The Effects of Salary Caps for Public Managers: Evidence from Italy

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Abstract

This paper explores how wage caps for public sector managers affect their career trajectories and manager selection into public employment. We study a wage cap introduced in Italy in 2014 leveraging matched employer-employee data for public employees and an event study design. Managers of public companies affected by the cap are more likely to leave their employer and move to a private sector job. Managers moving to the private sector are on average more productive than those who stay in the public sector. The co-workers of leaving managers are also more likely to leave public employment, and these effects are again concentrated among high-productivity individuals. We estimate that the reform led to a drop in the productivity of public management of 10 percent of a standard deviation, while reducing the costs of public employment by 0.1 percent.

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1. Introduction

In many advanced economies, the public sector represents a significant portion of total employment, averaging around 20 percent (OECD, 2021). Public employees are responsible for delivering essential services and play a crucial role in the functioning of modern democracies. Despite this, concerns about inefficiencies and the absence of competitive pressures have made the public sector a frequent subject of criticism (Garibaldi and Gomes, 2020; Boeri et al., 2021). Among the various proposals to reduce public employment costs, capping the salaries of public managers has proven particularly popular; within the European Union alone, eleven countries have implemented such measures (Bruni, 2017). However, the effects of these policies remain poorly understood. Salary caps could discourage productive managers, potentially undermining the quality of public employment. Despite their pivotal role in overseeing public service delivery, implementing policies, and improving the productivity of their teams (Fenizia, 2022), research on how public managers respond to wage incentives is limited.

This paper fills this void by examining the effects of a salary cap introduced in Italy in 2014, which applied to all employees in public administration or state-owned enterprises. The cap, set at €240,000 gross per year, primarily targeted top public managers, leading to a net reduction in their salaries. Our analysis focuses on the career trajectories of the affected managers, exploring whether the policy increased their likelihood of leaving their positions and, if so, whether they chose retirement or moved to the private sector. Furthermore, we investigate potential spillover effects on the colleagues of these managers and assess the policy's broader implications for the productivity of public sector management.

Our analysis leverages two primary data sources. The first is matched employer-employee data from the Italian Social Security Institute (INPS), covering the entire population of

private sector workers and firms in Italy. The second is matched employer-employee data for all public sector employees in Italy, which includes detailed information on employees that is typically unavailable in social security records.[1] This data enables us to identify all public managers, measure their wages, and determine whether they were affected by the salary cap policy.

These two datasets allow us to track transitions between the public and private sectors, from public to private employment and vice versa. Our main analysis focuses on managers employed by wholly or partly state-owned companies operating in market-oriented industries such as transport, finance, and communications. These managers are particularly interesting as they will likely have outside opportunities in the private sector. We refer to this subset of public employees as "public-in-private."

Our empirical strategy employs a difference-in-differences approach to compare the career trajectories of managers affected by the salary cap with those unaffected before and after the policy's introduction. The baseline treatment group consists of public-in-private managers earning €240,000 or more in 2014 who experienced a direct wage reduction due to the reform. As controls, we include public managers earning below the cap but above €150,000 in 2014. To ensure comparability, we select control managers using a one-to-one exact matching algorithm within age*gender cells.

Our analysis confirms that treated and control managers followed parallel trends before 2014. By 2020, six years after the cap's introduction, treated managers were approximately ten percentage points more likely to have left their 2014 firm than control managers—an effect size of about 14 percent. This increase in turnover is almost entirely attributable to managers transitioning to private sector jobs, with no significant effect observed on

[1] In many countries public sector employees are not formally paying social security contributions and are therefore not included in administrative data drawn from social security registrars.

retirement probabilities. Additionally, treated managers experienced an average weekly wage reduction of 5-10 percent, concentrated among those who remained in the public sector. This resulted in an overall decline in annual labor earnings due to the reform.

