

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

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Abstract

We study how increasing the relative cost of fixed-term contracts affects firms' labor and skill demand. We exploit a 2018 Italian labor law reform, which increased the cost of fixed-term contracts while leaving that of permanent contracts unchanged. Using rich data covering the near-universe of online job vacancies in Italy, we are able to characterize the demand for labor, human capital, and specific skill requirements under different contract types. Identification is based on a difference-in-differences research design that exploits variation in firms' exposure to the reform stemming from their heterogeneous reliance on fixed-term contracts due to varying reactions to earlier labor market reforms. We find that the increase in the cost of hiring under temporary contracts led to a decline in the relative demand for temporary workers and an increase in that for permanent workers, accompanied by rising demand for human capital and specific skill requirements. When offering jobs under a permanent contract, firms increased their demand for workers with a college degree and social skills, while reducing demand for workers with a high school degree and no work experience. When offering jobs under a fixed-term contract, firms increased demand for workers with some work experience and social skills. These findings suggest that, while restricting fixed-term contracts promoted the hiring of permanent workers, this type of reform may have unintended consequences by raising hiring standards for entry into jobs, thereby reducing employment opportunities for less qualified workers.

Keywords: Hiring costs, employment protection, dual labor markets, skills

JEL Codes: J23, J24, J63, K31

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1 Introduction

Since the 1980s, a growing divergence has emerged in employment forms across many European countries with the coexistence of fixed-term contracts, which offer flexibility to firms to adjust the level and composition of their workforce, and open-ended contracts, which are subject to stricter firing rules. These developments have bred dual labor markets with permanent workers under open-ended contracts benefiting from job stability, while temporary workers in the secondary segment under fixed-term contracts face higher employment and earning risks, limited training opportunities, and uncertainty as to whether employment under a fixed-term contract could act as a stepping stone to an open-ended contract, or lead to a dead end ([Bentolila et al., 2020](#); [Blanchard and Landier, 2002](#); [Boeri, 2011](#); [Booth et al., 2002](#)). In more recent years, globalization and technological progress have exacerbated this divergence in the forms of employment with the emergence of new contract types, such as variable hours contracts and platform work, which create duality in labor markets even in countries experiencing low shares of fixed-term contracts ([OECD, 2019](#)).¹

To address the effects of labor market segmentation, policymakers have implemented reforms of Employment Protection Legislation (EPL) with the aim to strike a balance between the flexibility offered to firms and the potentially negative consequences for workers' well-being due to the precarious nature of these flexible forms of employment. These reforms include weakening employment protection for permanent workers by lowering the costs firms incur when terminating open-ended contracts and increasing the cost of hiring under fixed-term contracts. A large literature evaluating these reforms has documented the employment effects of: (i) liberalizing fixed-term contracts (e.g., [Daruich et al., 2020](#); [García-Pérez et al., 2018](#)); (ii) easing the regulation of regular employment based on permanent contracts (e.g., [Boeri and Garibaldi, 2019](#); [Sestito and Viviano, 2018](#)) and (iii) restricting the use of fixed-term contracts ([Cahuc et al., 2022](#); [Palladino and Sartori, 2021](#)).

Although understanding the employment effects of these reforms is important, employment effects can be biased by general equilibrium effects because EPL changes affecting employment will also affect unemployment and hence the rate at which vacant jobs are filled ([Cahuc et al., 2022](#)). Furthermore, EPL reforms may also alter the demand for skills and thus affect not only the level of employment but also the composition of the workforce.

In this paper, we use online job vacancy data, to study the effect of restricting the use of fixed-term contracts on firms' labor and skill demand. Higher costs of hiring under fixed-term contracts are likely to reduce the demand for workers under temporary contracts.

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

However, whether they lead to the substitution of temporary with permanent workers and whether they induce changes in the skill composition remain open questions. Understanding these labor and skill demand adjustments is important for evaluating the efficiency of policy responses, and the extent to which they can achieve a balance between firm flexibility and employment opportunities for the most vulnerable workers they intend to protect.

We exploit a 2018 Italian labor law reform (known as “Decreto Dignità”) that increased the cost of fixed-term contracts while leaving that of open-ended contracts unchanged. We evaluate the effects of the reform on the demand for labor under different contract types, the demand for standard measures of human capital such as education and experience, and the demand for specific types of skill, which we can characterize by using rich data covering the near universe of online job vacancies in Italy. Our identification strategy is based on a difference-in-differences research design leveraging variation in the exposure of firms to the reform. This variation of exposure stems from firms’ heterogeneous reliance on fixed-term contracts due to varying reactions to earlier labor law reforms. That is, we compare the evolution of the demand for labor, as well as the demand for human capital and the demand for skills, between firms more exposed to the higher cost of hiring workers under fixed-term contracts and their less exposed counterparts. The main threat to identification is the presence of pre-existing diverging trends in labor and skill demand across firms experiencing different degrees of exposure to the reform. Using a dynamic specification, we show that there are no differential pre-trends in the outcome variables across more and less exposed groups and placebo tests reinforce the validity of the research design.

We find that the increase in the costs of hiring under a temporary contract led to an increase in the relative demand for permanent workers and a drop in the relative demand for temporary workers. These findings suggest a large degree of substitution in firms’ demand of permanent for temporary workers and indicate that the reform was successful in promoting hiring workers under permanent contracts. However, the increase in the cost of hiring temporary workers did not only shift the demand from temporary to permanent workers, but it also changed the demand for skills. Specifically, we find that: (i) in job vacancies offering permanent contracts firms increased their demand for workers with a college degree and with social skills, while they reduced their demand for workers with a high school degree and with no work experience; and (ii) in job vacancies offering fixed-term contracts firms increased their demand for workers with some work experience and with social skills.

Overall, our findings document that although restricting the use of fixed-term contracts promoted the hiring of permanent workers, it diminished the labor market opportunities among workers who are less educated and without prior experience in favor of more qualified workers. This suggests that firms require better signals of worker quality when they face

higher screening and labor costs.

This paper relates to different strands of the literature. Firstly, it relates to the literature that examines the effects of labor market reforms altering the EPL provisions of fixed-term contracts. Several studies have looked at the employment effects of liberalizing (e.g., Autor and Houseman, 2010; Blanchard and Landier, 2002; Cappellari et al., 2012; Daruich et al., 2020; García-Pérez et al., 2018; Güell and Petrongolo, 2007) and restricting (Cahuc et al., 2022; Palladino and Sartori, 2021) fixed-term contracts. We contribute to this literature in two important ways. First, we provide estimates on the effect of changing the terms of use of temporary contracts on labor demand, which is less sensitive to general equilibrium effects. Second, we provide the first evidence on the effects of reforming fixed-term contracts on 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). The closest study is Cappellari et al. (2012) which evaluates the impact of easing fixed-term contracts on skill composition defined as the ratio between non-manual and manual workers within a firm. Thanks to the use of online vacancy data, we can offer a broader perspective on the impact of changes in labor market flexibility on the demand for skills.

Secondly, this paper relates to the literature on firms' hiring strategies and the costs of screening workers. Employers value probationary work arrangements because they allow them to learn about worker productivity, and thus potential long-term match quality, before entering more stable work relationships. Given their flexible terms of use and low firing costs, fixed-term contracts provide firms with a cost-effective screening device (Faccini, 2014; Kuhnen and Oyer, 2016; Portugal and Varejão, 2009). More generally, screening costs arising from informational frictions have been shown to affect hiring strategies. For example, Weinstein (2018) shows that, when screening is costly, recruiters concentrate primarily on strong signals of worker quality. Similarly, other studies (Ballance et al., 2020; Shoag and Veugel, 2016) document that the increased costs of screening lead to firm demand for upskilling in terms of education and experience requirements, effectively shifting the burden of providing better signals of performance to job applicants. Our paper contributes to this strand of the literature by providing evidence of demand for upskilling by prospective employers in response to higher costs of screening through fixed-term contracts. Specifically, we show that once screening through temporary contracts becomes more expensive, firms increase their standards in terms of required skills to cope with uncertainty and insure themselves against adverse selection.

Thirdly, this paper is related to the strand of the literature (e.g., Bratti et al., 2021; Cabrales et al., 2017) studying gaps in training opportunities, human capital accumulation,

and job content between fixed-term and open-ended contracts. These studies document the presence of differentials in firm-sponsored on-the-job training in two-tier labor markets, with temporary workers benefiting from fewer learning opportunities leading to training gaps that are increasing in the extent of labor market dualism across countries. A recent study by [Garcia-Louzao et al. \(2022\)](#) provides empirical evidence that limited training for fixed-term workers hampers their human capital accumulation and, in turn, results in lower returns to experience compared to permanent workers since experience accumulated under temporary contracts is less valuable. Similar gaps are found in the literature also with respect to 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 linking larger skill content gaps between permanent and temporary jobs to higher duality in the labor market. We contribute to this literature by providing direct evidence, based on within-country variation due to a policy reform, on the effects of changing the relative cost of hiring under temporary contracts on the demand for skills for permanent and temporary hires.

Finally, this paper also relates to a recent and rapidly growing literature that leverages the availability of highly detailed data from online job vacancies to study a wide range of labor market issues such as: the variation in skill requirements both between and within narrowly defined occupations and the extent of complementarity between different skills ([Alabdulkareem et al., 2018](#); [Deming and Kahn, 2018](#)); changes in the demand for skills in response to the Great Recession ([Hershbein and Kahn, 2018](#)), minimum wage increases ([Clemens et al., 2021](#)), and over the business cycle ([Modestino et al., 2016; 2020](#)); labor market concentration and its mediating role on the employment effects of minimum wage increases ([Azar et al., 2019; 2020](#)); and trends in the demand for AI skills and the relationship between AI adoption and several firm and labor market outcomes ([Acemoglu et al., 2020](#); [Alekseeva et al., 2021](#); [Babina et al., 2022a; 2022b](#)). We contribute to this literature by providing evidence on the effect of a policy reform of labor contracts on the demand for labor and skills.

The rest of the paper is organized as follows: Section 2 introduces the Italian institutional setting by describing the reform that we study in a quasi-experimental setting and summarizes the earlier institutional context; Section 3 provides a description of the data sources; Section 4 illustrates the research design; Section 5 presents the results of our empirical analysis and robustness checks; and Section 6 provides concluding remarks.

2 Institutional setting

2.1 The 2018 Italian reform of temporary contracts

In the summer of 2018, the Italian government that had just formed enacted a labor market law, which is known as “Decreto Dignità”, with the aim to “curb precarious employment” ([Menegotto et al., 2018](#)).² The new law increased considerably the cost of temporary contracts while leaving the legal provisions of permanent contracts unchanged. The reform built on the pre-existing regulatory framework by operating the following changes: (i) it shortened the maximum overall duration of a temporary work relationship (from 36 to 24 months), (ii) it reduced the number of times the termination date can be extended (from 5 to 4 times), (iii) it raised 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, it re-introduced the obligation to specify a “broad” motivation for choosing a fixed-term work relationship (for contracts of 12 months or longer). As it will become clear in the next section, this last point played a crucial role in raising 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 short summary of the institutional context in the years leading up to the 2018 reform. This is useful for motivating our identification strategy as the reforms enacted in the 2010s affected the gap in employment protection legislation (EPL) between temporary and permanent contracts (henceforth, the EPL gap) as well as the evolution in the relative employment under temporary contracts (with respect to permanent ones).³

After the initial deregulation of temporary contracts at the turn of the new millennium, the Italian EPL context did not undergo any substantial changes for about a decade. Then, beginning in 2012, a number of reforms were implemented. Figure 1a illustrates the timeline of their enactment alongside the margins on which they intervened. The top colored bar above the timeline indicates how such legislation affected the temporary contract margin, while the bottom bar below it shows how the legislation affected the permanent contract margin. Figure 1b plots aggregate trends in the relative employment under temporary contracts in the corresponding years. By looking at the two figures, it is possible to visually

²Decree-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 B.2](#).

³A more in-depth account of the Italian EPL framework from 1997 until 2017 is provided in [Appendix B.1](#).

identify three distinct periods. The first one relates to the “Fornero labor law” implemented in June 2012. This reform intervened on both margins by tightening the legal provisions of temporary contracts, on the one hand, and easing those on permanent contracts on the other. Its aim was to discourage the use of fixed-term contractual arrangements while providing incentives to hires under open-ended contracts. This is consistent with a fall in relative employment under temporary contracts, which can effectively be observed between 2012 and 2013, when it reached its lowest point in the decade.

The second period is associated with a set of three measures that came to be known as “Jobs Act” (JA). The first measure (“Decreto Poletti”), introduced in March 2014, operated a major easing of the terms of use of temporary contracts. Most importantly, among other things, it abolished the mandatory requirement for the employer to specify *any motivation* for hires under a temporary contract of *any duration*, a measure unprecedented in Italian labor law. The other two measures of the JA that followed intervened on the permanent contract margin. In January 2015, a subsidy was introduced to provide firms with incentives to hire under permanent contracts, which was progressively phased out in the course of the following 3 years. Then, in March 2015, a new graded security permanent contract was introduced, which further eased the employment protection of permanent contracts in the wake of the 2012 reform. Overall, the combined medium-term effect of these three measures seems to have been larger on the temporary contract margin, as reflected by the rampant rise of the relative employment under temporary contracts observed between 2013 and 2018 (see Figure 1b).

The “Decreto Dignità” policy reform that entered into law in 2018 abruptly interrupted the surge in the use of temporary contracts. The sheer magnitude of the 2018 reform’s impact on the EPL gap can be visually appraised by looking at Figure 2, which plots the temporary-to-permanent ratio of OECD employment protection summary indicators over 2011-2019.⁴ Through a substantial and one-sided tightening of the restrictions on temporary contracts, the reform almost doubled the temporary-to-permanent indicator ratio, which soared from 0.657 to 1.221, in just one year. Our identification strategy is based on the idea that, in the lead-up to the 2018 reform, firms that had grown relatively more reliant on temporary contracts were most exposed to the reform. Accordingly, the exposure-to-treatment measure that we construct to identify the effects of the reform, 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.

⁴The indicators are published in the 2020 version of the Employment Protection Legislation Database by [OECD \(2020\)](#).

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 main data source is a proprietary dataset of online job vacancies (OJVs) that we obtained from WollyBi, a labor market analytics company of the Burning Glass Technologies (BGT) group.⁵ Using web crawling, WollyBi browses some 250 online job boards and employment agencies' websites to collect data from job postings which are de-duplicated and parsed into a standardized format suitable for data analysis. The dataset made available to us contains over 6.5 million OJVs spanning the period 2014-2019, which according to WollyBi captures the near-universe of job vacancies posted online in Italy.

For each OJV, WollyBi creates about 40 standardized fields encompassing the source and date of the job posting and a wide number of job characteristics and requirements. Of particular interest is the information on the location (at the NUTS-3 level, i.e. province), industry (at the NACE Rev.2 1-digit level), occupation (available at up to the ISCO-08 4-digit level), advertised type of contract, education and experience requirements. For the analysis, we use OJVs posted over the period 2017Q1-2019Q4 (≈ 4.5 million ads) and restrict the main estimation sample to job postings having non-missing values for province, industry, and occupation ($N=3,331,650$; 74% of all ads in the period of interest).⁶

In addition to the job attributes described above, the dataset contains rich granular information on skill requirements for each vacancy. Through a proprietary algorithm, WollyBi parsed the text of each OJV and encoded such narrowly-defined skills posted in the ad (if any) into a standardized format, i.e., level 3 of the skills/competences pillar of the ESCO v1 classification.⁷ As a result, each vacancy can contain none or several of 748 unique standardized English-language text fields such as “brainstorm ideas”, “perform planning”, “make

⁵WollyBi (www.wollybi.com), previously known as Tabulaex, is powered by Burning Glass Europe, the European division of the Burning Glass Technologies (BGT) group.

