)
Primary	0.010	(0.100)	0.032	(0.176)	0.008	(0.087)	0.007	(0.083)
Lower Secondary	0.001	(0.038)	0.001	(0.027)	0.000	(0.016)	0.001	(0.037)
Upper Secondary	0.577	(0.494)	0.719	(0.450)	0.676	(0.468)	0.494	(0.500)
Post-Secondary (Non-Tertiary)	0.113	(0.317)	0.065	(0.247)	0.107	(0.310)	0.087	(0.282)
Tertiary (Short-cycle)	0.005	(0.073)	0.005	(0.072)	0.006	(0.080)	0.006	(0.079)
Tertiary (Bachelor)	0.216	(0.412)	0.119	(0.324)	0.119	(0.324)	0.265	(0.441)
Tertiary (Master's')	0.067	(0.251)	0.051	(0.220)	0.075	(0.263)	0.135	(0.341)
Tertiary (PhD)	0.007	(0.082)	0.006	(0.077)	0.006	(0.078)	0.004	(0.066)
<i>Experience</i>								
Experience requirement stated	0.589	(0.492)	0.660	(0.474)	0.594	(0.491)	0.633	(0.482)
0 years	0.073	(0.260)	0.140	(0.347)	0.177	(0.382)	0.339	(0.473)
<=1 years	0.042	(0.202)	0.028	(0.164)	0.027	(0.162)	0.055	(0.228)
1-2 years	0.103	(0.304)	0.198	(0.398)	0.111	(0.314)	0.107	(0.309)
2-4 years	0.209	(0.407)	0.211	(0.408)	0.157	(0.364)	0.065	(0.246)
4-6 years	0.053	(0.223)	0.033	(0.178)	0.023	(0.151)	0.005	(0.070)
6-8 years	0.008	(0.092)	0.006	(0.075)	0.003	(0.059)	0.003	(0.058)
8-10 years	0.018	(0.132)	0.008	(0.091)	0.006	(0.078)	0.002	(0.043)
>=10 years	0.083	(0.276)	0.037	(0.188)	0.088	(0.284)	0.057	(0.231)
<i>Specific skills</i>								
Specific skill requirements stated	0.923	(0.266)	0.802	(0.398)	0.881	(0.323)	0.912	(0.284)
Cognitive	0.616	(0.486)	0.402	(0.490)	0.567	(0.496)	0.520	(0.500)
Social	0.684	(0.465)	0.482	(0.500)	0.606	(0.489)	0.685	(0.464)
Character	0.626	(0.484)	0.446	(0.497)	0.559	(0.497)	0.620	(0.485)
Writing	0.018	(0.134)	0.010	(0.098)	0.015	(0.120)	0.015	(0.122)
Customer	0.433	(0.495)	0.358	(0.480)	0.437	(0.496)	0.432	(0.495)
Project management	0.274	(0.446)	0.144	(0.351)	0.201	(0.401)	0.200	(0.400)
People management	0.302	(0.459)	0.187	(0.390)	0.253	(0.435)	0.244	(0.430)
Financial	0.149	(0.356)	0.105	(0.307)	0.116	(0.321)	0.116	(0.320)
Computer	0.485	(0.500)	0.295	(0.456)	0.391	(0.488)	0.440	(0.496)
Software	0.330	(0.470)	0.156	(0.363)	0.267	(0.442)	0.251	(0.434)
N of online job vacancies (OJVs)	804,332		1,489,804		549,927		318,638	

Note: The table reports descriptive statistics on disaggregate job requirements—i.e., human capital and specific skills requirements—mentioned in the online job vacancies contained in the WollyBi data, by type of contract. Vacancies are weighted by the size of the labor force of the province in which the ad was posted (fixed at the level of 2017, the last pre-reform year).

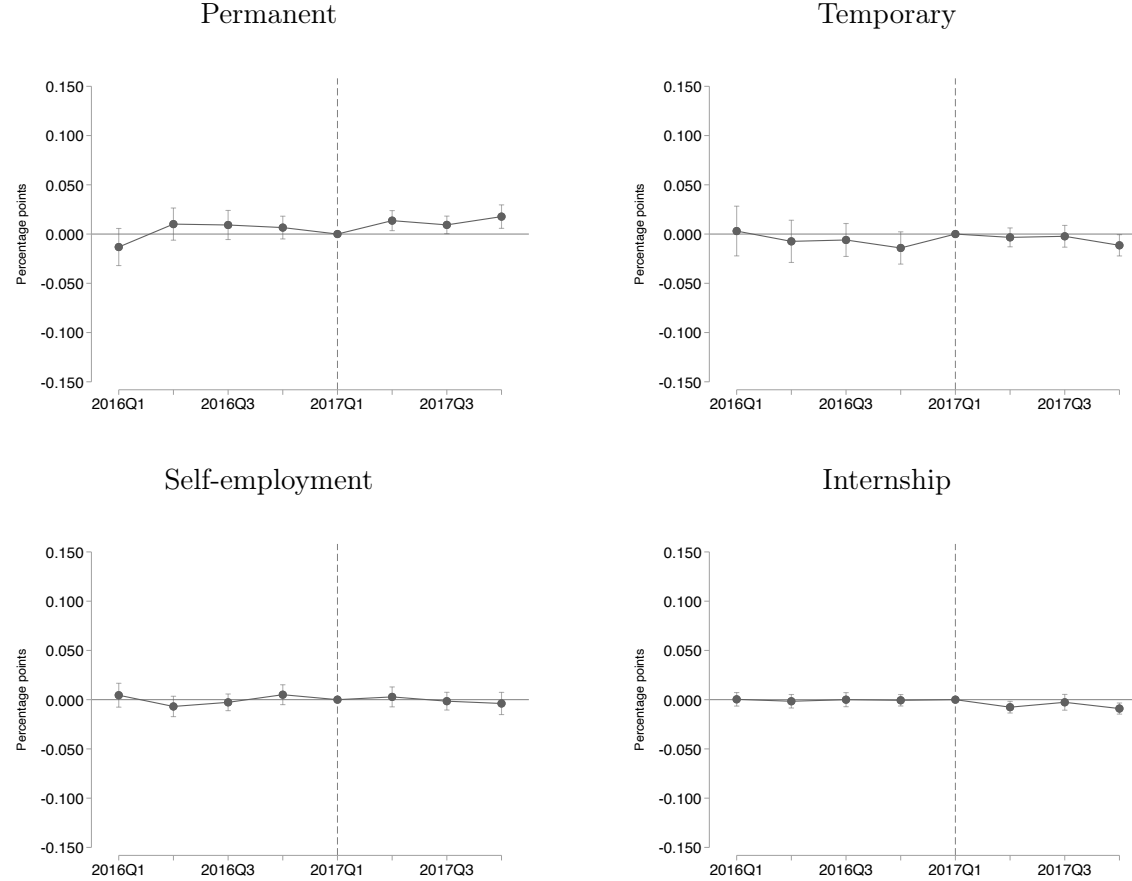
Table B.3: Descriptive statistics on number of online job vacancies (OJVs) posted per given cells.