Our findings are robust across various alternative matching approaches. These include considering potential control managers earning above €100,000 or €200,000 in 2014 (rather than the baseline threshold of €150,000), using one-to-two instead of one-to-one matching, and running event studies on the unmatched sample of managers. Additionally, we conduct a placebo analysis using a hypothetical reform in 2010, which produces no meaningful effects, further validating our methodology.

We extend the analysis to public sector employees in sectors with limited private sector employment opportunities, such as health, defense, education, and public administration. Due to data constraints, we observe individuals in these sectors only from 2014 onward. Our estimates for these employees are small and statistically insignificant, suggesting that public administration managers have fewer outside options than those in state-owned companies (our baseline sample).

Next, we examine potential spillover effects on the control group. While control managers earning below the cap were not directly affected by an immediate wage reduction, introducing the cap could lower their expected or potential future earnings. A placebo exercise comparing public managers earning just below the cap (€200,000-€240,000) to managers further away from it (€150,000-€200,000) reveals a small (2 percentage points) but imprecisely estimated increase in the likelihood of transitioning to the private sector among control managers. This effect aligns with the direction observed in our baseline analysis, suggesting that the baseline estimates may represent a lower bound for the true average treatment effect of the policy.

We further explore how the salary cap influenced the selection of managerial talent in public employment. To approximate manager productivity, we use worker-fixed effects estimated from an AKM wage regression (Abowd et al., 1999) applied to Italian public and private sector workers in the pre-policy years. Managers are classified as high- or low-quality based on whether their estimated AKM effect falls above or below the group median, and we estimate our models separately for these two groups.

Our findings reveal that the increased likelihood of treated managers leaving public employment for the private sector is entirely driven by high-AKM managers. Moreover, public firms that lose these managers fail to replace them with individuals of comparable quality, indicating a decline in the overall productivity of public management. We quantify this drop as approximately 10 percent of a standard deviation relative to pre-reform levels. This productivity loss far outweighs the total savings from the cap, estimated at €180 million between 2015 and 2020—equivalent to just 0.1 percent of public employment costs.

The overall impact of the wage cap on public employment extends beyond the direct effects on affected managers and may also influence their co-workers. In the final section of the paper, we examine these potential spillover effects by comparing the peers of treated managers who left the public sector for private sector jobs to observationally similar peers of treated managers who remained in public firms. Our analysis confirms parallel pre-trends between the treated and control co-worker groups. We find evidence of an increased propensity for peers of departing managers to leave their firms and transition to the private sector, though these estimates are not precisely measured. A heterogeneity analysis reveals that this effect is concentrated among high-AKM individuals in management and middle-management roles, suggesting that the loss of talented co-workers further exacerbates the wage cap's adverse effects on public sector productivity.

Our work contributes to several strands of literature. It relates to the long-standing literature studying the economics of the public sector, its incentive structures, and hierarchies (Katz and Krueger, 1991; Dustmann and van Soest, 1998; Bradley et al., 2017; Boeri et al., 2021; Fenizia, 2022; Fenizia et al., 2024). More specifically, by providing novel evidence on public wage policies, we add to the body of research examining the role of financial incentives in attracting and motivating public sector workers (Dal Bó et al., 2013; Burgess et al., 2017; Geys et al., 2017; Deserranno et al., 2024).[2] While this literature has focused mainly on specific case studies or field experiments (Finan et al., 2017), our analysis leverages a natural experiment and unique administrative data from social security records.[3] In addition, while existing studies focus on financial incentives in the form of higher pay, our paper examines the opposite scenario of a salary cap.