⁶The 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>).

⁷See <https://ec.europa.eu/esco/portal/skill>.

numerical calculations”, “motivate others”, “manage time”, “ICT debugging tools”, “develop ICT workflow”, etc.⁸

We apply a keywords-search routine to these text fields in order to map each of them to one of the job skill categories devised by [Deming and Kahn \(2018\)](#). Table 1 illustrates the keywords and phrases used to derive the 10 job skill categories of their taxonomy, alongside the corresponding 4 aggregate job skill categories that we devise in this study: cognitive, management, computer, and social skills.

Table 2 reports descriptive statistics on the characteristics and requirements posted in the OJVs such as type of contract and, in aggregate terms, education, experience, and specific skill requirements.⁹ In computing such summary statistics, we follow the approach adopted by [Hershbein and Kahn \(2018\)](#) and weigh each OJV 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). The table shows that a very high share of vacancies (95%) state details about the type of contract offered, which are mostly temporary contracts (45% of all OJVs) followed by permanent contracts (24%), self-employed positions (17%) and internships (10%).¹⁰ Virtually all job postings (99.8%) report an education requirement, with the majority (68%) asking for only up to secondary education (i.e., high school) and the remaining 32% requiring post-secondary education levels (i.e., college). A somewhat lower share of job postings specifies an experience requirement (62%). Out of all the ads in the sample, a sizeable proportion (18%) requires no experience (up to 1 year). Another 33% requires some experience (from 1 to 4 years), while only 11% requires high experience (4 years and above). Weighting job ads by the size of the labor force of the province in which they were posted, we obtain that 85% of them report at least 1 of the 4 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.

In Table 3 we compare the distributions of skill requirements across contract types. The share of vacancies stating an education requirement remains invariant between contract types. However, education requirements for job ads advertising temporary contracts and self-employed positions are markedly skewed towards lower levels of education (in the order of 70% demanding up to secondary education). Job ads offering a permanent contract exhibit a similar but slightly less pronounced pattern (about 60% requiring up to secondary edu-

⁸In our main estimation sample, 80.88% of the ads post at least one specific skill requirement, with an average number of 8.47 and a median of 6 specific skill requirements among ads posting any.

⁹Analogous descriptive statistics obtained using disaggregate job requirements are reported in Tables [A.1](#) and [A.2](#).

¹⁰In the Italian legal framework, self-employed workers are independent workers, that is, those who perform their work on their own account, not subordinate to any employer. Rather, they offer their services to one or multiple clients as independent contractors.

tion), while education requirements are evenly distributed among ads offering internships. The share of job postings with an experience requirement is about 4-7 percentage points lower among permanent contracts and self-employed positions (about 59%) as compared to temporary contacts and internships (63-66%). Moreover, job ads for permanent contracts tend to post higher experience requirements (16% ask for 4 or more years of experience, as opposed to 12 and 31% requiring 0-1 and 1-4 years respectively) while internships ask for lower ones (40% 0-1 years of experience, versus 17 and 7% posting 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% 1-4 years, and 8% 4 or more years, while the corresponding shares for self-employed positions are 20, 27, and 12%. Lastly, ads for permanent contracts and internships post at least one specific skill requirement at rates higher than 90% while this occurs at lower rates for self-employment positions (88%) and temporary contracts (80%). This pattern remains unchanged by looking at each macro job skill category. Furthermore, the ranking of specific skill requirements seen in Table 2 does not change across contract types.

3.2 ASIA dataset

To construct the exposure-to-treatment measure described in Section 4, we employ aggregate data derived from the Italian Statistical Register of Active Enterprises (ASIA).¹¹ The ASIA dataset, which is maintained by the Italian National Institute of Statistics (ISTAT), covers the universe of Italian firms and integrates several administrative sources such as social security, tax authority, and chamber of commerce data.¹² Among other things, this dataset contains very detailed information on firms' stocks of active work positions in a given period for both employees and non-employees (i.e. self-employed workers and external personnel). Importantly for our purposes, it distinguishes employees having a temporary contract from those with a permanent one. Although we do not have access to the whole dataset, we obtained from ISTAT yearly aggregate information on the total stock of (i) temporary employees, (ii) permanent employees, and (iii) other non-employees by province (NUTS-3) and NACE Rev.2 1-digit sector for the period 2011-2017. We use this information to compute both the nationwide sector-level relative employment annual growth rates in 2017 and 2013, and the sector shares of total province-level employment in 2011.

¹¹This excludes information regarding 1-digit industry codes A, O, T, and U of the NACE Rev.2 industry classification which could not be made available to us.

¹²For further details, see [Consalvi et al. \(2008\)](#).

4 Research Design

4.1 Exposure-to-treatment measure

The goal of the analysis is to estimate the causal effect of the increase in the cost of hiring under temporary contracts — due to the 2018 Italian “Decreto Dignità” reform — on labor demand and skill requirements. Since the 2018 reform of temporary contracts applied to all Italian firms, we derive a measure of exposure-to-treatment that exploits variation in relative employment under temporary contracts (relative to permanent contracts) across sectors and provinces and over time.¹³ We follow the approach proposed by [Hershbein and Kahn \(2018\)](#) and apply it to the Italian context, where two labor law reforms prior to the 2018 reform induced a fall and then a sharp rise of relative employment under temporary contracts.¹⁴

As discussed in detail in Section 2.2, the first reform is the 2012 “Fornero labor law”, which simultaneously restricted the use of temporary contracts and eased that of permanent ones, leading to a *drop* in relative employment under temporary contracts. We define 2013 as the *trough* year, when relative employment reached its lowest point following the “Fornero labor law”. The second reform is the 2014-2015 “Jobs Act”, which first relaxed regulations for temporary contracts and then for permanent ones, leading to a sharp *increase* in relative employment under temporary contracts. The increase continued at a rising pace well into 2018, the year the 2018 “Decreto Dignità” reform was enacted. Hence, we define 2017 as the *peak* year (the last pre-2018 reform year).

We project — at the province level — the nationwide sector-level annual growth rates of relative employment at trough and peak years, using pre-determined province-specific sectoral employment shares, as shown in Equation (1) below:

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

where $RE_{s,t}$ denotes the nationwide relative employment under temporary contracts in sector s and year t (where $t \in \{2013, 2017\}$, the trough and peak years, respectively), and $\frac{e_{p,s,2011}}{e_{p,2011}}$

¹³Relative employment under temporary contracts (or simply relative employment) is defined as the ratio of aggregate stocks of active temporary and permanent contracts from the ASIA dataset (see also Section 3.2).

¹⁴[Hershbein and Kahn \(2018\)](#) compute an exposure measure that captures spatial variation in the severity of the Great Recession in terms of employment losses to identify its effect on the demand for skills. Their measure exploits sectoral heterogeneity in employment losses by computing the difference between (locally projected) sector-level employment annual growth rates in the years surrounding the recession — namely, 2006, when employment was at its highest level before the recession (peak) and 2009, when it hit its lowest level (trough).

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 prior to the series of reforms that occurred in the 2010s). We then obtain our exposure-to-treatment measure by taking the difference between the two projections in 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 illustrates the nationwide sector-level annual growth in relative employment at trough and peak years, which correspond 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, and a positive growth in relative employment between 2016 and 2017, which both vary by sector. Figures 4a and 4b depict the projections of these nationwide sector-level annual growth rates in relative employment at the province level in the trough ($\widehat{\Delta RE}_{p,2013}$) 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, which exhibits substantial variation across provinces as shown in Figure 4c.

To summarize, our measure is a Bartik-type exposure-to-treatment measure which leverages: (i) nationwide sectoral adjustment over time of relative employment under temporary contracts in response to the two earlier reforms; and (ii) pre-determined sectoral employment distributions within provinces to project the nationwide sector-level exposure at the province level. Therefore, the proposed measure captures which local areas (i.e., provinces) had grown relatively more reliant on temporary contracts in the lead-up to the 2018 “Decreto Dignità” reform owing to previous labor market reforms.

This measure offers three major advantages. Firstly, by projecting nationwide sector-level exposure at the local level, the Bartik measure we propose avoids measuring directly local relative employment under temporary contracts, which may reflect 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 using static measures, since 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 pre-determined employment shares, thereby maximizing variation and enhancing precision.

4.2 Specification

We proceed by collapsing the job postings into 13,383 province-sector-occupation (pso) groups by 12 calendar quarters (t) from 2017Q1 to 2019Q4. The resulting dataset is an unbalanced panel with 109,861 province-sector-occupation-quarter ($psot$) cells each containing, on average, about 30 job postings.¹⁵ We then construct the following cell-level labor demand and skill requirement indicators: firstly, we define the demand for employment under a given contractual arrangement as the share of all job postings in a cell offering such contract; secondly, we derive the demand for each of several human capital and specific skill requirements by computing the share of job postings in a cell demanding such skills, both overall and conditional on the advertised contract type. These indicators constitute the outcomes of interest (Y_{psot}).

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

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

where $\mathbf{1}[exposure_p \geq p50]$ is a discrete exposure-to-treatment indicator, which is equal to 1 when $exposure_p$ is above its median value and 0 otherwise.¹⁶ $post_t$ is a dummy for the treatment period which is equal to 1 when $t \geq 2018Q3$, the calendar quarter in which the “Decreto Dignità” was implemented, and 0 otherwise.

The specification also includes a rich set of fixed effects to control for unobserved confounders in each of the four dimensions that make up our cells. Given that our identification strategy is based on the interaction of (i) national changes in relative employment growth by sector and (ii) province-level pre-determined sector composition, it is particularly important to add fixed effects at the levels of province (α_p) and sector (β_s). If the provinces and sectors that have a higher exposure-to-treatment exhibit systematic differences, for instance, related to technology adoption, affecting labor demand and skill requirements, then our treatment effect estimates would be biased. Including province and sector fixed effects addresses this possible omitted variable bias and strengthens the identification assumption on our exposure-to-treatment measure. We also include occupation (γ_o) fixed effects to account for occupation-level unobserved confounders and calendar quarter (δ_t) fixed effects to control for secular trends systematically related to the demand for labor and skills. Standard

¹⁵Detailed descriptive statistics on the number of (unweighted) online job vacancies (OJVs) posted per cell are provided in Table A.3.

¹⁶In Section 5, we check the sensitivity of our results to alternative ways of defining the exposure-to-treatment indicator variable.

errors are clustered at the level of province p to address potential serial correlation within local units.

Furthermore, following Hershbein and Kahn (2018), we also account for the size of the labor force and the number of job vacancies in given labor market cells, by weighting each cell-level observation by the product of the cell's total labor force fixed at the last pre-reform year (2017) and the cell's share of ads within each calendar quarter.

We also estimate a specification in which 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 outcome difference at each quarter between areas with high and low exposure to the reform with respect to 2017Q1 (the first quarter of the estimation sample, for which we impose $\theta_{2017Q1} = 0$). This specification allows to study the intensity and persistence of the estimated treatment effects at different points in time.¹⁷

In addition, estimating this specification allows to test for the validity of the parallel trends identifying assumption. 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, finding no evidence of underlying differential pre-trends in the outcome variables across exposure groups. Moreover, 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 prior to the reform and, as mentioned, we also examine the sensitivity of the results to other ways of defining the exposure-to-treatment indicator.

5 Results

To evaluate the effects of increasing the cost of hiring under temporary contracts, we estimate the difference-in-differences (DiD) specifications of Equations (3) and (4), which compare outcomes between firms in local areas more exposed to the use of temporary contracts in the lead-up to the reform (“treatment group”) and their less exposed counterparts (“control

¹⁷It should be noted that treatment applies simultaneously to all treated units and is an absorbing state. Therefore, our design is not of the staggered difference-in-differences type.

group") before and after the policy change. In Section 5.1, we report the impact of the reform on firms' labor demand separately for different types of contracts. In Section 5.2, we extend the analysis by focusing on firms' demand for standard measures of human capital, such as the level of education and work experience, while in Section 5.3 we consider detailed skill requirements.

5.1 Reform effects on labor demand by contract type

We first consider the impact on the demand for labor under four different contract types, that is, (i) a permanent contract, (ii) a temporary contract, (iii) as self-employed, or (iv) an internship. The outcome variables are then defined as the share of job vacancies offering each of these contracts.

Hiring intentions across contract types constitute a potential margin of labor demand adjustment for firms in response to the reform. Higher costs of hiring workers under temporary contracts may lead to an increased reliance on other forms of flexible employment (such as jobs as self-employed or internship jobs) or may shift demand to permanent workers. Table 4 reports the estimates from Equation (3) showing that firms more exposed to the higher cost of hiring temporary workers increased their demand for permanent workers (by 5.9 percentage points), while they reduced their demand for temporary workers (by 5.3 percentage points). By contrast, there is no impact on the demand for self-employed workers or interns. These findings suggest that the reform induced a shift in labor demand away from temporary towards permanent contracts.

Figure 5 shows the coefficient estimates of the dynamic specification from Equation (4) along with 95% confidence intervals for each of the four outcome variables. These coefficients capture the treatment effect of the reform in each calendar quarter τ relative to 2017Q1.¹⁸ The top-left plot of Figure 5 shows that prior to the reform there is no significant difference in firms' propensity to post permanent job vacancies, supporting the parallel trends assumption. After the implementation of the reform, in 2018Q3, we observe that firms in areas more exposed to the higher costs of hiring under temporary contracts increased their demand for permanent workers. By the last quarter (2019Q4), the probability of posting a vacancy for a permanent worker increased by 8.6 percentage points (relative to 2017Q1). The magnitude of this estimate corresponds to a 32% increase in demand for permanent workers relative to the pre-reform average share of advertised permanent job positions (.268).

¹⁸We set the reference period to the first calendar quarter (2017Q1) to make sure the estimates are not influenced by possible anticipation effects. Although the reform officially entered into force in July 2018, there was ongoing public discussion about the policy change since January 2018 due to the election campaign and the general election which was held in March 2018 (for more details on the roll-out of the reform see Appendix B.2).

The top-right plot of Figure 5 also shows no differential pre-trends and that after the reform firms in more exposed areas reduced their demand for temporary workers. By the last quarter, the probability of posting a vacancy offering a temporary contract decreased by 6.2 percentage points, which implies a 13% decline relative to the pre-reform average share of advertised temporary positions (.482).

The two plots at the bottom of Figure 5 confirm the evidence reported in Table 4 of no effects of the reform on the demand for workers under other flexible contract types, such as self-employment and internships. Overall, these findings point towards a large degree of substitution of temporary with permanent contracts, consistent with a differential adjustment of hires in response to the sudden and sharp increase in the relative cost of temporary contracts.

5.1.1 Robustness

To assess the robustness of the above findings, we first conduct a placebo test by shifting the time frame backwards to the window between 2016Q1 and 2017Q4, which is before the reform entered into law at the beginning of 2018Q3, while keeping the reference period for the dynamic specification estimates at 2017Q1. Figure C.1 shows that there are no treatment effects in these placebo regressions, which provides additional evidence for the validity of the research design and the results reported in Figure 5.