	Mean	Min	P25	P50	P75	Max
Ads posted per Province-Sector-Occupation cell (N cells=13,383)	248.95	1	9	36	143	35,188
Ads posted per Province-Sector-Occupation-Quarter cell (N cells=109,861)	30.33	1	2	6	20	5,107

Note: The table reports detailed descriptive statistics on the number of (unweighted) online job vacancies posted within each province-sector-occupation and province-sector-occupation-quarter cell.

C Robustness checks

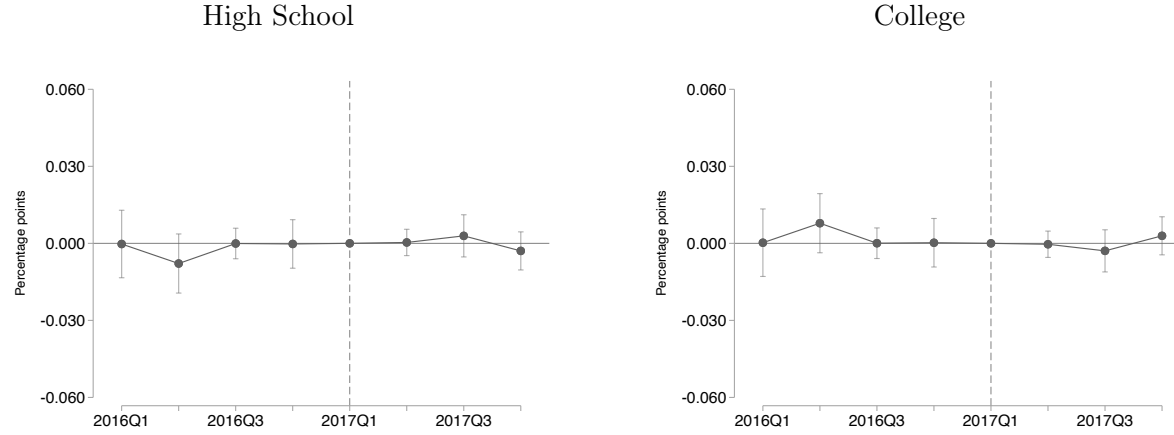
Figure C.1: Labor demand by contract type (Overall: out of all vacancies.)
Placebo test: $t \in [2016Q1, 2017Q4]$.



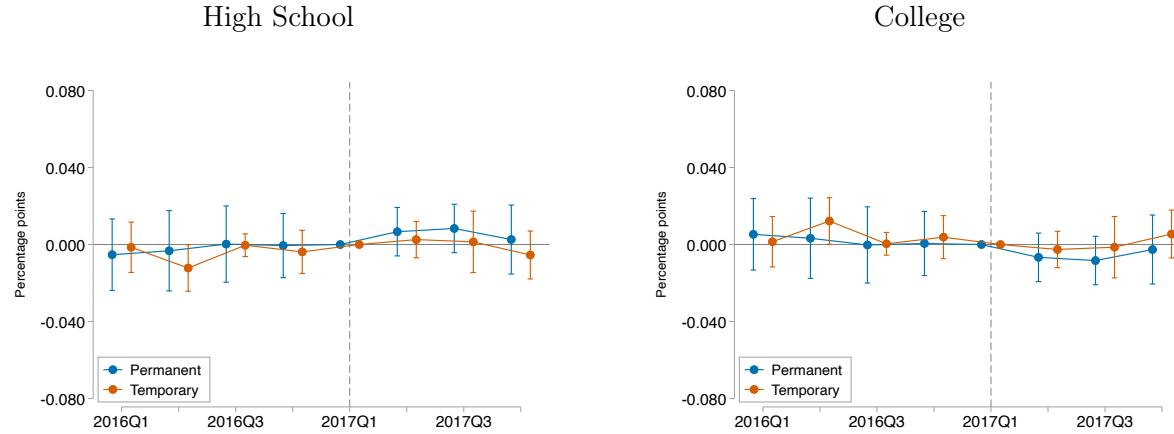
Note: The figure presents placebo tests using the specification of Equation (4). These tests involve shifting the time frame to $[2016Q1, 2017Q4]$ (given that the reform was introduced in 2018Q3) to estimate the treatment effects on the share of total vacancies that offer job positions under different contract types: permanent contract (top-left), temporary contract (top-right), self-employment (bottom-left), and internship (bottom-right). The outcome shares are calculated within each province-sector-occupation-calendar quarter cell, which serves as the unit of analysis. The treatment indicator takes a value of 1 for provinces with high exposure to the reform (defined as $exposure_p \geq p50$) and 0 for provinces with low exposure ($exposure_p < p50$). The estimated treatment effect coefficients $\{\theta_\tau\}_{\tau \neq 2017Q1}$ are plotted alongside their corresponding 95% confidence intervals. These coefficients capture the differences in outcomes at each quarter between provinces with high and low exposure to the reform relative to the reference period 2017Q1. Standard errors are clustered at the province level p . In the regressions analysis, each cell-level observation is weighted by the total labor force in the corresponding province-sector-occupation cell for 2017, multiplied by the cell's share of ads within each calendar quarter.