This paper contributes to the broader literature on managers. While this literature has documented the critical role of managers and managerial practices in driving private sector performance (Bertrand and Schoar, 2003; Bloom and Van Reenen, 2007; Lazear et al., 2015; Giorcelli, 2019; Sauvagnat and Schivardi, 2024), considerably less is known about managerial dynamics in the public sector (e.g., Rasul and Rogger, 2018; Janke et al., 2019). A recent notable study by Fenizia (2022) highlights the pivotal role of managers in enhancing public sector productivity and output using administrative data from Italy. Our work provides new insights into the incentives faced by public executives and their behavioral responses to wage caps. Given the essential role of public managers in modern bureaucracies and the widespread implementation of wage cap policies, our findings hold

[2] These issues are particularly relevant because the definition of public wages falls under the policymaker's prerogative rather than being determined by the market. As a result, public wages can sometimes be susceptible to political manipulation, given that they are often perceived as a transfer benefiting a specific group of individuals (Garibaldi and Gomes, 2020).
[3] Gagliarducci and Nannicini (2013) focus on Italy but consider elected politicians, not public employees. Nieddu and Pandolfi (2022) study instead the effects of promotion incentives in the Italian academia.

important implications for policymakers seeking to balance cost-saving measures with maintaining public sector efficiency and productivity.

This paper makes two additional contributions. First, it adds to the body of work on the impact of pay cuts (e.g., Coviello et al., 2022; Krueger and Friebel, 2022), which has documented negative effects of unexpected salary reductions on worker productivity and effort. Second, it relates to the literature on peer effects in the workplace (e.g., Dahl et al., 2014; Cornelissen et al., 2017; Bianchi et al., 2022; Jäger and Heining, 2022), which has documented how individual characteristics and labor supply shocks affect the careers and wages of co-worker networks. We extend both lines of research to the public sector, where the above effects may be mediated by non-wage amenities and lower job mobility, and highlight how wage caps affect individual decisions and broader organizational dynamics.

The paper is structured as follows. Section 2 provides more detail on the institutional background and the policy and describes the data. Section 3 shows descriptive statistics on the Italian public sector, focusing on managers, and outlines the empirical strategy. Section 4 describes the main results and robustness tests. Section 5 evaluates how the policy has affected the quality of public employment and explores possible spillover effects on co-workers. The last Section concludes.

2. Background, Data and Descriptive Statistics

2.1. Institutional background

The Italian public sector. Public employees account for roughly 13 percent of total employment in Italy (about 3 million workers), compared to an OECD average of 18 percent (OECD, 2021). This share has declined slowly in the past two decades, partly due to several cost-cutting measures such as turnover restrictions. As of 2021, Italian public employees

were, on average, just below 50 years old, almost ten years older than private sector workers. The Italian public sector offers more stable careers and higher weekly earnings than the private sector (€660 versus €590) – see the INPS Annual Report (2023) for more details. Public managers represent roughly 170,000 people, just below 5 percent of public employment. Except for a few categories (e.g., defense), the number of public managers has decreased in line with the rest of public employees (Corte dei Conti, 2020).

The recruitment of public employees, including managers, is highly regulated and occurs almost exclusively through national public calls. The wage progression of managers in public administration depends on firm tenure and experience and is largely determined by national collective bargaining agreements. However, state-owned companies, while still required to follow transparency and impartiality criteria set by the Law (Decree n. 165/2001), have more freedom in setting their hiring and promotion procedures or pay policies and are more similar to private firms. Public managers can be hired with either open-ended or fixed-term contracts. In the latter case, contracts typically last three years.

The policy. We study Law n. 89\2014, which in May 2014 imposed a salary cap of €240,000 per year (gross of taxes) for public sector managers. This measure aimed to reduce public spending, with the government claiming that "*no public manager should be paid more than the President of the Italian Republic.*"[4] The reform involved all managers working in the public administration and in (wholly or partly) state-owned companies, excluding those listed on the stock market.[5] It was de facto targeted at top managers – roughly 0.02 percent of public employees were earning more than the cap in 2014 (the cap

[4] The announcement of this policy was not anticipated, as confirmed by limited media coverage and online searches (based on Google Trends) before Spring 2014. See https://st.ilsole24ore.com/art/notizie/2014-03-21/renzi-vuole-tetto-stipendi-manager-pubblici-pari-indennita-napolitano-239181-euro-182229.shtml?uuid=ABTqCm4&refresh_ce=1.