Furthermore, we perform two additional checks to assess the robustness of the results to alternative definitions of the exposure-to-treatment indicator variable. Recall that for the baseline results, the exposure-to-treatment indicator variable is defined using the median ($p50$) as the cutoff. First, we check the sensitivity of the results to different cutoffs. As shown in Figure C.7, the baseline results are not sensitive to using cutoffs such as $p35$, $p40$, $p45$, $p55$, $p60$ or $p65$. Second, we define the indicator variable such that it is equal to 1 if $exposure_p$ belongs to the top tercile of its distribution, and equal to 0 if it belongs to the bottom tercile. This entails dropping observations for provinces with exposure in the medium tercile. Figure C.8 shows that the estimates are also largely unchanged with this alternative definition of the exposure-to-treatment indicator variable.

5.2 Reform effects on education and experience requirements

We next study the effects of the higher costs of hiring under temporary contracts on the demand for standard measures of human capital, such as the level of education and years of experience. We assess the impact of the reform both among all vacancies and separately by

contract type. Figure 6 presents estimates from the dynamic specification in Equation (4).¹⁹

Figure 6a shows that firms in areas more exposed to the reform increased their demand for college degree graduates (i.e., with post-secondary education) and decreased their demand for high school graduates (i.e., with upper-secondary education), relative to their less exposed counterparts. Conditioning on contract type, Figure 6b shows that these effects are driven by changes in the educational requirements of permanent job vacancies, which are observed immediately after the reform’s announcement in 2018Q2. These findings suggest that firms, in response to the increased cost of hiring under temporary contracts, are willing to hire more permanent workers but require higher educational qualifications.

Figure 7a shows that the reform induced an overall increase in the demand for workers with some experience (1-4 years) and a drop in the overall demand for workers with high levels of experience (4+ years) or no experience (0-1 years), although the latter effect is not very precisely estimated.²⁰ Conditioning on the type of the contract, Figure 7b shows that the decline in the demand for workers with no experience is mostly driven by job positions offering permanent contracts, while the increase in the demand for workers with some experience is mostly driven by job positions offering temporary contracts.

Overall, we find that the increase in the cost of hiring temporary workers has not only shifted the demand from temporary to permanent workers, but it has also changed their required levels of human capital. Specifically, firms offering permanent contracts require a college degree more frequently and are also less likely to offer such contracts to workers with no experience. In addition, firms offering temporary contracts are more likely to require some work experience. Taken together, our findings suggest that, although the reform promoted the hiring of permanent workers, it has also diminished the labor market opportunities among workers who are less educated and without prior experience.

5.3 Reform effects on the demand for skills

In this section, we extend the analysis beyond standard measures of human capital by considering the impact of the reform on the demand for skills. Following the classification discussed in Section 3.1 (see also Table 1), we consider the following skill types: cognitive, management, computer, and social skills.

¹⁹Based on the evidence that the reform had no effect on the demand for workers under self-employed and internships contracts, in what follows we focus the analyses on permanent and temporary contracts only.

²⁰The decline in the demand for highly experienced workers could be due to firms’ reluctance to hire this type of workers as they entail higher labor costs. Another potential explanation relates to the possibility of conversions from temporary to permanent contracts within firms that involve high-experience workers.

5.3.1 Share of job vacancies requiring a specific skill type

As a first approach, we define the dependent variable for each skill type as the share of job vacancies in a cell requiring such skill. As in the case of education and experience requirements, we consider the effects of the reform both for all vacancies and separately for vacancies offering temporary or permanent jobs.

When we focus on all vacancies, Figure 8a shows that firms more exposed to higher costs of hiring under temporary contracts increased the demand for social skills relative to their less exposed counterparts. These are skills pertaining to communication, organization, and support tasks. Instead, we find no effect of the reform on the demand for cognitive, management, and computer skills. When we condition on the contract type, Figure 8b shows that the increase in the demand for social skills occurs both among vacancies for temporary and permanent jobs. The absolute magnitude of the two effects is similar (around 2.2 percentage points), but it is larger in relative terms among vacancies for temporary jobs (+3.9% on a pre-reform average of .576) than among vacancies for permanent jobs (+2.8% from to .795). These results provide evidence that the reform led more exposed firms to raise their standards in terms of the social skills required for entry into jobs, especially for prospective temporary hires that had become relatively more expensive.

5.3.2 Relative importance of specific skills

We also consider two alternative dependent variables based on the relative importance of each skill type. Drawing on the approach suggested by Alabdulkareem et al. (2018), these alternative measures allow us to study 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 of type k required *within* a labor market defined by cell c at time t , where there are K skill types referring to cognitive, management, computer and social type of skills.

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

Note that each vacancy can require one or more specific skills of a given type k (s_v^k is the count per vacancy v), while cells correspond to province-sector-occupation (*psos*) groups introduced in Section 4. As shown in Equation (5), the relative importance of a skill type k is defined as the ratio of the number of specific skills of type k required in vacancies in cell c at time t

to the total number of specific skills out of all types required in all vacancies posted in cell c at time t . In other words, this measure tracks the prevalence of a given skill type out of all types of skills demanded *within* a certain labor market across time. Its range goes from 0 to 1.

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

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

It measures the extent to which a given skill type k is over-expressed in the vacancies in cell c at time t relative to the overall labor market. When a skill type k is demanded more in market c compared to the overall market, then this measure is larger than 1, while when it is demanded less in market c compared to the overall market it is lower than 1.

We find that the reform brought about a significant reallocation of the demand for social skills both within and between labor markets. Figure 9a shows that firms more exposed to higher costs of hiring under temporary contracts increased their demand for social skills relative to other skill types. This indicates that social skills became relatively more prevalent within more exposed markets. Furthermore, Figure 10a shows that firms in more exposed markets started demanding social skills more relative to all markets as a whole. No significant reallocation effects were found for other skill types such as cognitive, management, and computer types of skills. Furthermore, these effects are strongly driven by skill demand reallocation among temporary job postings (Figures 9b and 10b). Hence, these results suggest that firms more exposed to the reform reacted by assigning a higher weight to social skills, particularly for temporary hires.²¹

²¹Placebo tests have also been carried out for the outcomes examined in Sections 5.2 and 5.3, shown in Figures C.2 and C.6, which attest the robustness of the results. Similarly, we conducted further sensitivity tests confirming the robustness of the results presented in those sections to alternative definitions of the exposure-to-treatment indicator variable, which are available upon request.

6 Conclusion

In the last decade, policymakers in several European countries have taken steps to reduce the long-lasting divergence in the level of employment protection between open-ended and fixed-term forms of employment that had given rise to dual labor markets. They have sought to achieve this goal by both easing the legal provisions of open-ended contracts and, more recently, by restricting the terms of use of fixed-term contracts.

In this paper, we analyze the impact on labor demand and skill requirements of the “Decreto Dignità”, a labor law reform enacted in Italy in 2018 that severely increased the cost of temporary employment contracts after a period in which their use had seen an increasing expansion. To identify the effects of the reform, we exploit the heterogeneity of this expansion (across sectors and geographical areas) resulting from a differential reaction to previous labor market reforms enacted over the period 2012-2017, whereby some employers had grown relatively more reliant on temporary contracts to hire workers.

Using data from online job vacancies, we show that the increased cost of hiring under fixed-term contracts led to a decline in the relative demand for temporary workers and an increase in the relative demand for permanent workers. Moreover, this substitution of labor demand was accompanied by a demand for upskilling in terms of both general human capital and specific skill requirements. In permanent job vacancies, firms increased their education requirements and their propensity to require social skills, while they became more reluctant to hire workers with no work experience. In temporary job vacancies, firms increased their demand for workers with some work experience and for those endowed with social skills.

Overall, our results suggest that employers who were more exposed to the increase in the cost of hiring fixed-term workers reacted through a strong disinvestment in the use of temporary contracts and a higher willingness to take more commitment by hiring more permanent workers. However, as a result of the increase in the cost of screening and the consequent loss of flexibility, firms shifted their labor demand under both contracts to a different pool of workers endowed with higher human capital, in order to compensate for the loss of worker quality signals and thus insure themselves against adverse selection.

The evidence provided in this paper brings important insights into the current policy debate by highlighting that labor law reforms aiming at restricting the use of fixed-term contracts (or other forms of flexible employment more generally) can have unintended consequences by raising hiring standards for entry into jobs in terms of skills demand, thus worsening the employment opportunities for less qualified workers and, potentially, also the efficiency of job search and matching.

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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 derived 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 A.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 A.2.

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

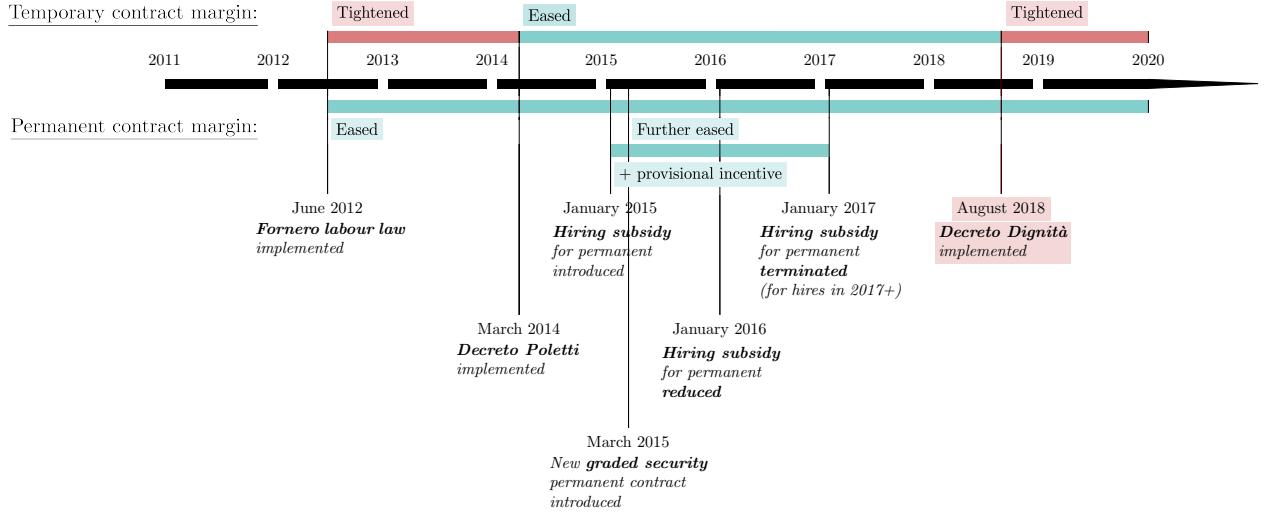
	Share of all vacancies			
	Permanent	Temporary	Self-employment	Internship
$\mathbb{1}[exposure_p \geq p50] * post_t$	0.059*** (0.015)	-0.053*** (0.017)	-0.005 (0.006)	-0.004 (0.003)
Province FE	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes
Occupation FE	Yes	Yes	Yes	Yes
Calendar quarter FE	Yes	Yes	Yes	Yes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

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

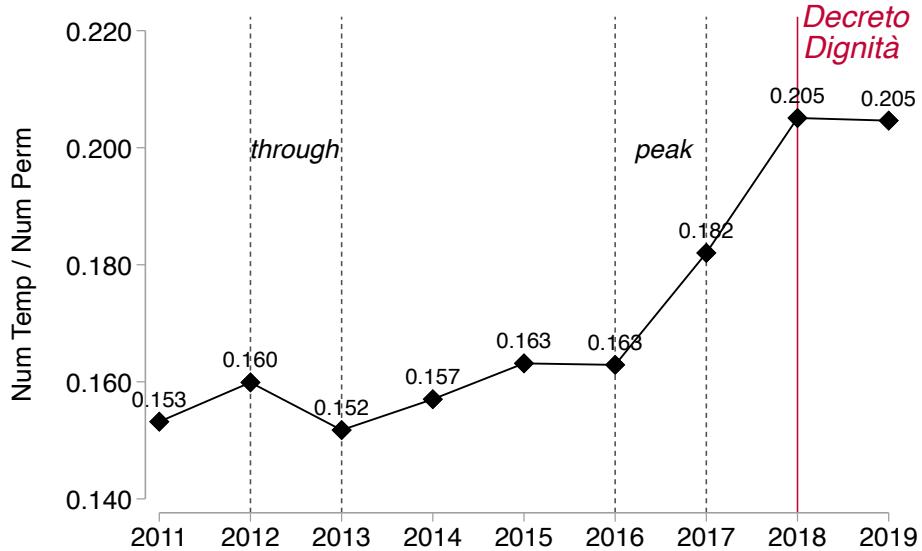
Figures

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

a. Timeline of EPL policies affecting Temporary and Permanent contracts

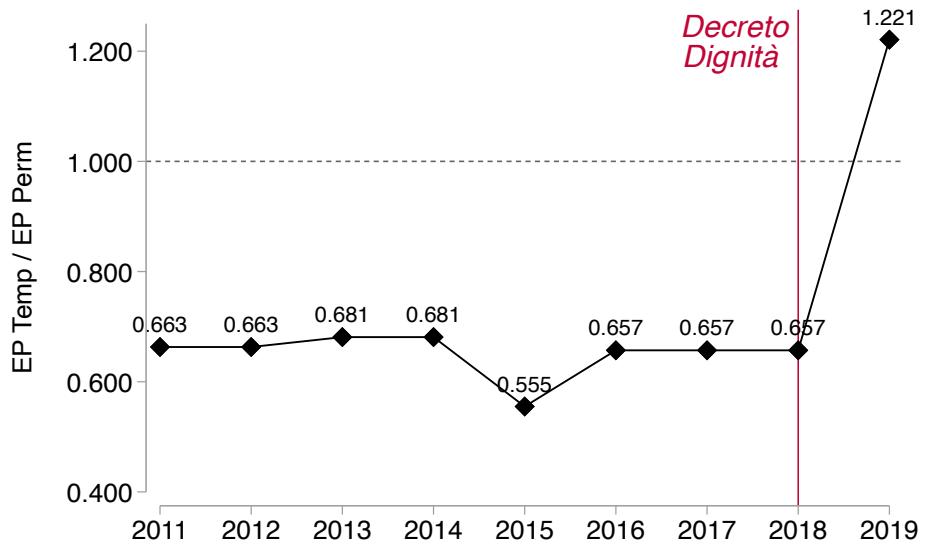


b. Aggregate trends in Relative Employment under Temporary contracts
(Ratio of Temporary-to-Permanent employment stocks)