Figure C.2: Education requirements.
Placebo test: $t \in [2016Q1, 2017Q4]$.

a. *All vacancies*

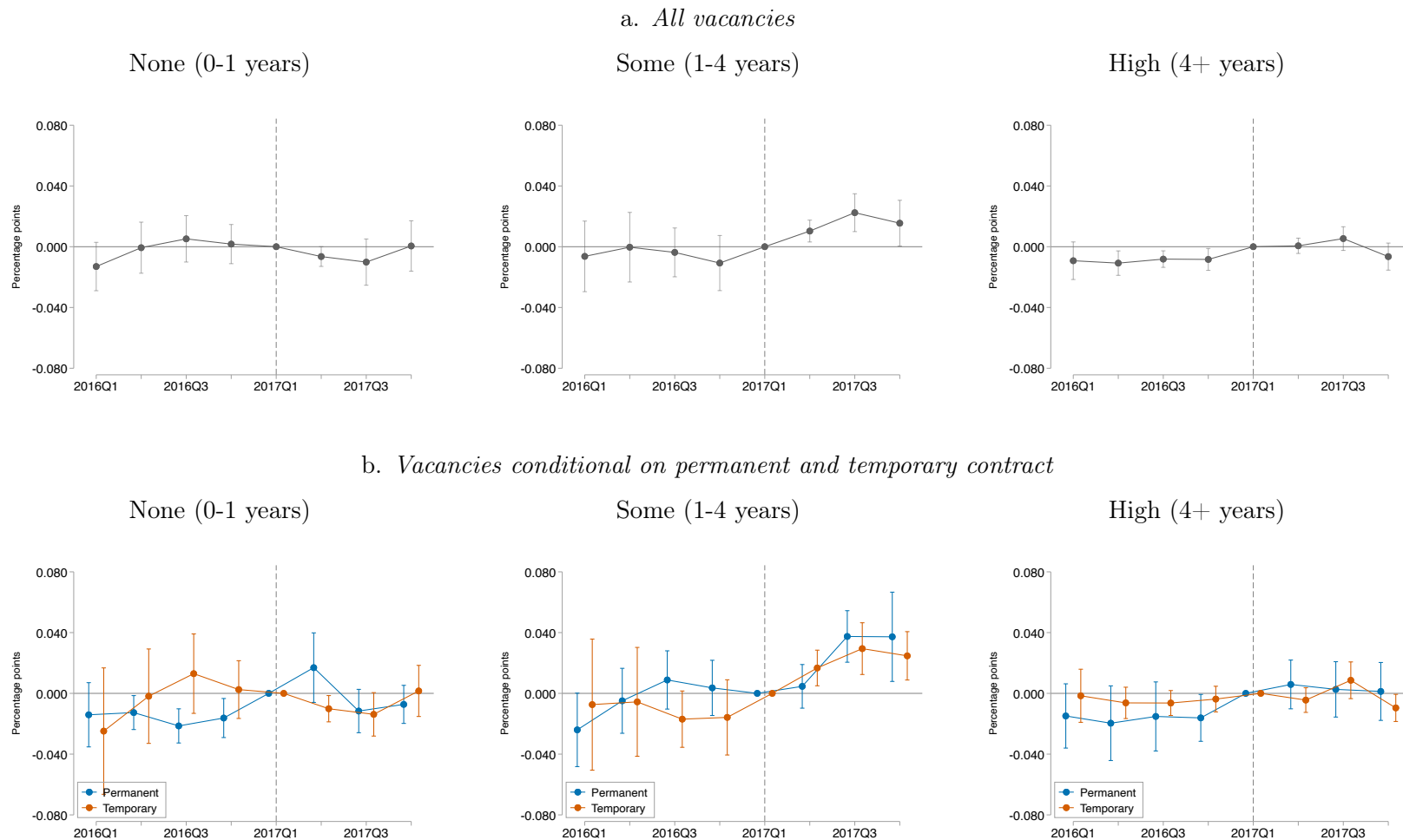


b. *Vacancies conditional on permanent and temporary contract*



Note: The figure presents placebo tests using the specification of Equation (4). These tests involve shifting the time frame to $[2016Q1, 2017Q4]$ (given that the reform was introduced in 2018Q3) to estimate the treatment effects on the share of vacancies (out of total job vacancies in panel a, and out of permanent or temporary job vacancies in panel b) requiring up to secondary education (high school in the left column), and post-secondary education (college degree in the right column). The outcome shares are calculated within each province-sector-occupation-calendar quarter cell, which serves as the unit of analysis. The treatment indicator takes a value of 1 for provinces with high exposure to the reform (defined as $exposure_p \geq p50$) and 0 for provinces with low exposure ($exposure_p < p50$). The estimated treatment effect coefficients $\{\theta_\tau\}_{\tau \neq 2017Q1}$ are plotted alongside their corresponding 95% confidence intervals. These coefficients capture the differences in outcomes at each quarter between provinces with high and low exposure to the reform relative to the reference period 2017Q1. Standard errors are clustered at the province level p . In the regressions analysis, each cell-level observation is weighted by the total labor force in the corresponding province-sector-occupation cell for 2017, multiplied by the cell's share of ads within each calendar quarter.