[5] A wage cap for public administration workers had already been set in 2011 equal to the salary of the President of the Constitutional Court (around €310,000). The 2014 law substantially lowered the cap to €240,000 and extended it to managers of state-owned companies, in addition to those in the public administration.

was also well above the 99th percentile of the wage distribution in the private sector). This policy sparked intense debate in the political arena, with prominent managers of public companies threatening to resign and warning that the cap could harm the quality of public sector management.[6]

2.2. Data

The empirical analysis relies on longitudinal matched employer-employee data for the population of Italian workers in both the public and private sector, sourced from the Italian Social Security Institute (INPS), which we access through the VisitINPS Scholars program. We merge information from various INPS archives.

"Public-in-private". Our primary source is the matched database for the population of Italian workers and firms in the *private* sector. Due to contribution-related reasons, around 240,000 public employees – roughly 7.5 percent of public employment in Italy – are also included in this database (as stated above, we refer to these as "public-in-private" workers). Because this is the only group of public employees that we can observe before the reform year (2014), our analysis will focus predominantly on public-in-private workers. This group includes employees of state-owned companies operating in industries with market-like characteristics such as broadcasting, transportation, and financial services, but not workers in broader public administration, health, or education. We conjecture that the effects of a public sector wage cap are more salient for workers with outside options in the private sector, making this group of public employees particularly interesting to study. In addition, while managers of state-owned companies were, for the first time, covered by a wage cap in 2014, managers of the broader public administration had been subject to a higher cap

[6] See, for example, https://www.ansa.it/amp/english/news/business/2014/04/09/fat-cat-managers-wont-be-missed_2a117bbe-6a36-4892-8ca6-2f0f5b5a8581.html.

(€310,000) since 2011 (see Section 2.1). Appendix A provides more detail on how we identify public employees in the private sector data.

The data span the years between 2005 and 2020. It collects information on workers' earnings, weeks worked, contract status (full- or part-time, temporary or permanent), broad occupation (managers, mid-managers, white collars, blue collars) and demographics (e.g., age, gender, location). We only observe total pay and cannot distinguish between base pay and bonuses. We also use this database to collect information for the population of private sector workers and firms, making it possible to identify worker transitions between the public and the private sector.

Broader public sector. The INPS archives also contain longitudinal matched employer-employee data on broader public sector employment (e.g., public administration, health, education, and defense – the majority of public employees in Italy), reporting information on earnings, sector, contract type, and demographics. However, the data are available only starting in 2014, when the wage cap was announced. Thus, we cannot implement our difference-in-differences strategy (described in the next Section) for these workers, as we cannot estimate pre-reform coefficients for them. That said, we will investigate the effects of the wage cap for this broader group of public employees by examining their post-2014 labor market outcomes – including possible transitions to private firms, as we can track these workers in the private sector database, too.

Firm-level data. Additional firm data is available in the INPS records, reporting the firm's sector, size, age, and location. We merge these data with the worker data using firm identifiers. We also obtained balance sheet data (including information on firm value added) from income statements collected by the Cerved Group. This data, available until 2018, can again be linked to the INPS archives using firm identifiers. Unfortunately, the Cerved balance sheet data primarily cover private-sector firms, with only a limited number of public

companies in our already small sample (see Section 3) matched to it. This limitation prevents us from conducting a comprehensive analysis of the policy's effects on public companies' productivity. Consequently, we will only present suggestive evidence later in the paper.

2.3. Descriptive statistics

Worker characteristics. Table 1 provides an overview of Italian public sector workers' demographic and labor market characteristics, with all statistics referring to 2014. The analysis begins with the entire public sector (Columns 1–3) before focusing on public-in-private workers (Columns 4–6). For each group, we present descriptive statistics for all workers (Columns 1 and 4) and then narrow the focus to those earning above €150,000 in 2014—approximately the 99th percentile of the public wage distribution—referred to hereafter as "top public managers." Among these top public managers, we further differentiate between those earning below the €240,000 cap (Columns 2 and 5) and those earning above it (Columns 3 and 6).