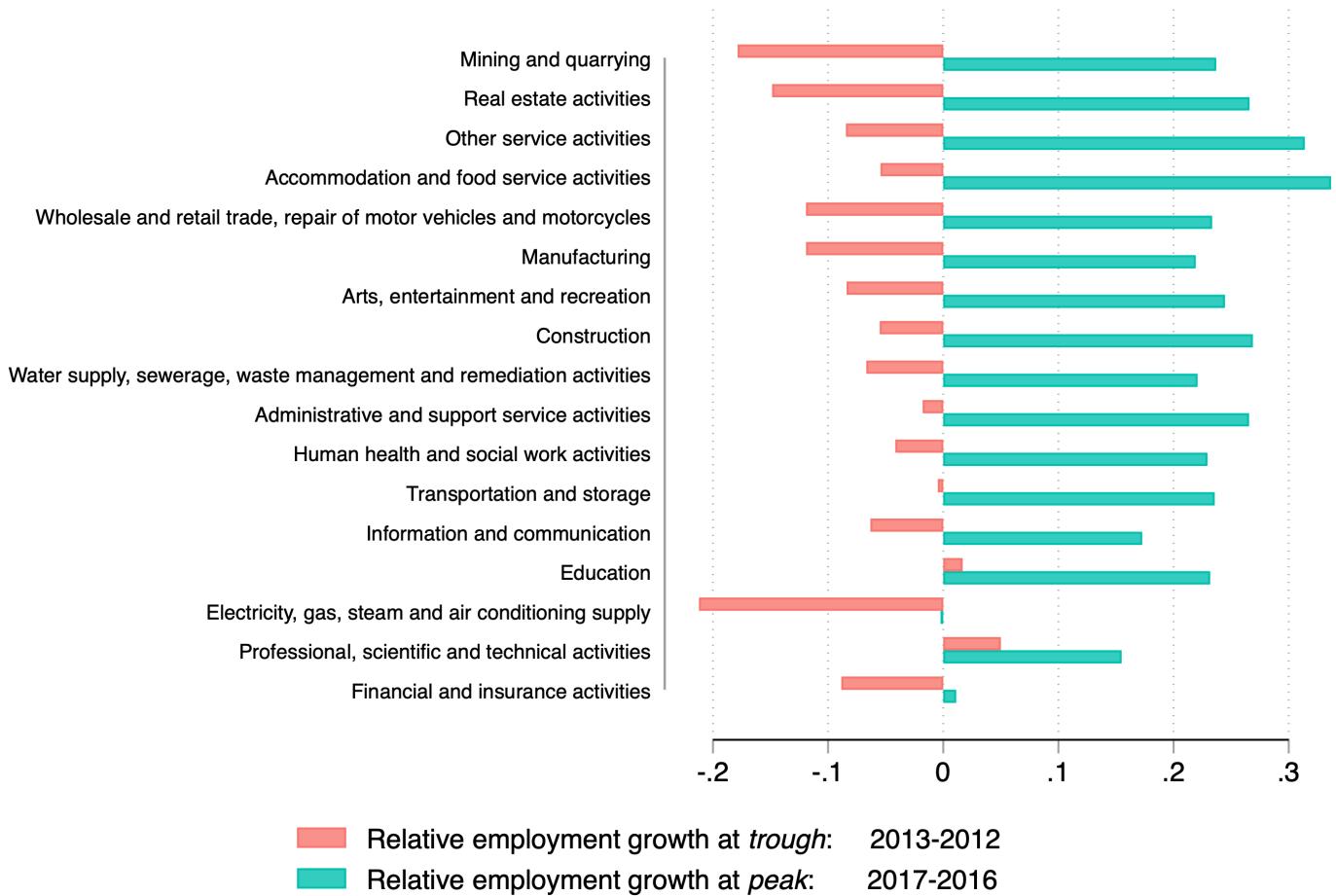
Note: The top figure illustrates a timeline summarizing the Italian employment protection legislation (EPL) changes that occurred in the 2010s. The figure describes how each reform affected the EPL of temporary contracts (in the top colored bar above the timeline) and/or permanent contracts (in the bottom colored bar). Tightening and easing interventions are indicated, respectively, in red and green. The bottom figure plots aggregate trends (at the nationwide level) over the same period in relative employment under temporary contracts, computed as the ratio of temporary and permanent employment stocks using ISTAT's Labor Force Survey. The dashed vertical lines enclose the *trough* and *peak* years discussed in Section 4.1.

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



Note: The figure plots trends in the ratio of the OECD's employment protection legislation (EPL) indicators for temporary and permanent contracts respectively. This ratio captures the relative employment protection of temporary contracts (as compared to permanent ones) thus proxying the EPL gap between the two contracts. The horizontal dashed line indicates the point at which the two contracts have the same level of employment protection. The indicators used are published in the 2020 version of the Employment Protection Legislation Database by the OECD. The methodology used to derive them is described in detail at the following webpage: <https://www.oecd-ilibrary.org/sites/1686c758-en/1/3/3/index.html?itemId=/content/publication/1686c758....>

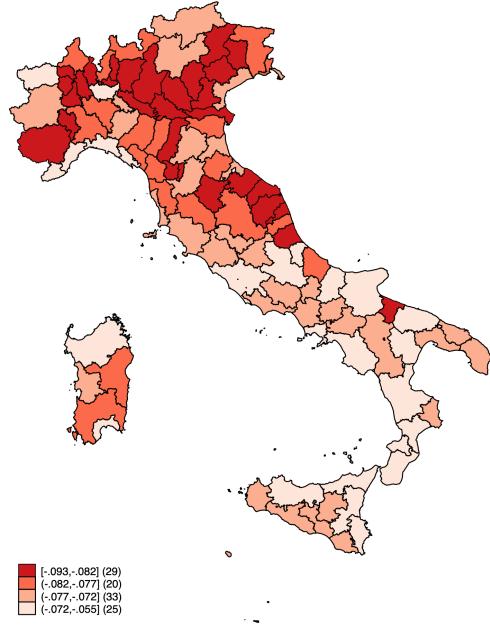
Figure 3: Nationwide sector-level annual growth rates of relative employment at *trough* and *peak*



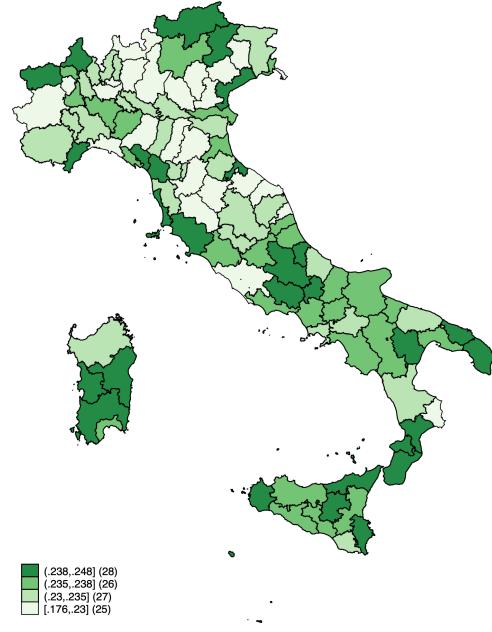
Note: The figure shows nationwide sector-level annual growth rates in relative employment under temporary contracts — $[(\ln(RE_{s,t}) - \ln(RE_{s,t-1}))]$ in Equation (1) — computed at *trough* ($t = 2013$, red bars) and *peak* ($t = 2017$, green bars) years. For each 1-digit sector s and year t , relative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

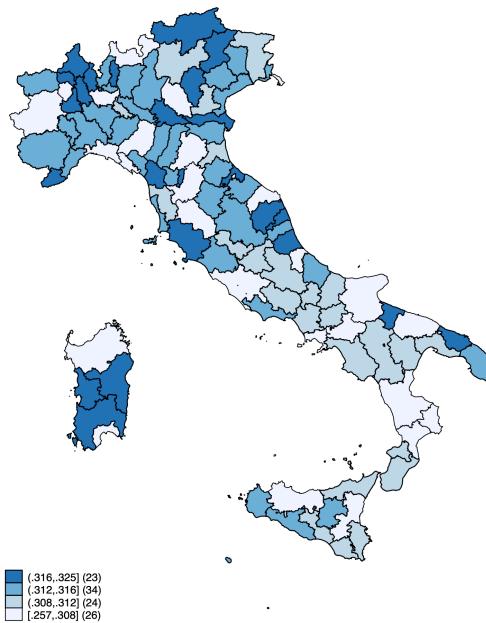
a. Projected relative employment growth
 at trough: 2013-2012 ($\widehat{\Delta RE}_{p,2013}$)



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

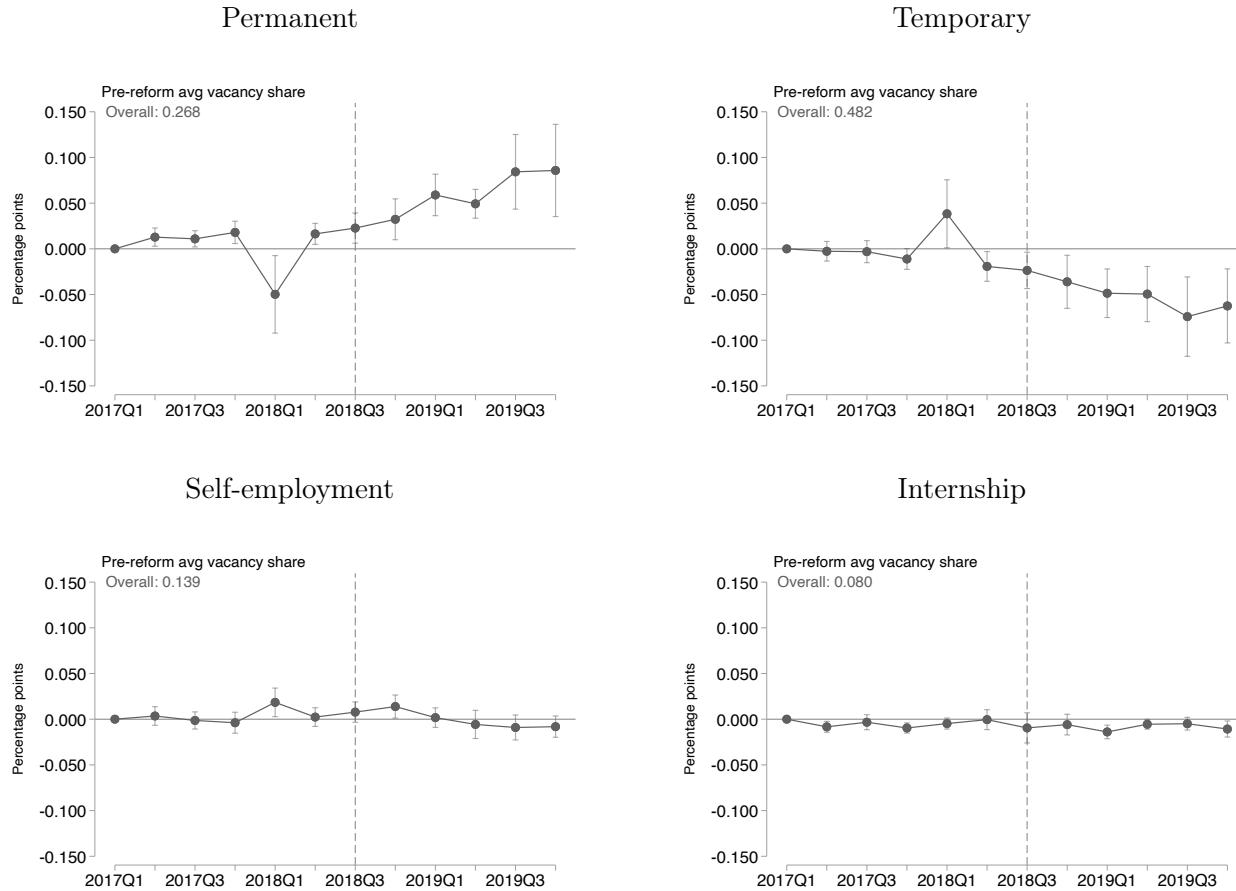


c. Change in proj. rel. employment
 annual growth bewteen peak and trough
 $exposure_p = \widehat{\Delta RE}_{p,2017} - \widehat{\Delta RE}_{p,2013}$



Note: The figures at the top left (panel a, in red) and at the top right (panel b, in green) respectively show variation in province-level projections of relative employment annual growth at *trough* ($\widehat{\Delta RE}_{p,2013}$) and *peak* ($\widehat{\Delta RE}_{p,2017}$), computed as illustrated by Equation (1), where the projection weights are province-specific sectoral employment shares of total employment in 2011. The bottom-left figure (panel c, in blue) shows variation in $exposure_p$, which is defined as the difference between the two above projections, as shown in Equation (2). All calculations exploit aggregate information from the ASIA dataset.

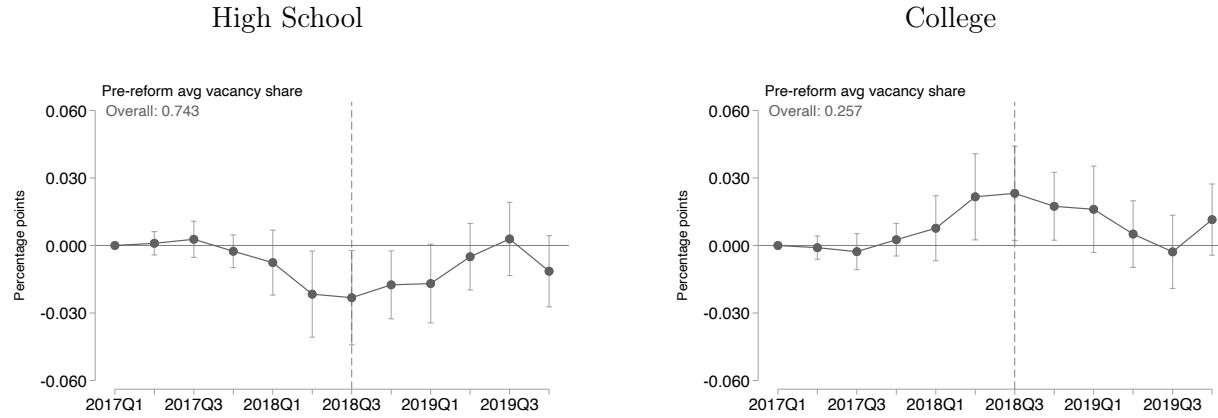
Figure 5: Labor demand by contract type



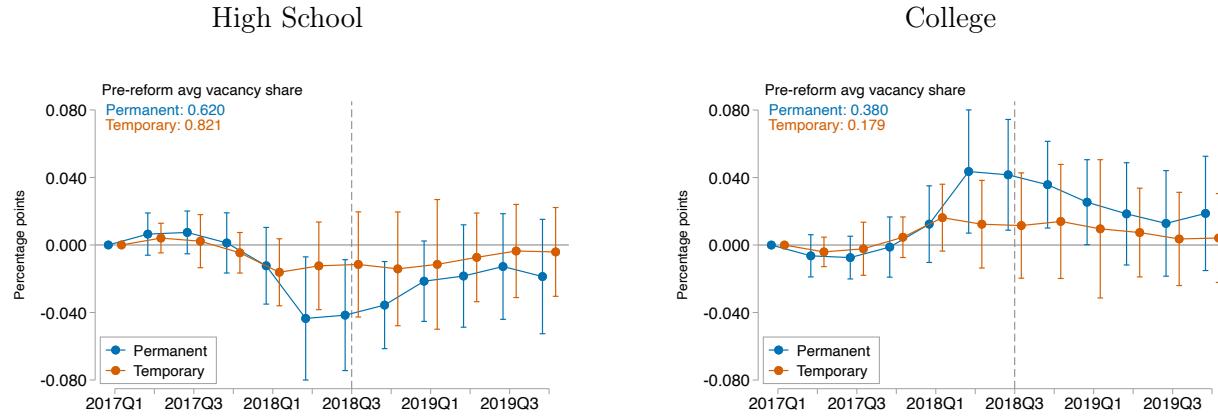
Note: The figure shows the effect of the reform on the share of total vacancies posting job positions under a permanent contract (top-left), temporary contract (top-right), self-employment (bottom-left), and internship (bottom-right). Outcome shares are computed within each province-sector-occupation-calendar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