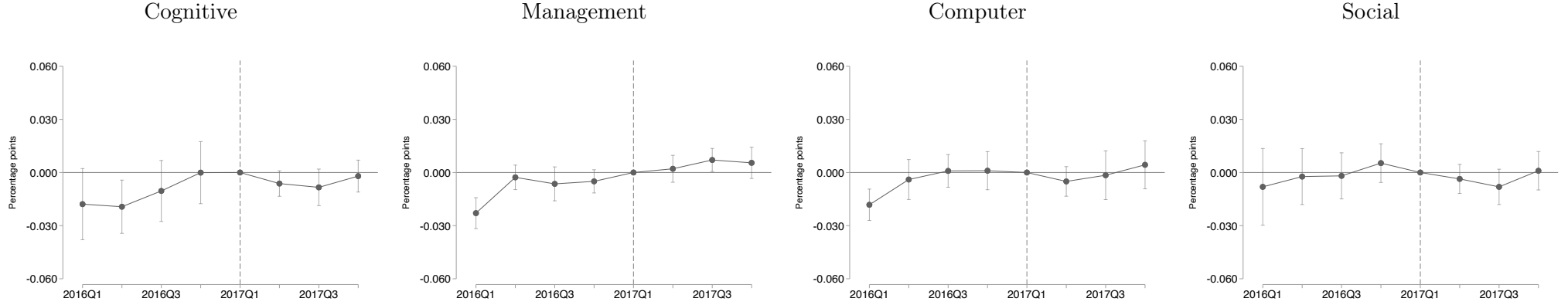
Figure C.3: Experience requirements.
Placebo test: $t \in [2016Q1, 2017Q4]$.



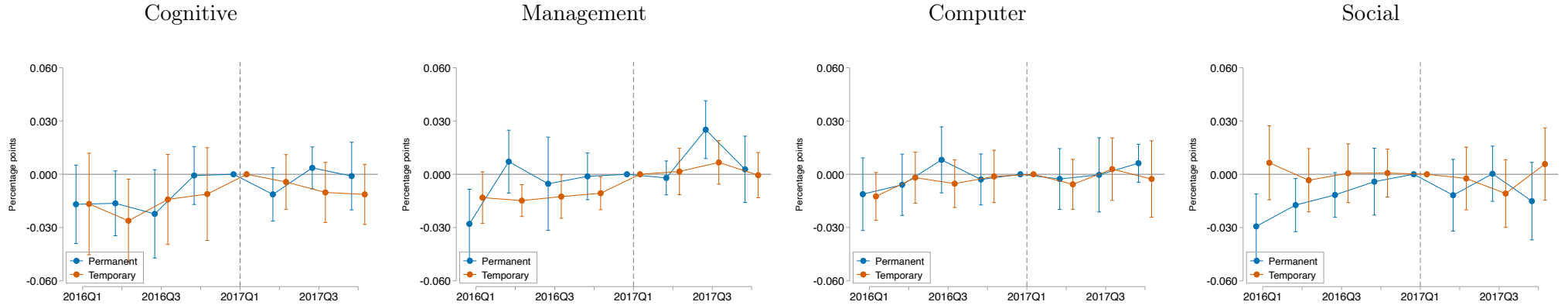
Note: The figure presents placebo tests using the specification of Equation (4). These tests involve shifting the time frame to $[2016Q1, 2017Q4]$ (given that the reform was introduced in 2018Q3) to estimate the treatment effects on the share of vacancies (out of total job vacancies in panel a, and out of permanent or temporary job vacancies in panel b) requiring no experience (0-1 years in the left column), some experience (1-4 years in the center column) and high experience (4+ year in the right column). The outcome shares are calculated within each province-sector-occupation-calendar quarter cell, which serves as the unit of analysis. The treatment indicator takes a value of 1 for provinces with high exposure to the reform (defined as $exposure_p \geq p50$) and 0 for provinces with low exposure ($exposure_p < p50$). The estimated treatment effect coefficients $\{\theta_\tau\}_{\tau \neq 2017Q1}$ are plotted alongside their corresponding 95% confidence intervals. These coefficients capture the differences in outcomes at each quarter between provinces with high and low exposure to the reform relative to the reference period 2017Q1. Standard errors are clustered at the province level p . In the regressions analysis, each cell-level observation is weighted by the total labor force in the corresponding province-sector-occupation cell for 2017, multiplied by the cell's share of ads within each calendar quarter.

Figure C.4: Specific skill requirements.
 Placebo test: $t \in [2016Q1, 2017Q4]$.