In 2014, the Italian public sector employed almost 3.2 million workers (Column 1). Approximately 7.5% of these workers (around 240,000) were employed in public companies operating in market-like industries and are included in the public-in-private database (Column 4). As previously noted, public-in-private workers differ significantly from the rest of the public employees in Italy: they have a much smaller female share (29% compared to 57%), a higher incidence of temporary contracts, and more extensive prior experience in the private sector. Top public managers (Columns 2–3 and Columns 5–6) exhibit an even lower female share compared to other public employees, and they tend to be older and have more experience on average. The baseline analysis will focus on public-in-private top managers earning more than €150,000 in 2014 – about 1,600 workers (Columns 5-6). Of these, 219 managers earned above €240,000 in 2014 and will constitute our main treatment group

(Column 6).[7] Appendix Table B1 compares these public managers (within industries) to private sector managers earning more than €240,000. We do not find large differences between the two groups, although private sector managers are on average paid more and are younger.

Sectors. Table B2 presents the sector distribution of public-in-private workers, specifically focusing on top public managers in 2014. Most public employees in this database are concentrated in sectors such as financial activities, transportation, information technology, and other services. In contrast, Table B3 provides the sector distribution for the remainder of the public sector, highlighting a predominance of top managers in the central administration compared to other sectors like education, health, and defense.

Transitions. Table 2 provides a descriptive overview of the career outcomes for top public managers following the introduction of the cap. Only 40% of treated managers remained in the public sector after 2014, compared to 63% of managers earning below the cap. The higher exit rate among treated managers is driven by a greater likelihood of retirement (41% versus 24% for managers below the cap) and more frequent transitions to the private sector (13% versus 5% for managers below the cap). However, some of these differences may be attributable to demographic disparities between the two groups—treated managers tend to be older than control managers and are, therefore, closer to retirement (as shown in Table 1). The next section outlines our empirical design, which addresses these imbalances.

3 Empirical Strategy

Baseline strategy. Our identification strategy uses the clear design of the law, where public-in-private workers earning above the €240,000 in 2014 constitute the treatment

[7] In the full public sector, there were 657 public managers earning above the cap in 2014 (Column 2 in Table 1). Notably, the public-in-private sample accounts for one-third of the total number of managers affected by the cap (219), despite representing less than one-tenth of total public sector employment.

group (see column 6 of Table 1). The "broad" control group includes all public-in-private managers earning below the cap. To improve comparability between the treated and control groups, we first consider public sector workers with annual wages between €150,000 and €240,000 in 2014 (Table 1 Column 5).[8] Because the treatment group still includes a higher share of male and older workers (Columns 5–6 in Table 1), we further refine the control group using a simple matching strategy. Specifically, we assign each treated public manager to one control public manager, selected randomly from within the same age*gender cell and earning between €150,000 and €240,000 in 2014.[9] Nearly all treated managers (213 out of 219) are successfully matched to a control unit (without replacement), with descriptive statistics provided in Table B4, Columns 1–2. Aside from the (mechanical) differences in earnings, treated and control public managers exhibit very similar observed characteristics.

Based on this sample we compare the evolution of outcomes of treated and control managers before and after the introduction of the wage cap.[10] The main outcomes of interest include the probability of a manager leaving their job, retirement probability, transitions to the private sector, weeks worked, and earnings. By tracking these outcomes over time, we aim to identify and quantify any significant differences between treated and control managers, highlighting the impact of the wage cap policy on their career trajectories and labor market behavior. We estimate the following event-study regression:

$$y_{i,t} = \alpha_i + \theta_t + \sum_{j \neq 2013} \rho_j \cdot \mathbb{I}[t = j] \cdot T_i + \varepsilon_{i,t} \qquad (1)$$

Here, $y_{i,t}$ denotes the outcome of interest for worker i in year t, α_i and θ_t are worker and year-fixed effects, respectively, and T_i is a treatment indicator taking the value of one if

[8] As we will demonstrate below, the results remain robust to alternative specifications of this lower bound for the wages of potential control groups.
[9] We test the robustness of our results using alternative matching strategies below.
[10] Due to our decision to narrow the control group around the €240,000 threshold, our design closely resembles a difference-in-discontinuities approach (Grembi et al., 2016).

worker i's wage was above the €240,000 cap in 2014. The coefficients of interest ρ_j capture the difference in outcomes between treated and control managers each year compared to the same difference in 2013 (the year before the implementation of the cap), which is normalized to zero. Standard errors are clustered at the worker's level.