Figure 6: Education requirements

a. All vacancies

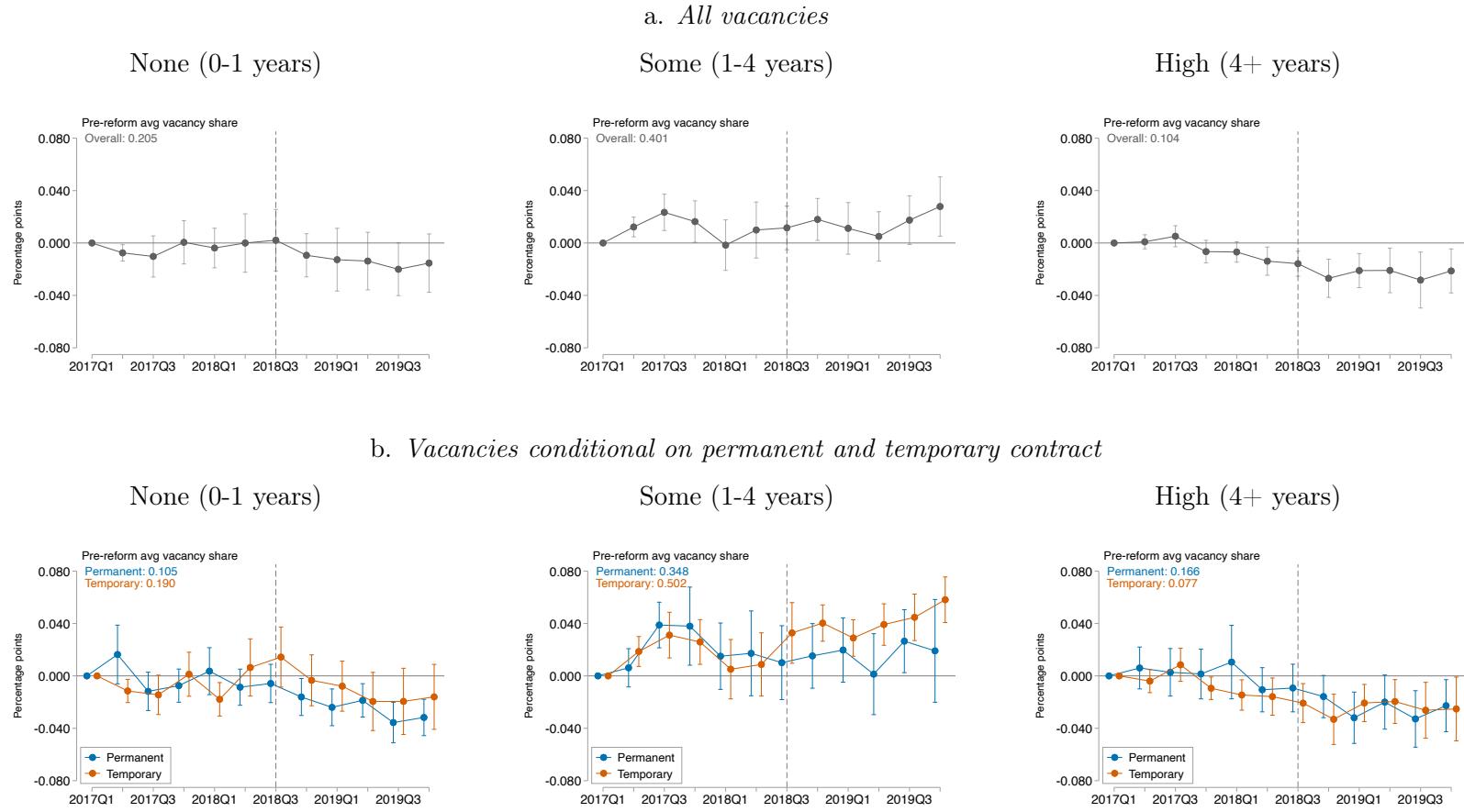


b. Vacancies conditional on permanent and temporary contract



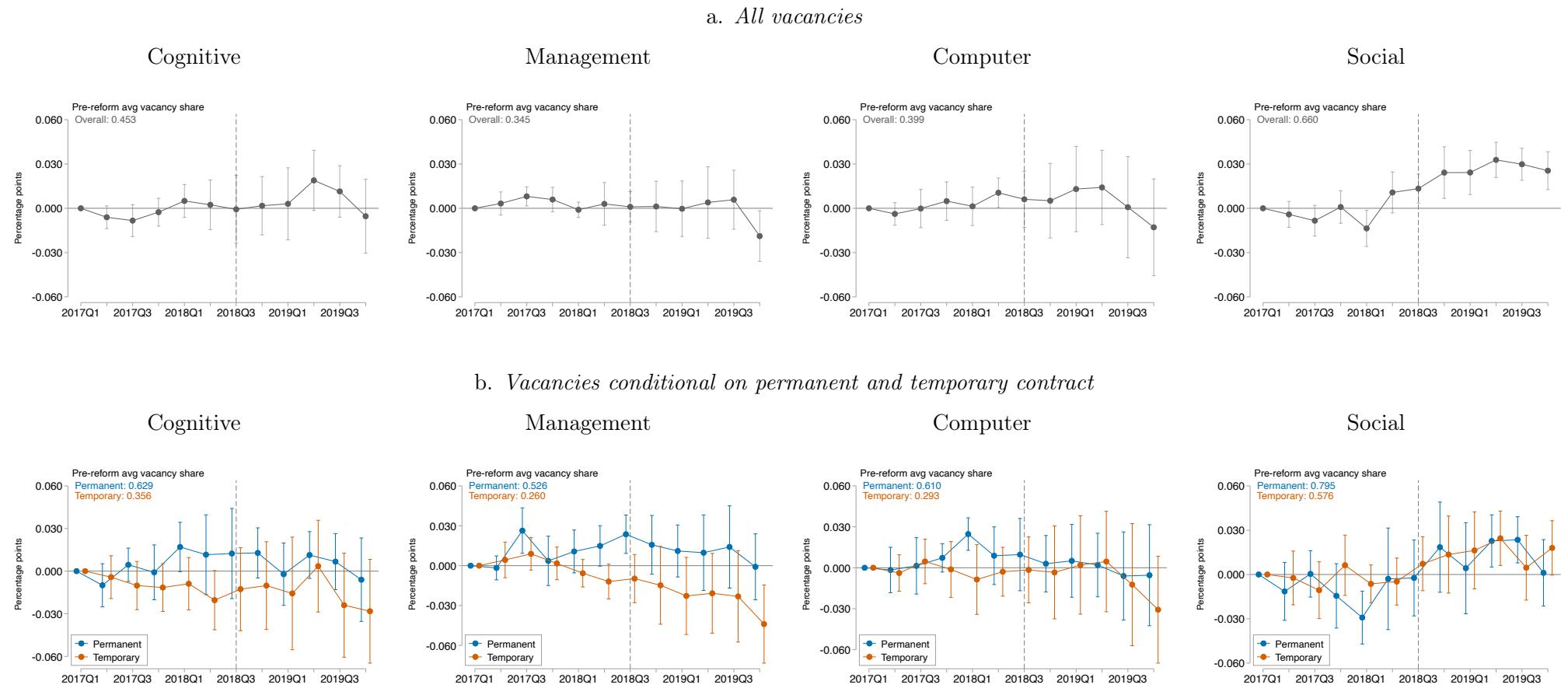
Note: The figure shows the effect of the reform on the share of vacancies requiring up to secondary education, i.e., high school (left column), and post-secondary education, i.e., a college degree (right column) respectively out of total vacancies (panel a, in grey), out of permanent job vacancies (panel b, in blue), and out of temporary job vacancies (panel b, in orange). Outcome shares are computed within each province-sector-occupation-quarter cell, which is the unit of analysis. The treatment indicator takes value 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 95% confidence intervals. They capture outcome differences at each quarter between provinces with high and low exposure to the reform with respect to 2017Q1. The reform was implemented in 2018Q3. Standard errors are clustered at the province level p . In the regressions, each cell-level observation is weighted by the total labor force in the corresponding province-sector-occupation cell in 2017 times the cell's share of ads within each calendar quarter.

Figure 7: Experience requirements



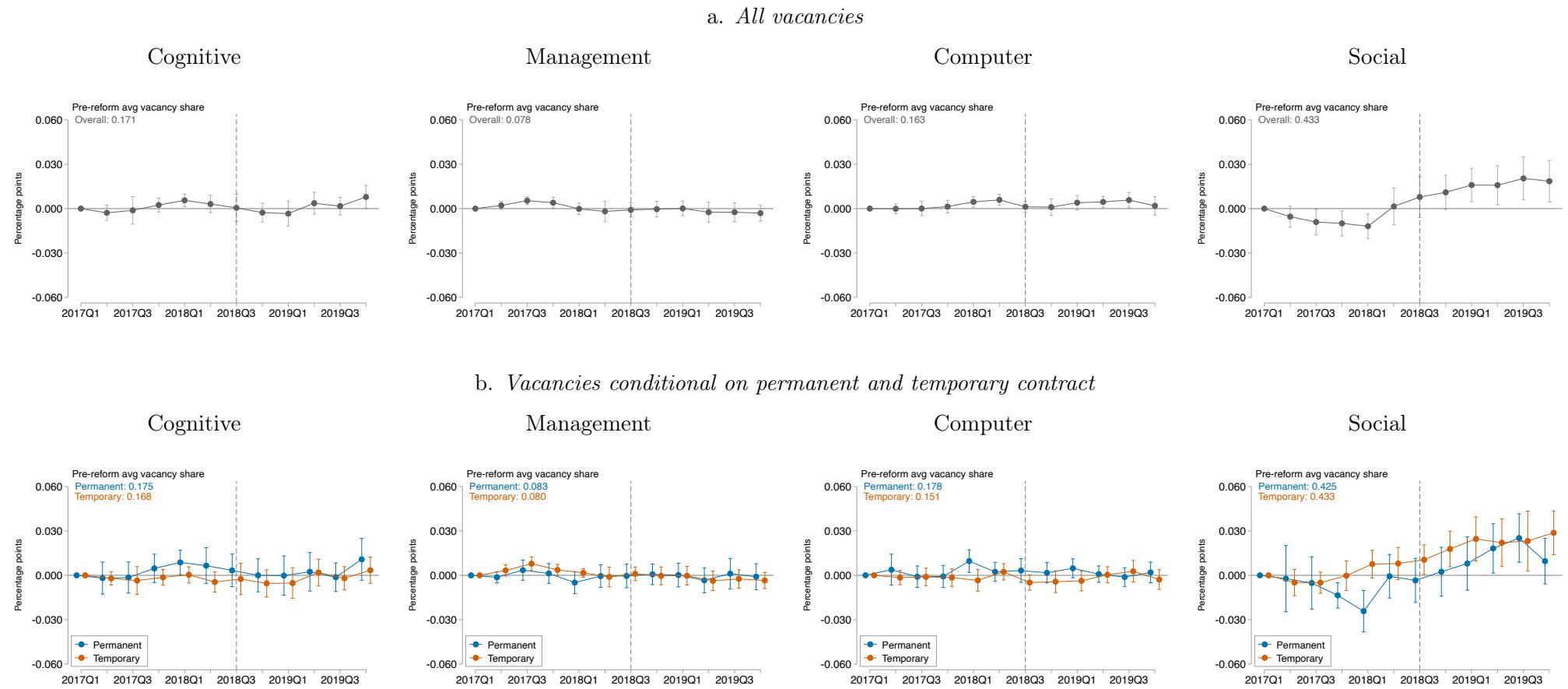
Note: The figure shows the effect of the reform on the share of vacancies requiring no experience, i.e., 0-1 years (left column), some experience, i.e., 1-4 years (center column) and high experience, i.e., 4+ (right column) skills respectively out of total vacancies (panel a, in grey), out of permanent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

Figure 8: Specific skill requirements



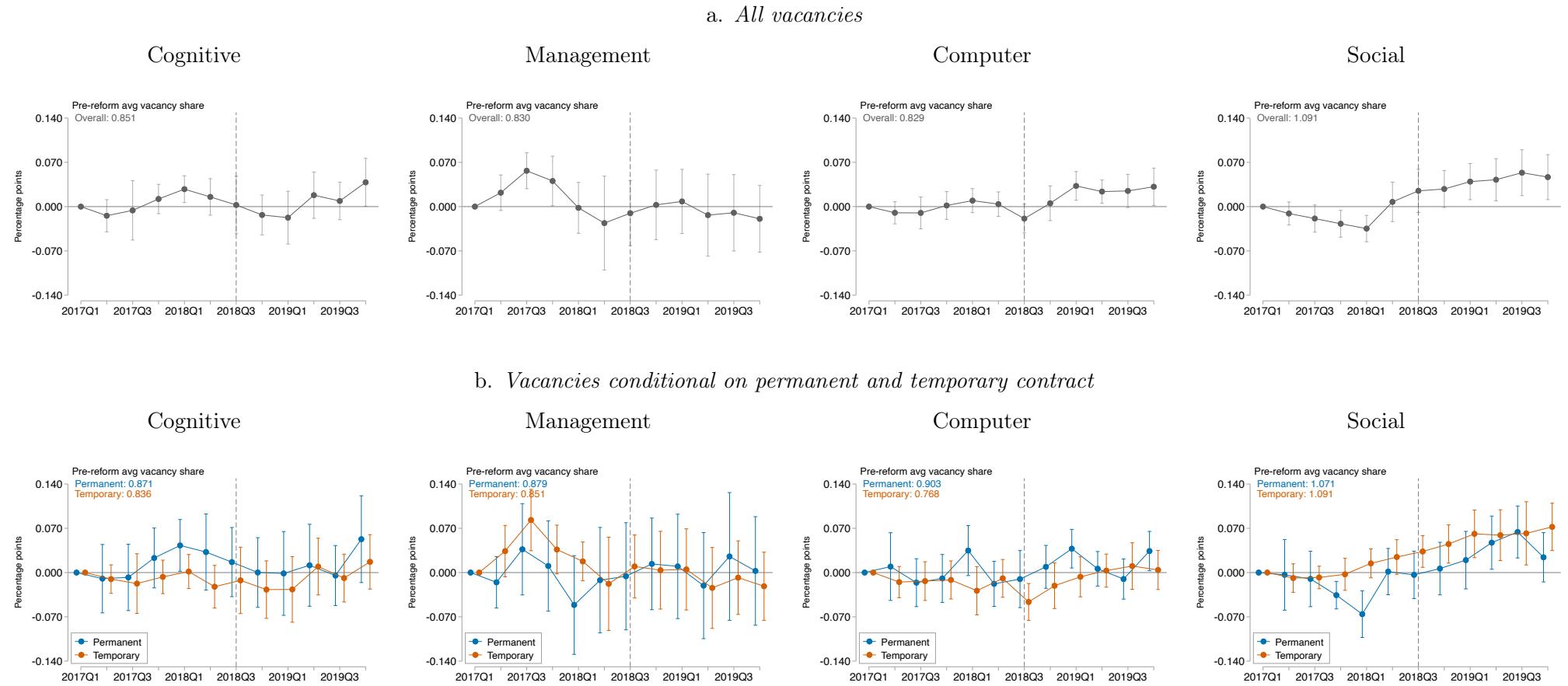
Note: The figure shows the effect of the reform on the share of vacancies requiring cognitive (first column), management (second column), computer (third column), and social (fourth column) skills respectively out of total vacancies (panel a, in grey), out of permanent job vacancies (panel b, in blue),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

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



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

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



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

A Further Descriptive Statistics

Table A.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 A.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	

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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 A.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 (OJVs) posted within each province-sector-occupation and province-sector-occupation-quarter cell.