a. *All vacancies*

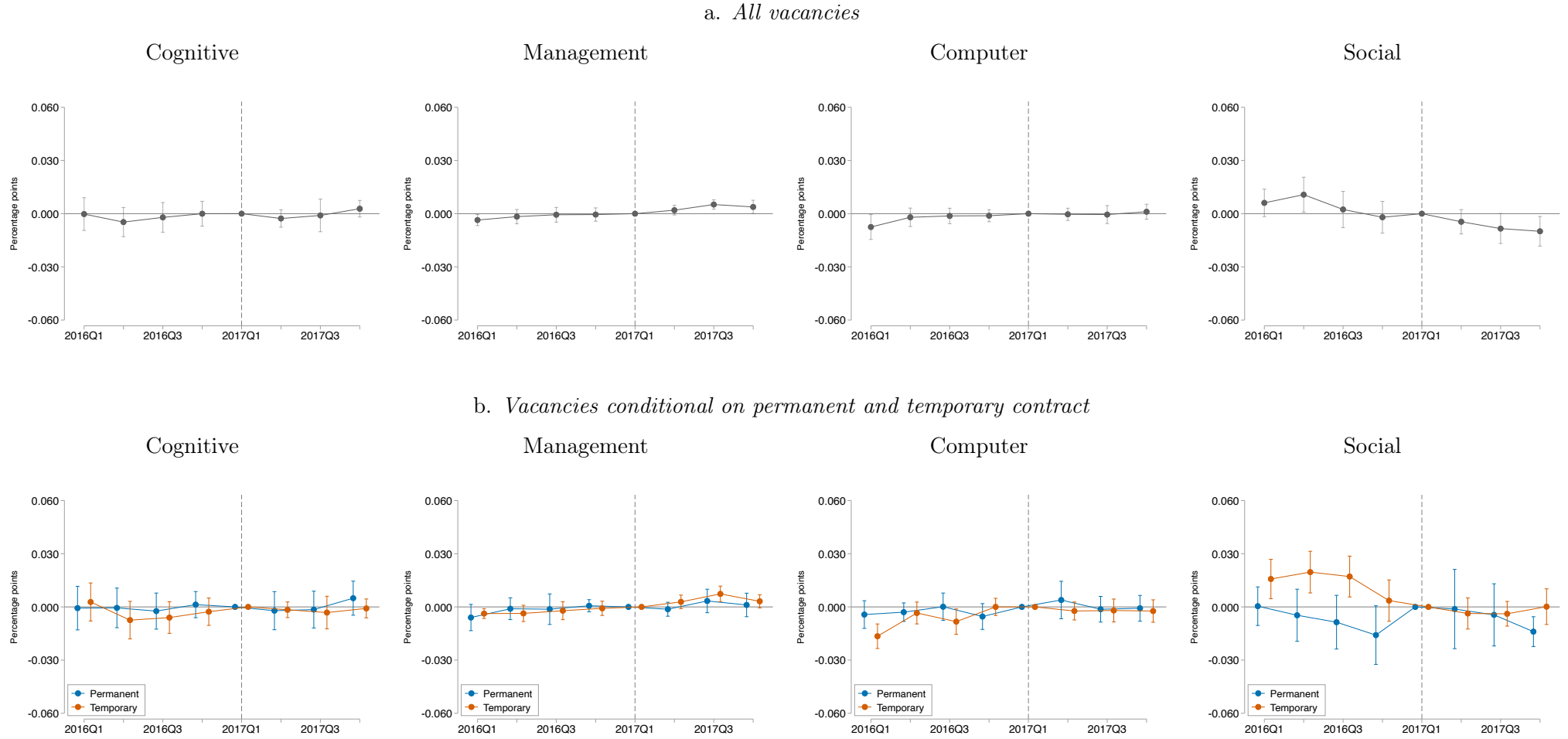


b. *Vacancies conditional on permanent and temporary contract*



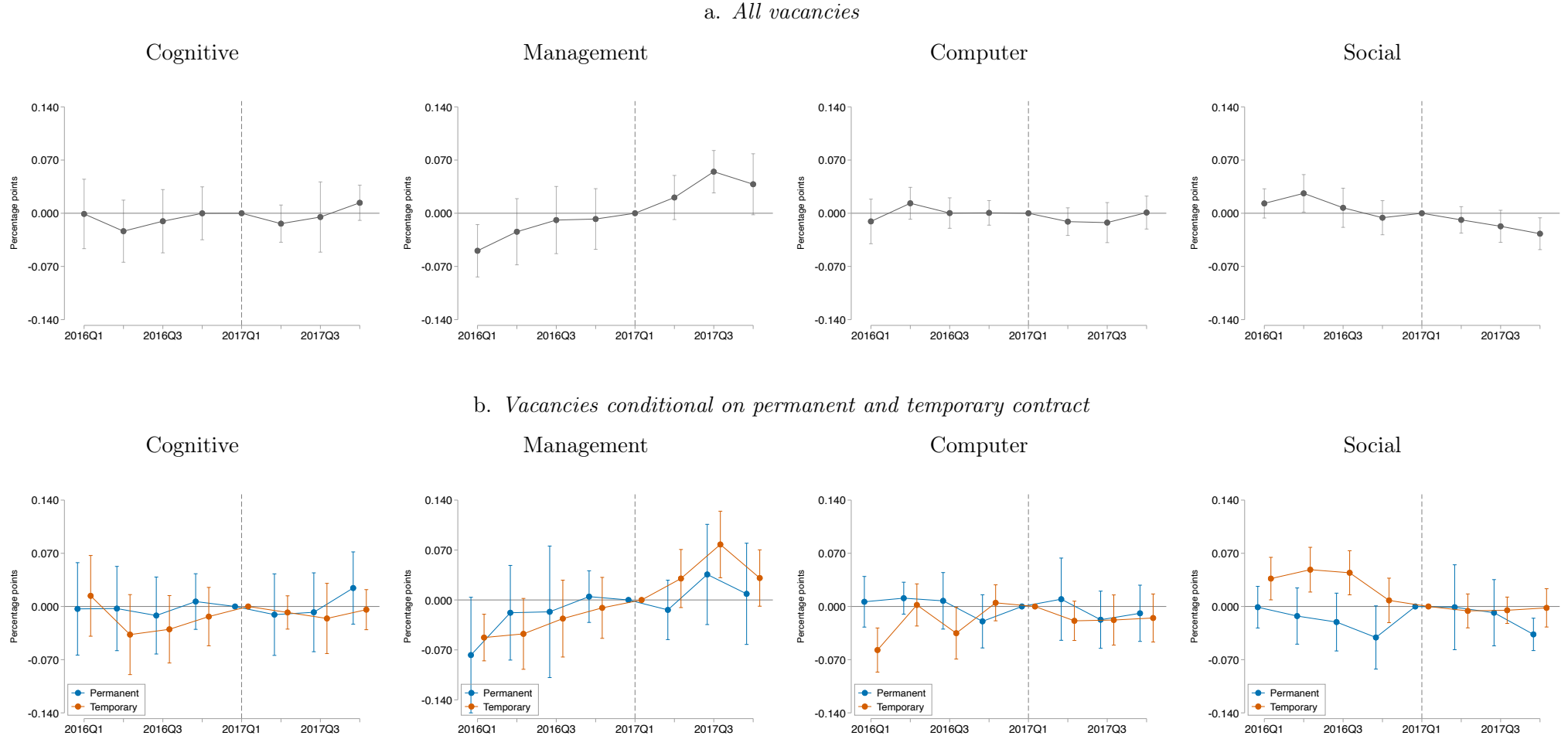
Note: The figure presents placebo tests using the specification of Equation (4). These tests involve shifting the time frame to $[2016Q1, 2017Q4]$ (given that the reform was introduced in 2018Q3) to estimate the treatment effects on the share of vacancies (out of total job vacancies in panel a, and out of permanent or temporary job vacancies in panel b) requiring cognitive (first column), management (second column), computer (third column), and social (fourth column) skills. The outcome shares are calculated within each province-sector-occupation-calendar quarter cell, which serves as the unit of analysis. The treatment indicator takes a value of 1 for provinces with high exposure to the reform (defined as $exposure_p \geq p50$) and 0 for provinces with low exposure ($exposure_p < p50$). The estimated treatment effect coefficients $\{\theta_\tau\}_{\tau \neq 2017Q1}$ are plotted alongside their corresponding 95% confidence intervals. These coefficients capture the differences in outcomes at each quarter between provinces with high and low exposure to the reform relative to the reference period 2017Q1. Standard errors are clustered at the province level p . In the regressions analysis, each cell-level observation is weighted by the total labor force in the corresponding province-sector-occupation cell for 2017, multiplied by the cell's share of ads within each calendar quarter.