The causal interpretation of the event-study coefficients relies on a standard parallel trends assumption: labor market outcomes of managers earning above and below the cap in 2014 would have evolved similarly without the policy. We will test parallel trends by plotting the estimated ρ_j coefficients in the years *before* the cap was implemented.

Spillovers. A potential concern with this design is that the imposition of a salary cap may affect not only the actual wages of managers earning above the cap but also the expected wages of those earning below it. As a result, the reform could influence our control group, potentially violating the Stable Unit Treatment Value Assumption (SUTVA). If these spillover effects align in the same direction as the ATT, our estimates would represent a lower bound for the policy's true impact. We address these potential spillover effects in Section 4.2.

Effects on co-workers. We extend our analysis to the potential impact of the policy on the co-workers of public managers affected by the policy, comparing the co-workers of *treated* managers who quit their jobs and moved to the private sector (treated group) to the co-workers of *treated* managers who stay at the public firm (control group).[11] We cannot directly observe teams in the data. Therefore, we adopt a broader definition of co-workers,

[11] To define the treatment group, we consider the co-workers of treated managers who left the public firm in the years after the introduction of the cap but before 2020 and moved to the private sector (27 of 219 treated managers, see Table 2). To define the control group, we consider the co-workers of treated managers who remained at their 2014 (public) company until 2020 (87 out of 219 treated managers, see Table 2).

including all workers employed in the same firm as a treated manager in 2014, the reform's implementation, with earnings below the cap.

Since the co-workers of managers who leave the public firm may differ from those of managers who stay, we perform one-to-one nearest-neighbor propensity score matching to ensure a more comparable set of co-workers for analysis. This matching is based on individual characteristics, including age, gender, tenure, and wage in 2014, within the same occupational group, resulting in a balanced sample of 2,369 treated co-workers and an equal number of control co-workers.[12] Appendix Table B5 provides a detailed description of this sample, separately presenting characteristics for co-workers in management and middle-management occupations (Columns 1–2) and those in other occupations, such as white- and blue-collar roles (Columns 3–4). Using this balanced sample, we estimate the following regression model:

$$y_{i,t} = \alpha_i + \theta_t + \sum_{j \neq 2013} \delta_j \cdot \mathbb{I}[t = j] \cdot LM_i + \upsilon_{i,t} \qquad (2)$$

Here, the treatment indicator LM_i takes the value of one if worker i's manager has left public firm and zero if their manager has remained. Standard errors are clustered at the level of the 2014 firm*municipality cell. Again, we include worker and year-fixed effects. While this design cannot account for unobserved, time-varying differences between co-workers of leaving versus staying managers, we will again inspect the pre-2014 coefficients to test parallel trends.

[12] The matching procedure is done without replacement, using a 0.05 caliper, and within broad workers' occupation cells (managers, mid-managers, white collars, blue collars).

4. Results

4.1. Baseline analysis

We plot the event-study coefficients resulting from the estimation of Equation 1 on the matched balanced sample of public managers described in Section 3.2. In each chart, we also report pooled DiD coefficients and standard errors summarizing the post-reform effects.