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

B.1 EPL reforms during 1997-2017

The Italian labor market is characterised by a two-tier structure whereby permanent workers enjoy substantially higher levels of employment protection than temporary workers. The origins of this duality can be traced back to the years at the turn of the new millennium when the introduction of legislations²² such as *Law 196/1997* (the so-called “Treu Package”) and *Legislative Decree 368/2001*²³—which, respectively, liberalised temporary contracts first and then considerably eased the legal restrictions on their use—markedly reduced the extent of employment protection of temporary contracts while however leaving that of permanent contracts largely unaffected (Boeri, 2011; Cappellari et al., 2012; Daruich et al., 2020). For this reason, such reforms of employment protection legislation (EPL), and the likes undertaken by other European governments over recent decades, have been dubbed *partial* EPL reforms (Boeri, 2011).

After a decade of substantial inactivity, the 2010s saw the implementation of a number of interventions, operated by various governments, which in most cases sought to undertake a more comprehensive and structural approach to EPL reform. In what follows, we provide an account of two major policy interventions that were enacted over the period 2012-2017, which feature different scopes and signs, thus diversely affecting the duality of the EPL framework as illustrated in the timeline in Figure 1a.

The first one, *Law 92/2012* (known as the “Fornero labor reform”), implemented in June

²²The “Treu Package” reduced the sanctions for violating the discipline on temporary contracts pertaining to the conversion into permanent ones, legalised temporary work agencies, and also liberalised apprenticeship contracts and regulated “atypical” ones; whereas *Legislative Decree 368/2001* replaced the detailed list of specific reasons among which firms were mandated to choose in order to motivate their choice of a fixed-term work relationship with a unique general one having “technical, organisational, production or replacement nature”, thus *de facto* disengaging firms from the burden of having to provide a particular reason for their use of a temporary contract.

²³Using aggregate firm data on 8 collective bargaining agreements (CCNLs), across which the implementation of the reform was staggered, Cappellari et al. (2012) find no significant effect of the reform of fixed-term contracts on the growth rate of the latter, but rather find evidence of increased use on external personnel. Moreover, not only they find evidence of substitution between temporary and permanent contracts ($\eta \approx 1$), they find that substitution effects are even higher across different types of temporary contracts ($\eta = 1.4$), and argue that such result might be ascribable to the increased uncertainty surrounding the usage of temporary contracts stemming from the removal of the “specific reasons” to justify it. However, building on the same research design, but using matched employer-employee data with information on 121 CCNLs, Daruich et al. (2020) find that the reform brought about an increase in the incidence of temporary contracts. Furthermore, they find that the reform led to significant wage losses (gains) for temporary (permanent) workers and increasingly to the exclusion of temporary workers from the sharing of firm-specific rents, thus amplifying both within-firm inequality and the overall duality of the labor market.

2012, stood in stark contrast with the spirit of the previous decade's policies (Massagli, 2018) in that it set out to discourage the use of “non-standard” relationships other than the permanent contract (i.e. temporary contracts, apprenticeships and collaborations) by making their use more onerous in a number of ways²⁴. Importantly, though, such reform also intervened on the margin of permanent contracts. Already prior to it, Italy had size-contingent firing costs for permanent contracts, which were sharply higher for firms with more than 15 employees²⁵. What the reform also did, then, was intervening on the firing cost wedge at the 15-employee threshold by operating a significant reduction of it²⁶.

The second set of interventions is part of a broader collection of policies which came to be known as the “Jobs Act”. Our focus here is on the three key measures therein contained that affected the EPL gap. As with the previous reform, these measures intervened on both the temporary and permanent contracts margin, although going only partially in the same direction. In fact, the first intervention, the so-called “Decreto Poletti”, *Decree-Law 34/2014* issued in March 2014 and converted into *Law 78/2014* in May, marked a sharp U-turn in the legislation governing temporary contracts in that it drastically relaxed the legal constraints on their use, going even beyond the 2011 reform regime (e.g. it abolished the obligation to specify any usage justification at all for *all* temporary contracts)²⁷.

Conversely, the two subsequent “Jobs Act” policies implemented shortly after, which intervened on the permanent contracts margin, built upon the framework laid by the analogo-

²⁴As to temporary contracts, the reform (i) increased pre-existing social security contributions by 1.4% of pre-tax earnings, redeemable in case of conversion to permanent contract, (ii) capped the maximum duration to 36 months, (iii) removed the need to specify the “generic” motivation for first-time fixed-term contracts of less than 12 months but ruling out the possibility of renewal for such “free” contracts, (iv) increased the minimum time interval between renewals (from 10 to 60 days for contract of less than 6 months and from 20 to 90 for longer ones); while apprenticeships were subject to some restrictions and other “atypical” (i.e. collaboration) contracts were either made more bureaucratically cumbersome or outright abolished.

²⁵In Italy, permanent workers can be fired either on the basis of misbehavior (*giusta causa o giustificato motivo soggettivo*) or because of re-organizational needs of the firm (*giustificato motivo oggettivo*); prior to the “Fornero labor reform”, in the event that a dismissal of a permanent worker were to be deemed “unfair” by a court, firms with more than 15 employees, to which Article 18 of *Law 300/1970* (“Statuto dei lavoratori”) applied at that time, would have to either reinstate the worker and compensate her for the earnings forgone between the firing and the sentence or pay her a severance payment equal to 15 months of salary, thus facing significantly higher potential costs than those with less than 15 employees which instead could choose between granting reinstatement (and no compensation) and severance payment (ranging from 2.5 to 14 months according to seniority) (Schivardi and Torrini, 2008; Hijzen et al., 2017; Bratti et al., 2021).

²⁶Specifically, regarding unjustly-dismissed permanent workers in above-threshold firms, (i) it lowered the amounts of the monetary compensation owed to the worker and diversified them by case, (ii) it reduced the uncertainty around the litigation’s duration and costs and speeded up the legal process, (iii) and, in some cases, it limited the choice of workers to either being reinstated or obtaining compensation (Bratti et al., 2021).

²⁷Furthermore, the “Decreto Poletti” (i) increased the maximum number of renewals from 1 to 5, (ii) restored the time intervals between renewals to the shorter pre-“Fornero law reform” durations, (iii) and imposed a 20% limit on the share of temporary workforce in a given firm.

gous margin-specific intervention in the “Fornero law reform” and reshaped it even further. In particular, through *Legislative Decree 23/2015* the government introduced a *new graded security permanent contract* for all new permanent hires, by all firms, after March 7th, 2015. Such new contract featured (i) severance payments increasing with tenure, (ii) a phasing out of the mandatory worker reinstatement in case of unfair dismissal in force under the previous regime, and (iii) an additional reduction of the judicial discretion in the legal process ([Boeri and Garibaldi, 2019](#); [Bovini and Viviano, 2018](#); [Sestito and Viviano, 2018](#)). The introduction of the graded security contract thus dramatically relaxed the legislation governing the firing costs of *newly-hired* permanent workers in firms with more than 15 employees.

In addition, in an attempt to further boost and promptly kickstart the creation of permanent jobs, a few months before introducing the new graded security permanent contract, with the *2015 Budget Law* the government introduced a very generous but provisional (3-year-long) *hiring subsidy* for all new permanent hires carried out between January and December of 2015, on the condition that the hired workers had not already been under a permanent worker during the previous 6 months. The subsidy was proportional to gross wages but was capped at 8,060 euros annually. As such, it was relatively more generous for small firms. Under analogous conditions, the subsidy was renewed by the *2016 Budget Law* which, however, more than halved the cap (to 3,250 euros annually) and reduced its duration to 2 years. Therefore, effectively, all new permanent hires from 2017 onwards did not enjoy any rebate at all of social security contributions, while all those who had benefited from the provisional hiring subsidy ceased to do so since January 1st, 2018.

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

January-February 2018

The election campaign reaches its climax. Political parties begin to publish their electoral programs. On January 18th, the 5 Star Movement, the party leading in the polls that will advocate to enact the temporary contracts reform once a government is formed, publishes its program containing a dossier on labor, which under the section “Labor Contracts” reads (ed., bold text as in original): “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.”.²⁸

²⁸The original document can be retrieved at: <https://www.bollettinoadapt.it/wp-content/uploads/2018/02/Lavoro.pdf>.

March-May 2018

The general election is held on March 4th. The 5 Star Movement is the political party securing 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).²⁹ Since it secured the relative majority in parliament, it is expected to lead the efforts to form a government. However, it needs to find some coalition partner(s) in order to form a parliamentary majority that can express a vote of confidence supporting a government. Negotiations begin to find a parliamentary majority. After a long period of great uncertainty (89 days, the longest ever in the history of the Italian Republic), the 5 Star Movement forms a majority together with the Northern League, which leads to the formation of the new government headed by PM Giuseppe Conte on June 1st.

June 2018

Shortly after the new government took office, the leader of the 5 Star Movement, MP Luigi Di Maio, gives his very first interview as the new Ministry of Labor on June 19th to the national economic newspaper, “Il Sole 24 Ore”, in which he presents the “Decreto Dignità”, broadly confirming the details anticipated in the January electoral program.³⁰

July-August 2018

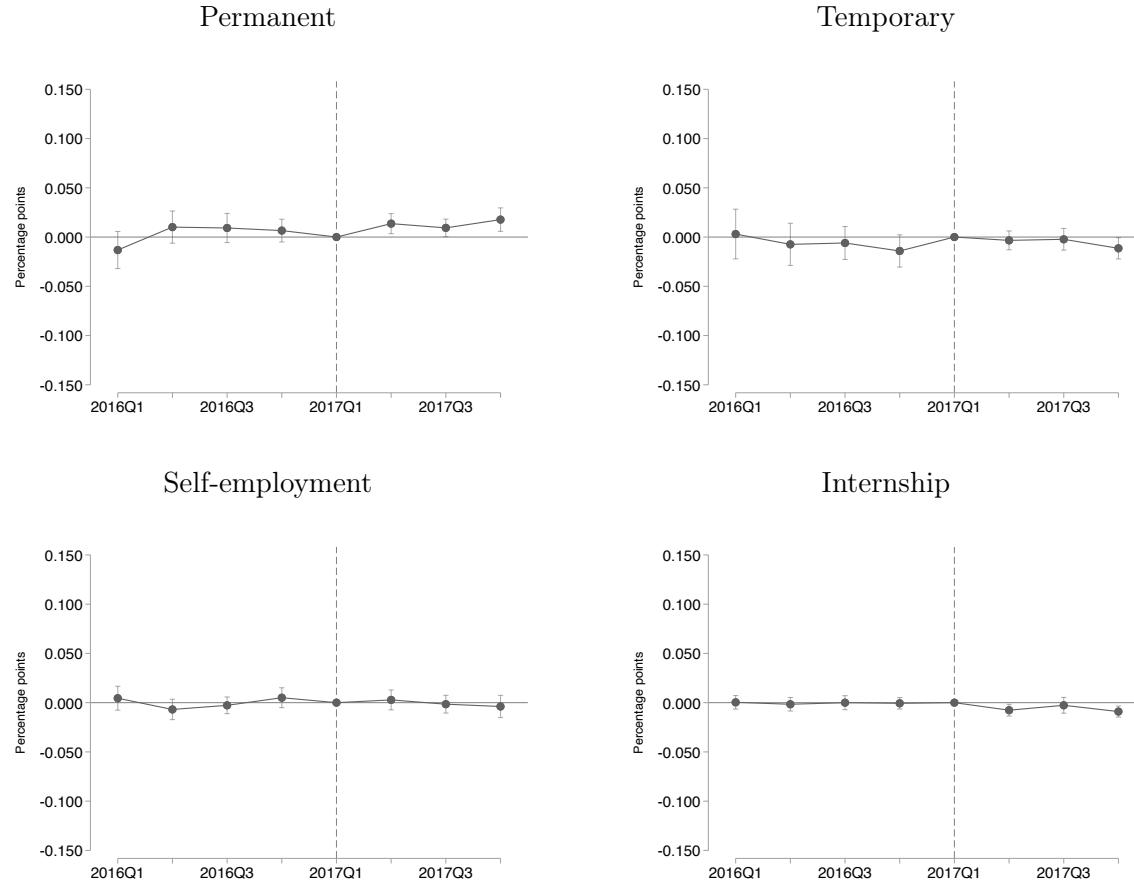
The “Decreto Dignità” is the first legislative act of the new government. It enters into law following the issuance of the *Decree-Law 87/2018* on July 12th and is later converted, with minor modifications, into *Law 96/2018* on August 9th.

²⁹Official 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>.

³⁰The interview is available at: https://www.ilsole24ore.com/art/parla-maio-piu-incentivi-lavoro-stabile-e-stretta-contratti-termine-AEzFsT8E?refresh_ce=1.

C Robustness checks

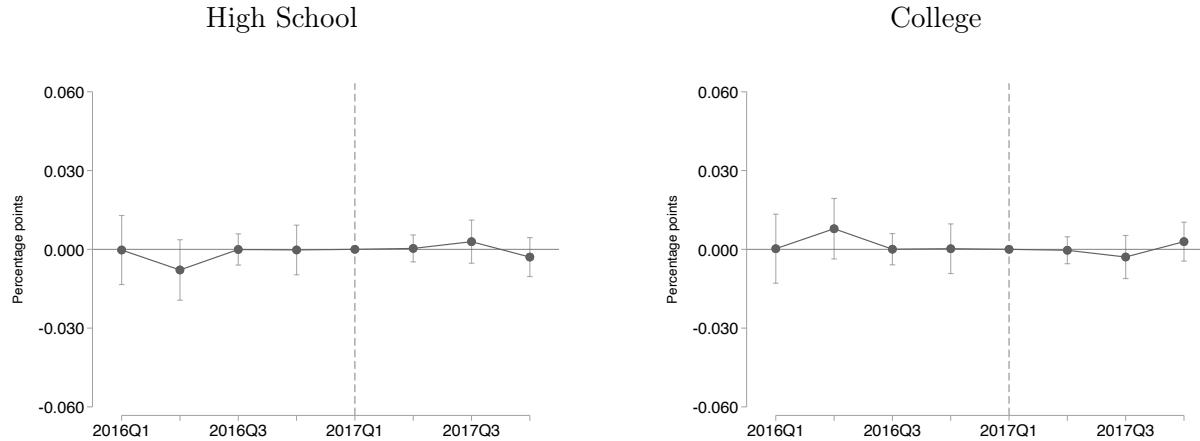
Figure C.1: Labor demand by contract type (Overall: out of all vacancies.)
 Placebo test: $t \in [2016Q1, 2017Q4]$.