Figure C.5: Specific skill requirements, *within*-cells relative importance.
 Placebo test: $t \in [2016Q1, 2017Q4]$.



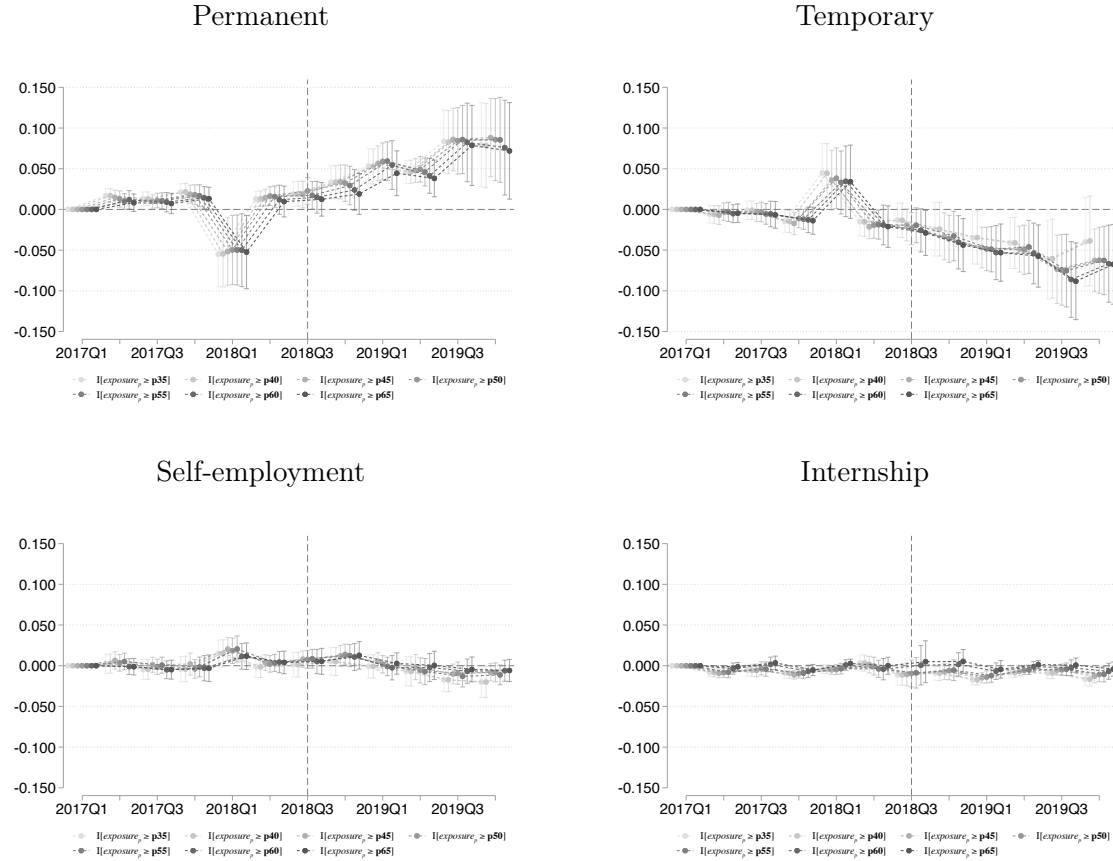
Note: The figure presents placebo tests using the specification of Equation (4). These tests involve shifting the time frame to $[2016Q1, 2017Q4]$ (given that the reform was introduced in 2018Q3) to estimate the treatment effects on the *within*-cells relative importance indicators (for total job vacancies in panel a, and for permanent or temporary job vacancies in panel b) for cognitive (first column), management (second column), computer (third column), and social (fourth column) skills. The within-cells relative importance indicators are calculated within each province-sector-occupation-calendar quarter cell, which serves as the unit of analysis. The treatment indicator takes a value of 1 for provinces with high exposure to the reform (defined as $exposure_p \geq p50$) and 0 for provinces with low exposure ($exposure_p < p50$). The estimated treatment effect coefficients $\{\theta_\tau\}_{\tau \neq 2017Q1}$ are plotted alongside their corresponding 95% confidence intervals. These coefficients capture the differences in outcomes at each quarter between provinces with high and low exposure to the reform relative to the reference period 2017Q1. Standard errors are clustered at the province level p . In the regressions analysis, each cell-level observation is weighted by the total labor force in the corresponding province-sector-occupation cell for 2017, multiplied by the cell's share of ads within each calendar quarter.