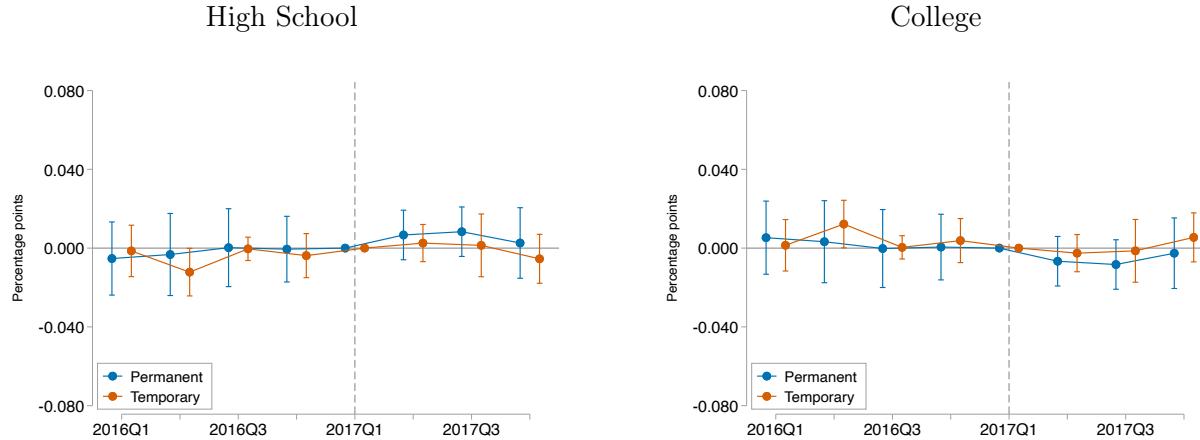
Note: The figure shows placebo tests, based on the specification in Equation (4), conducted by shifting the time frame to $[2016Q1, 2017Q4]$ (the reform was implemented in 2018Q3) when estimating treatment effects on the share of total vacancies posting job positions under a permanent contract (top-left), temporary contract (top-right), self-employment (bottom-left), and internship (bottom-right). Outcome shares are computed within each province-sector-occupation-quarter cell, which is the unit of analysis. The treatment indicator takes value 1 for provinces with high exposure to the reform (defined as $\text{exposure}_p \geq p50$) and 0 for provinces with low exposure ($\text{exposure}_p < p50$). The estimated treatment effect coefficients $\{\theta_\tau\}_{\tau \neq 2017Q1}$ from Equation (4) are plotted alongside 95% confidence intervals. They capture outcome differences at each quarter between provinces with high and low exposure to the reform with respect to 2017Q1. Standard errors are clustered at the province level p . In the regressions, each cell-level observation is weighted by the total labor force in the corresponding province-sector-occupation cell in 2017 times the cell's share of ads within each calendar quarter.

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

a. All vacancies

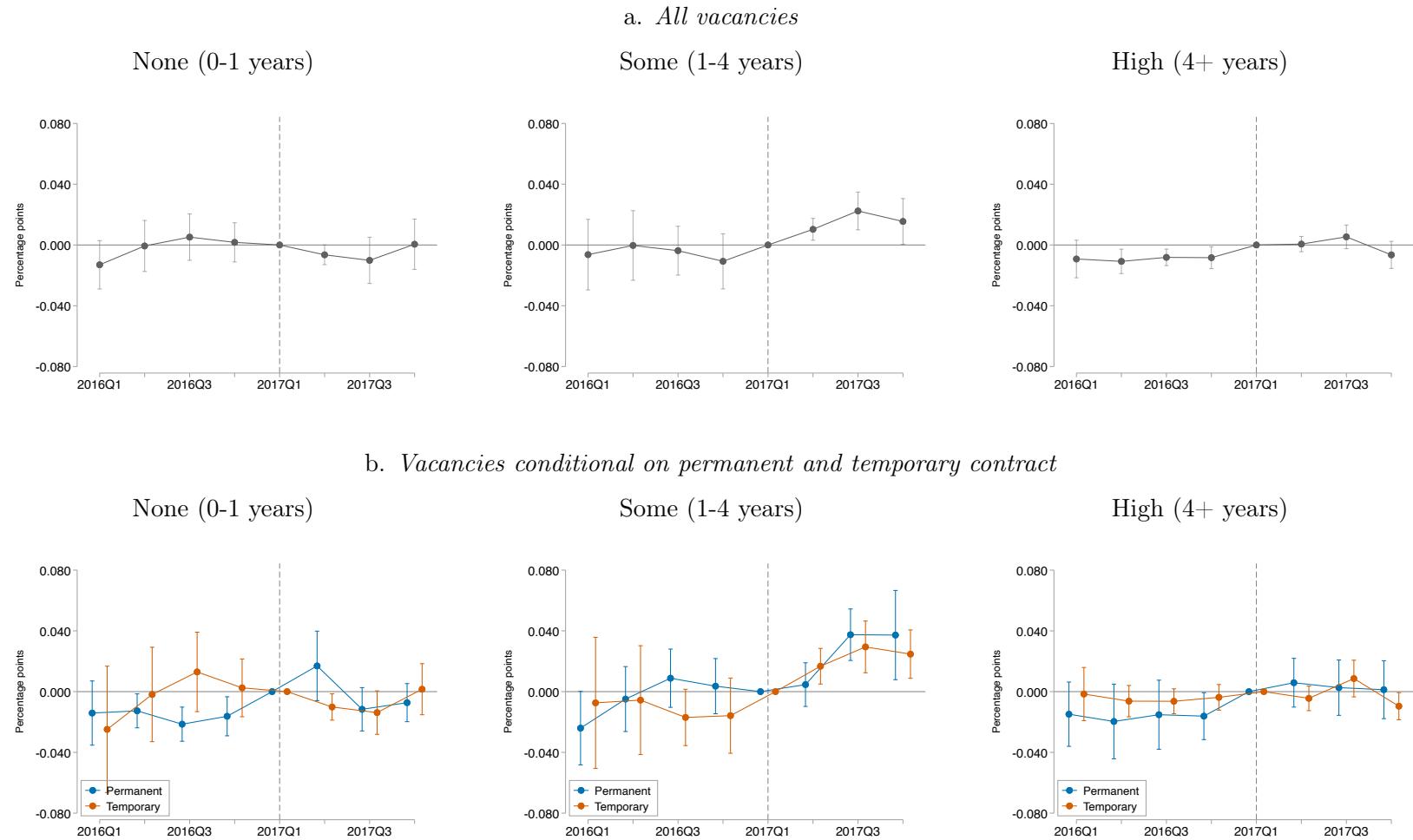


b. Vacancies conditional on permanent and temporary contract



Note: The figure shows placebo tests, based on the specification in Equation (4), conducted by shifting the time frame to [2016Q1, 2017Q4] (the reform was implemented in 2018Q3) when estimating treatment effects on the share of vacancies requiring up to secondary education, i.e., high school (left column), and post-secondary education, i.e., a college degree (right column) respectively out of total vacancies (panel a, in grey), out of permanent job vacancies (panel b, in blue), and out of temporary job vacancies (panel b, in orange). Outcome shares are computed within each province-sector-occupation-quarter cell, which is the unit of analysis. The treatment indicator takes value 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 95% confidence intervals. They capture outcome differences at each quarter between provinces with high and low exposure to the reform with respect to 2017Q1. Standard errors are clustered at the province level p . In the regressions, each cell-level observation is weighted by the total labor force in the corresponding province-sector-occupation cell in 2017 times the cell's share of ads within each calendar quarter.

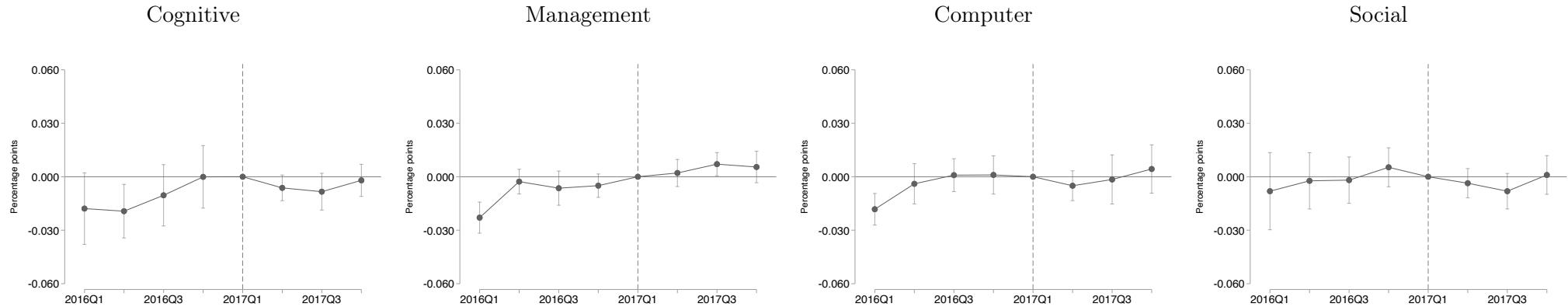
Figure C.3: Experience requirements.
Placebo test: $t \in [2016Q1, 2017Q4]$.



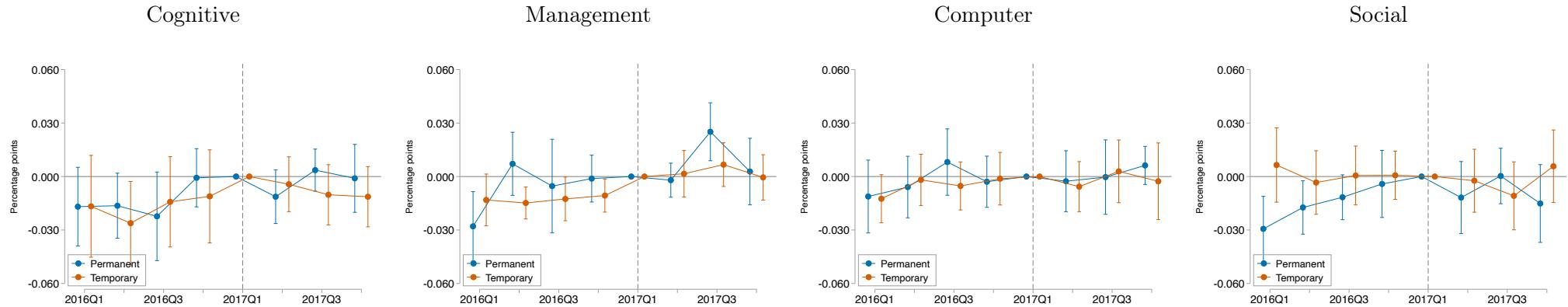
Note: The figure shows placebo tests, based on the specification in Equation (4), conducted by shifting the time frame to [2016Q1, 2017Q4] (the reform was implemented in 2018Q3) when estimating treatment effects on the share of vacancies requiring no experience, i.e., 0-1 years (left column), some experience, i.e., 1-4 years (center column) and high experience, i.e., 4+ (right column) skills respectively out of total vacancies (panel a, in grey), out of permanent job vacancies (panel b, in blue), and out of temporary job vacancies (panel b, in orange). Outcome shares are computed within each province-sector-occupation-calendar quarter cell, which is the unit of analysis. The treatment indicator takes value 1 for provinces with high exposure to the reform (defined as $\text{exposure}_p \geq p50$) and 0 for provinces with low exposure ($\text{exposure}_p < p50$). The estimated treatment effect coefficients $\{\theta_\tau\}_{\tau \neq 2017Q1}$ from Equation (4) are plotted alongside 95% confidence intervals. They capture outcome differences at each quarter between provinces with high and low exposure to the reform with respect to 2017Q1. Standard errors are clustered at the province level p . In the regressions, each cell-level observation is weighted by the total labor force in the corresponding province-sector-occupation cell in 2017 times 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

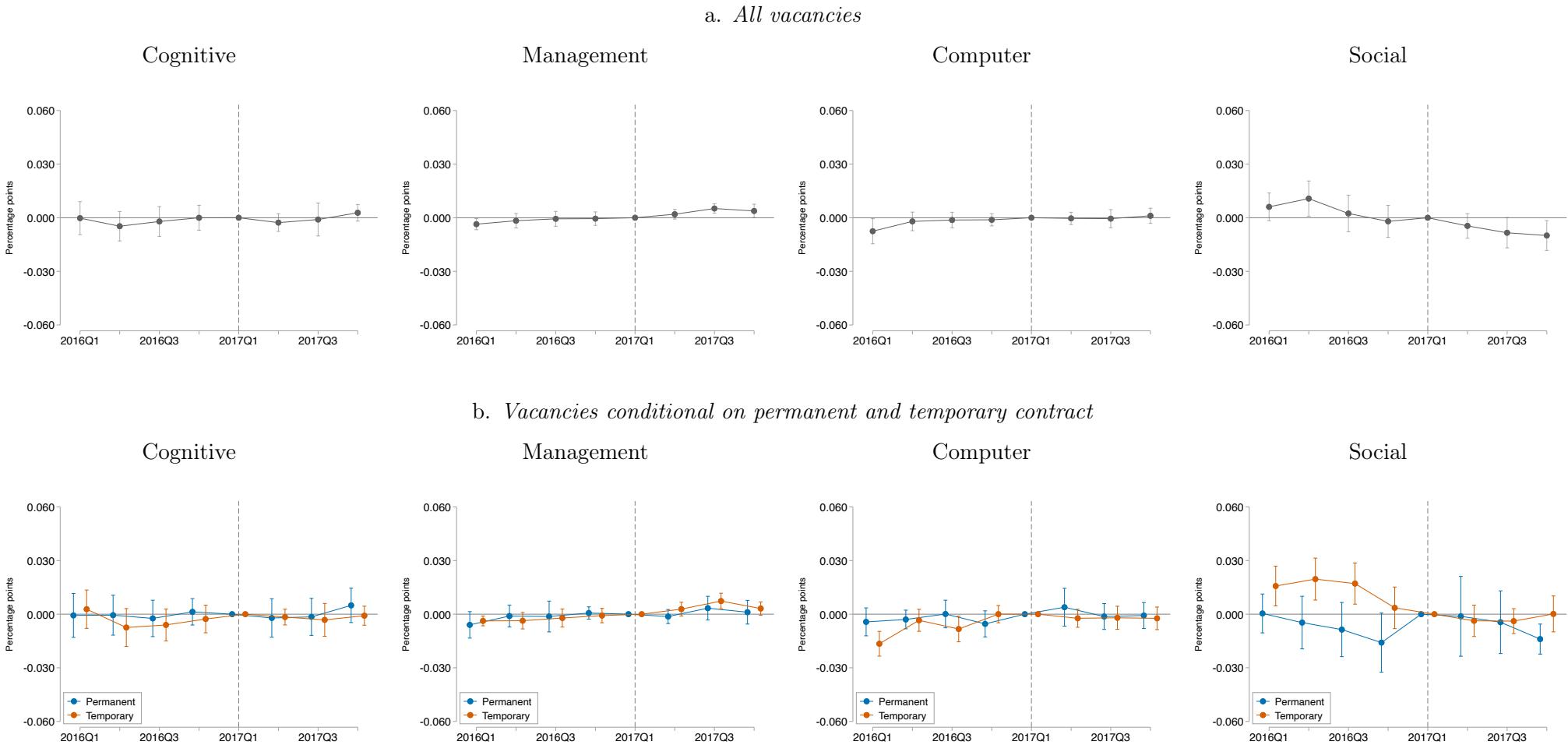


b. Vacancies conditional on permanent and temporary contract



Note: The figure shows placebo tests, based on the specification in Equation (4), conducted by shifting the time frame to [2016Q1, 2017Q4] (the reform was implemented in 2018Q3) when estimating treatment effects on the share of vacancies requiring cognitive (first column), management (second column), computer (third column), and social (fourth column) skills respectively out of total vacancies (panel a, in grey), out of permanent job vacancies (panel b, in blue), and out of temporary job vacancies (panel b, in orange). Outcome shares are computed within each province-sector-occupation-calendar quarter cell, which is the unit of analysis. The treatment indicator takes value 1 for provinces with high exposure to the reform (defined as $\text{exposure}_p \geq p50$) and 0 for provinces with low exposure ($\text{exposure}_p < p50$). The estimated treatment effect coefficients $\{\theta_\tau\}_{\tau \neq 2017Q1}$ from Equation (4) are plotted alongside 95% confidence intervals. They capture outcome differences at each quarter between provinces with high and low exposure to the reform with respect to 2017Q1. Standard errors are clustered at the province level p . In the regressions, each cell-level observation is weighted by the total labor force in the corresponding province-sector-occupation cell in 2017 times the cell's share of ads within each calendar quarter.

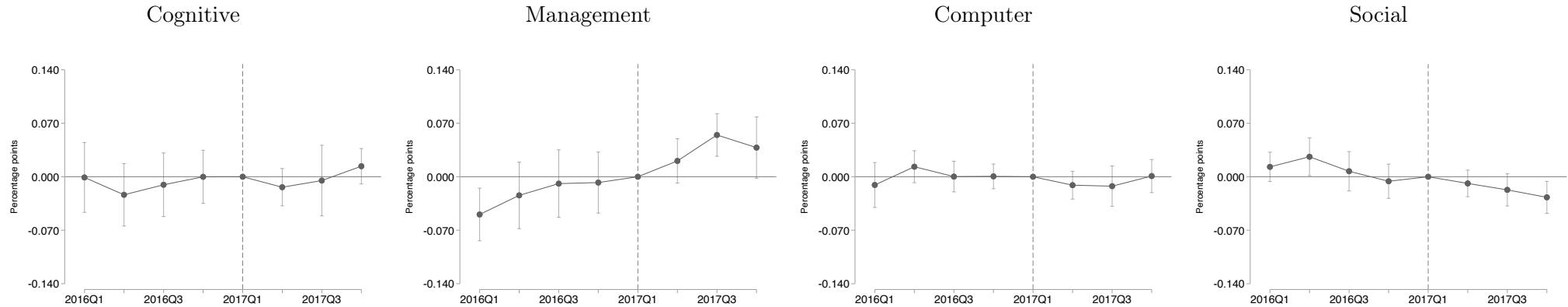
Figure C.5: Specific skill requirements, *within*-cells relative importance.
 Placebo test: $t \in [2016Q1, 2017Q4]$.



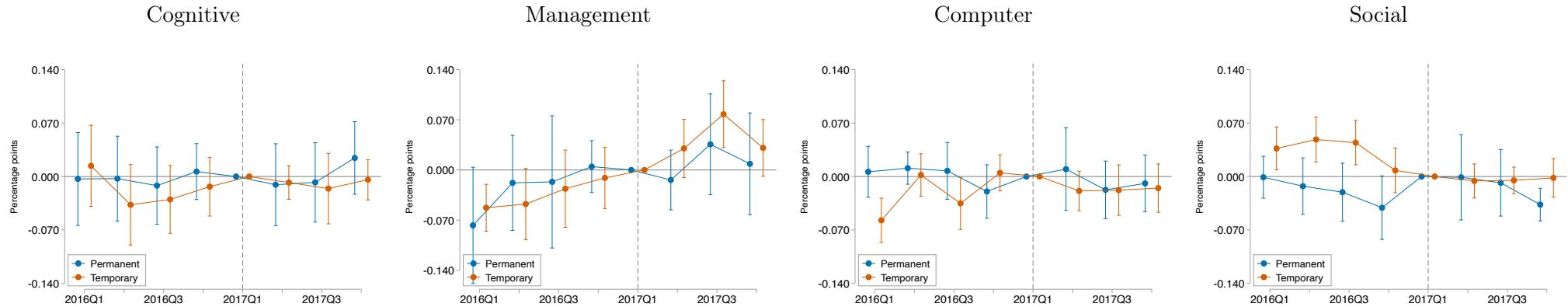
Note: The figure shows placebo tests, based on the specification in Equation (4), conducted by shifting the time frame to [2016Q1, 2017Q4] (the reform was implemented in 2018Q3) when estimating treatment effects on the *within*-cells relative importance indicators for cognitive (first column), management (second column), computer (third column), and social (fourth column) skills respectively for total vacancies (panel a, in grey), for permanent job vacancies (panel b, in blue), and for temporary job vacancies (panel b, in orange). Indicators are computed within each province-sector-occupation-calendar quarter cell, which is the unit of analysis. The treatment indicator takes value 1 for provinces with high exposure to the reform (defined as $\text{exposure}_p \geq p50$) and 0 for provinces with low exposure ($\text{exposure}_p < p50$). The estimated treatment effect coefficients $\{\theta_\tau\}_{\tau \neq 2017Q1}$ from Equation (4) are plotted alongside 95% confidence intervals. They capture outcome differences at each quarter between provinces with high and low exposure to the reform with respect to 2017Q1. Standard errors are clustered at the province level p . In the regressions, each cell-level observation is weighted by the total labor force in the corresponding province-sector-occupation cell in 2017 times the cell's share of ads within each calendar quarter.

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

a. All vacancies

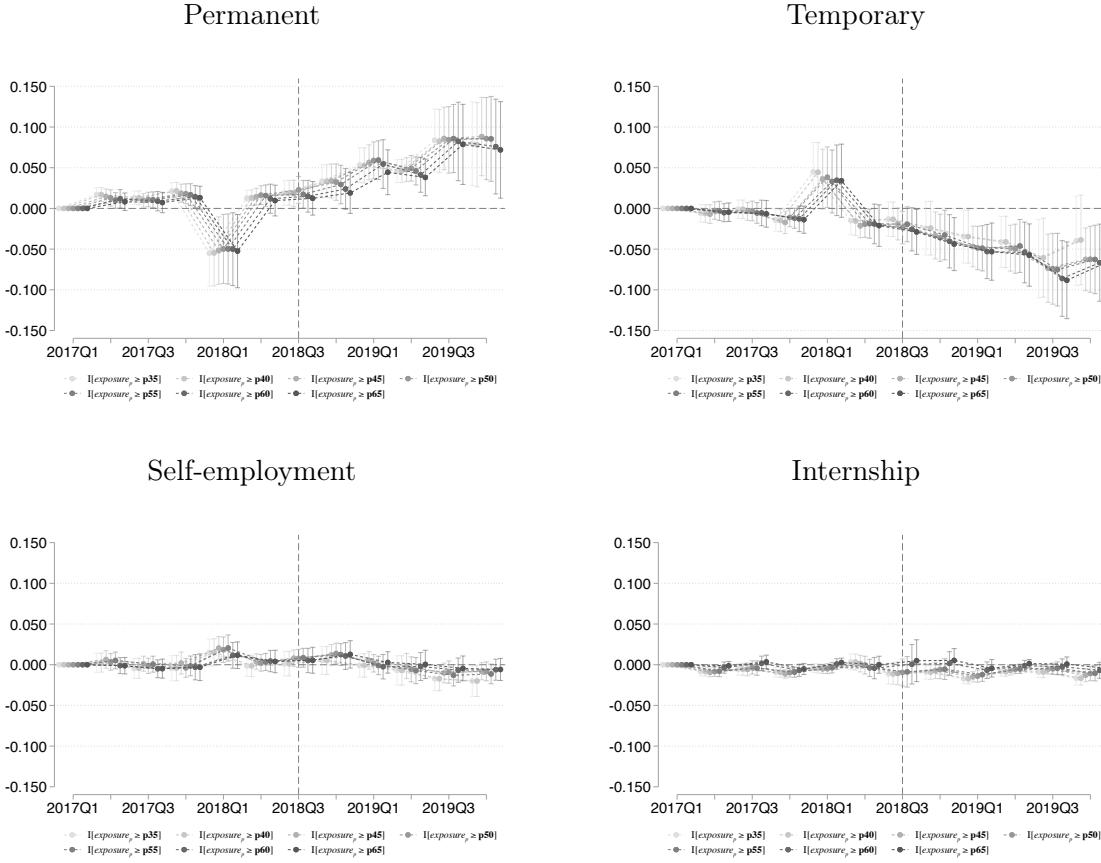


b. Vacancies conditional on permanent and temporary contract



Note: The figure shows placebo tests, based on the specification in Equation (4), conducted by shifting the time frame to [2016Q1, 2017Q4] (the reform was implemented in 2018Q3) when estimating treatment effects on the *between-cells* relative importance indicators for cognitive (first column), management (second column), computer (third column), and social (fourth column) skills respectively for total vacancies (panel a, in grey), for permanent job vacancies (panel b, in blue), and for temporary job vacancies (panel b, in orange). Indicators shares are computed within each province-sector-occupation-calendar quarter cell, which is the unit of analysis. The treatment indicator takes value 1 for provinces with high exposure to the reform (defined as $\text{exposure}_p \geq p_{50}$) and 0 for provinces with low exposure ($\text{exposure}_p < p_{50}$). The estimated treatment effect coefficients $\{\theta_\tau\}_{\tau \neq 2017Q1}$ from Equation (4) are plotted alongside 95% confidence intervals. They capture outcome differences at each quarter between provinces with high and low exposure to the reform with respect to 2017Q1. Standard errors are clustered at the province level p . In the regressions, each cell-level observation is weighted by the total labor force in the corresponding province-sector-occupation cell in 2017 times the cell's share of ads within each calendar quarter.

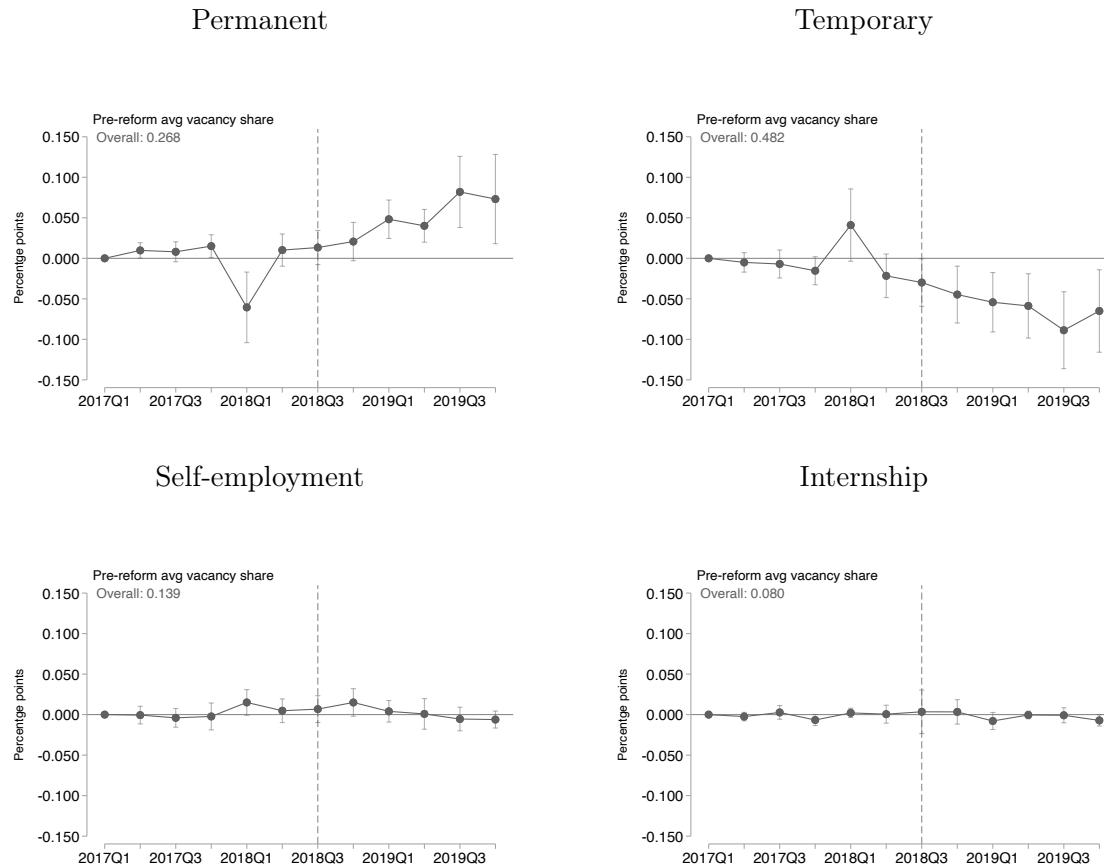
Figure C.7: Labor demand by contract type (Overall: out of all vacancies.)
 Cutoff sensitivity: $\mathbb{1}[exposure_p \geq c]$ with $c \in \{p35, p40, p45, p50, p55, p60, p65\}$.



Note: The figure shows a robustness test, based on the specification in Equation (4), that addresses the sensitivity of the main estimates on the share of total vacancies posting job positions under a permanent contract (top-left), temporary contract (top-right), self-employment (bottom-left), and internship (bottom-right) to a change of the baseline cutoff ($p50$) chosen to define the treatment indicator based on our exposure measure ($\mathbb{1}[exposure_p \geq c]$), using cutoffs $c \in \{p35, p40, p45, p50, p55, p60, p65\}$. Outcome shares are computed within each province-sector-occupation-quarter cell, which is the unit of analysis. The treatment indicator takes value 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 estimated treatment effect coefficients $\{\theta_\tau\}_{\tau \neq 2017Q1}$ from Equation (4) are plotted alongside 95% confidence intervals. They capture outcome differences at each quarter between provinces with high and low exposure to the reform with respect to 2017Q1. Standard errors are clustered at the province level p . In the regressions, each cell-level observation is weighted by the total labor force in the corresponding province-sector-occupation cell in 2017 times 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 shows a robustness test, based on the specification in Equation (4), that addresses the sensitivity of the main estimates on the share of total vacancies posting job positions under a permanent contract (top-left), temporary contract (top-right), self-employment (bottom-left), and internship (bottom-right) to an alternative definition of the exposure-to-treatment indicator variable, such that it takes value 1 if exposure_p belongs to the top tercile of its distribution, and 0 if it belongs to the bottom tercile. This entails dropping observations for provinces with exposure in the medium tercile. Outcome shares are computed within each province-sector-occupation-calendar quarter cell, which is the unit of analysis. The estimated treatment effect coefficients $\{\theta_\tau\}_{\tau \neq 2017Q1}$ from Equation (4) are plotted alongside 95% confidence intervals. They capture outcome differences at each quarter between provinces with high and low exposure to the reform with respect to 2017Q1. Standard errors are clustered at the province level p . In the regressions, each cell-level observation is weighted by the total labor force in the corresponding province-sector-occupation cell in 2017 times the cell's share of ads within each calendar quarter.