Figure C.6: Specific skill requirements, *between*-cells relative importance.
 Placebo test: $t \in [2016Q1, 2017Q4]$.



Note: The figure presents placebo tests using the specification of Equation (4). These tests involve shifting the time frame to $[2016Q1, 2017Q4]$ (given that the reform was introduced in 2018Q3) to estimate the treatment effects on the *between*-cells relative importance indicators (for total job vacancies in panel a, and for permanent or temporary job vacancies in panel b) for cognitive (first column), management (second column), computer (third column), and social (fourth column) skills. The between-cells relative importance indicators are calculated within each province-sector-occupation-calendar quarter cell, which serves as the unit of analysis. The treatment indicator takes a value of 1 for provinces with high exposure to the reform (defined as $exposure_p \geq p50$) and 0 for provinces with low exposure ($exposure_p < p50$). The estimated treatment effect coefficients $\{\theta_\tau\}_{\tau \neq 2017Q1}$ are plotted alongside their corresponding 95% confidence intervals. These coefficients capture the differences in outcomes at each quarter between provinces with high and low exposure to the reform relative to the reference period 2017Q1. Standard errors are clustered at the province level p . In the regressions analysis, each cell-level observation is weighted by the total labor force in the corresponding province-sector-occupation cell for 2017, multiplied by the cell's share of ads within each calendar quarter.

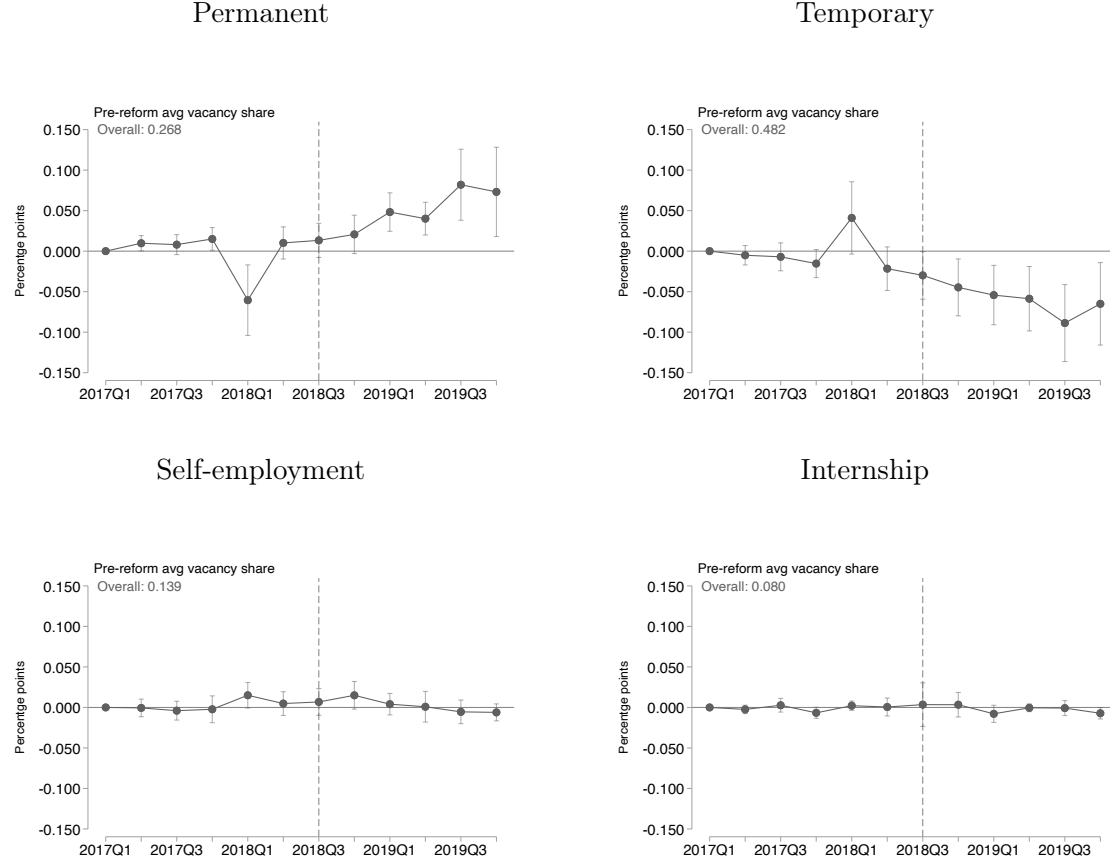
Figure C.7: Labor demand by contract type (Overall: out of all vacancies.)
Cutoff sensitivity: $\mathbb{1}[exposure_p \geq c]$ with $c \in \{p35, p40, p45, p50, p55, p60, p65\}$.



Note: The figure presents a robustness test using the specification of Equation (4) for the sensitivity of the main estimates of the impact of the reform on the share of total vacancies offering job positions under permanent contracts (top left), temporary contracts (top right), self-employment (bottom left), and internships (bottom right) to alternative definitions of the treatment indicator variable considering cutoffs (denoted as c) such as $p35, p40, p45, p55, p60$, or $p65$. The treatment indicator takes a value of 1 for provinces with high exposure to the reform (defined as $exposure_p \geq c$) and 0 for provinces with low exposure ($exposure_p < c$). The outcome shares are calculated within each province-sector-occupation-calendar quarter cell, which serves as the unit of analysis. The estimated treatment effect coefficients $\{\theta_\tau\}_{\tau \neq 2017Q1}$ are plotted alongside their corresponding 95% confidence intervals. These coefficients capture the differences in outcomes at each quarter between provinces with high and low exposure to the reform relative to the reference period 2017Q1. The reform was implemented in 2018Q3. Standard errors are clustered at the province level p . In the regressions analysis, each cell-level observation is weighted by the total labor force in the corresponding province-sector-occupation cell for 2017, multiplied by the cell's share of ads within each calendar quarter.

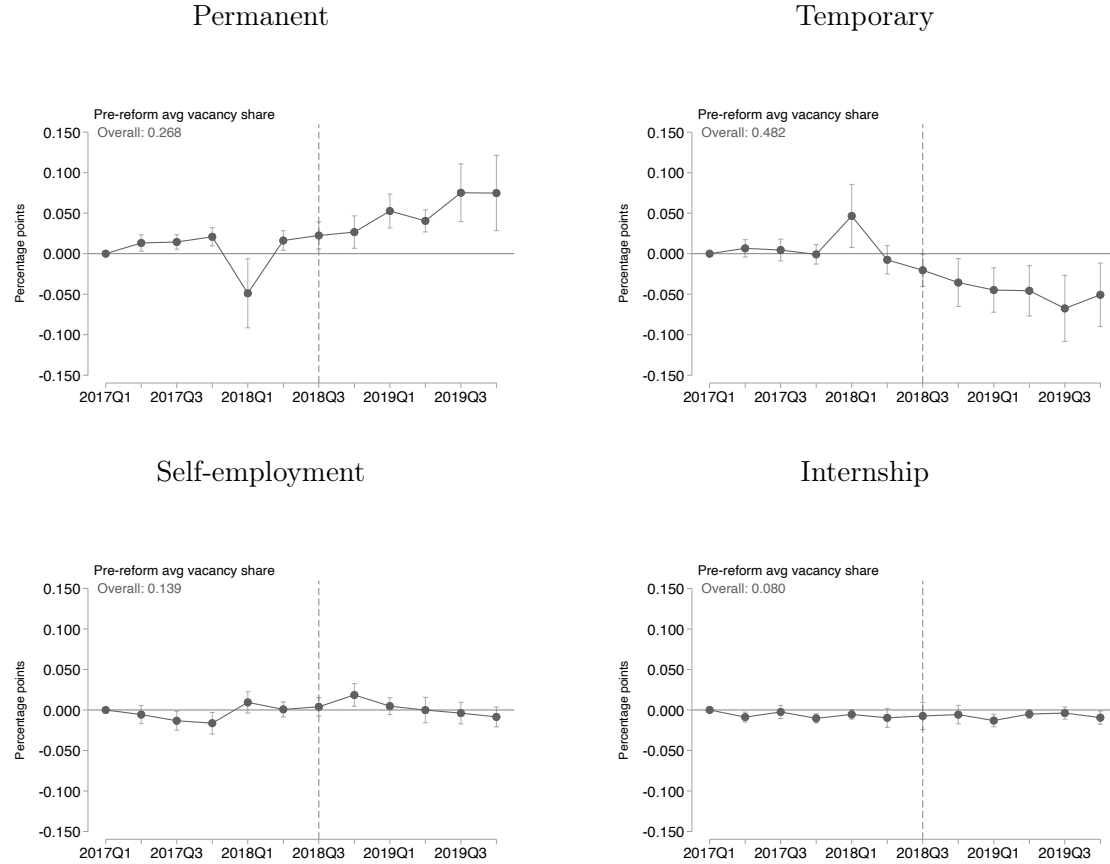
Figure C.8: Labor demand by contract type (Overall: out of all vacancies.)

Indicator variable sensitivity: exposure-to-treatment indicator =
$$\begin{cases} 1 & \text{exposure}_p \in \text{top tercile} \\ 0 & \text{exposure}_p \in \text{bottom tercile} \end{cases}$$



Note: The figure presents a robustness test using the specification of Equation (4) for the sensitivity of the main estimates of the impact of the reform on the share of total vacancies offering job positions under permanent contracts (top left), temporary contracts (top right), self-employment (bottom left), and internships (bottom right) to an alternative definition of the treatment indicator variable. The treatment indicator equals 1 if exposure_p falls within the top tercile of its distribution, and 0 if it lies within the bottom tercile. This test involves excluding observations for provinces with medium tercile exposure. The outcome shares are calculated within each province-sector-occupation-calendar quarter cell, which serves as the unit of analysis. The estimated treatment effect coefficients $\{\theta_\tau\}_{\tau \neq 2017Q1}$ are plotted alongside their corresponding 95% confidence intervals. These coefficients capture the differences in outcomes at each quarter between provinces with high and low exposure to the reform relative to the reference period 2017Q1. The reform was implemented in 2018Q3. Standard errors are clustered at the province level p . In the regressions analysis, each cell-level observation is weighted by the total labor force in the corresponding province-sector-occupation cell for 2017, multiplied by the cell's share of ads within each calendar quarter.

Figure C.9: Labor demand by contract type (Overall: out of all vacancies.)
Specification with additional sector by calendar quarter fixed effects.



Note: The figure shows the impact of the reform on the share of total vacancies offering job positions under permanent contracts (top left), temporary contracts (top right), self-employment (bottom left), and internships (bottom right) from an extended specification of Equation (4) that includes sector by calendar quarter fixed effects. These fixed effects capture sector-specific changes over time that might influence labor demand. The outcome shares are calculated within each province-sector-occupation-calendar quarter cell, which serves as the unit of analysis. The treatment indicator takes a value of 1 for provinces with high exposure to the reform (defined as $exposure_p \geq p50$) and 0 for provinces with low exposure ($exposure_p < p50$). The estimated treatment effect coefficients $\{\theta_\tau\}_{\tau \neq 2017Q1}$ are plotted alongside their corresponding 95% confidence intervals. These coefficients capture the differences in outcomes at each quarter between provinces with high and low exposure to the reform relative to the reference period 2017Q1. The reform was implemented in 2018Q3. Standard errors are clustered at the province level p . In the regressions analysis, each cell-level observation is weighted by the total labor force in the corresponding province-sector-occupation cell for 2017, multiplied by the cell's share of ads within each calendar quarter.