Employment outcomes. Figure 1 focuses on employment outcomes. Across the various event-study plots, coefficients in the pre-reform years are close to zero, validating the parallel trends assumption. Panel A presents the probability of staying at the 2014 firm, demonstrating that treated managers are more likely to leave their jobs following the introduction of the cap. The effect is modest (ranging from 2 to 5 percentage points) in 2015 and 2016 but grows progressively larger over time.[13] By 2020, managers affected by the cap are about 10 percentage points (roughly 14 percent of the post-2015 control mean, see Appendix Figure B1) more likely to have left their public sector firm than control managers. Panel B shows that this is not due to increased retirement probabilities, for which the estimated coefficient hovers around zero after 2014, but instead a stark increase of the probability that managers move to the private sector (Panel C). This effect amounts to about 10 percentage points in 2020, or five times the post-2015 control mean.[14] Finally, we find no evidence of effects on treated managers' labor supply at the intensive margin. This is

[13] One reason these effects were gradual is that the wage cap did not apply to existing fixed-term contracts, and managers earning above the cap could keep their current wage until contract expiration. As noted in Section 2.1, public-sector fixed-term contracts typically last up to three years.

[14] When looking at the overall probability of being employed, we notice a relatively small (5 percentage points) and insignificant effect (Appendix Figure B2). In the INPS data, workers that we classify as "employed" are public and private sector workers, while "non-employed" can be either retired, unemployed or self-employed. Given the null effect on retirement probabilities, the small negative effect on employment may be driven by few treated managers becoming unemployed or, more likely, self-employed.

reflected in the small and imprecisely estimated coefficients for the number of weeks worked, as shown in Panel D.

Earnings. Figure 2, Panel A, displays the event-study coefficients for (log) weekly wages. Managers affected by the cap experience a wage decline of 5–10 percent in the years following the reform. This wage reduction is primarily driven by treated managers who remain in the public sector, as shown by the red coefficients in Figure 2, Panel A. In contrast, treated managers who transition to the private sector, on average, see an increase in their wages, as indicated in Appendix Figure B3.[15] Combining wage and employment effects, we find that the policy led to an overall average reduction in labor earnings for treated managers (Figure 2 Panel B).

4.2. Robustness and other results

We now address potential concerns with our design. First, we examine the possibility of spillovers affecting the control group. Next, we test the robustness of our findings through placebo exercises, and alternative samples and specifications. All corresponding results are provided in Appendix B.

Spillover effects. Beyond the direct effects on managers earning above the cap, the imposition of a salary cap is also likely to influence the expected or potential wages of workers earning below the cap, potentially causing spillover effects on the control group. To estimate these spillover effects, we replace the baseline treatment group (managers earning above €240,000) with an alternative group not directly affected by the cap but likely to respond to the policy: managers earning between €200,000 and €240,000. This alternative

[15] Public managers moving to a private firm earn wages that are on average well above the 99th percentile of the wage distribution in the private sector. The 27 treated managers moving to the private sector are distributed across 23 distinct private firms.

treatment group is compared to the remainder of the original control group, comprising managers earning between €150,000 and €200,000 in 2014.

As in the baseline design, we perform one-to-one matching within age*gender cells to ensure comparability. This sample's balancing properties are reported in Table B4, Columns 3–4.

Appendix Figure B4 presents the event-study coefficients estimated from Equation 1 using this sample. The results indicate no significant effect on the probability of managers leaving their job compared to control managers, nor on their retirement outcomes or weeks worked. There is a small, statistically insignificant positive effect (2 percentage points after five years) on the probability of transitioning to the private sector. Importantly, this spillover effect on the control group aligns with the policy's direct effect on treated managers. This suggests that our baseline design (Figure 1) provides a conservative estimate, representing, if anything, a lower bound of the true average treatment effect on the treated (ATT).

Alternative matching strategies. Next, we present results using alternative sample choices to test the robustness of our baseline findings. Appendix Figures B5–B8 display the results, and Appendix Table B4 (Columns 5–12) reports the balancing statistics for the various samples described below. First, we varied the pool of potential control managers by including those earning either above €100,000 or above €200,000 in 2014 (rather than above €150,000) before implementing the matching. These adjustments result in different matched samples, over which we re-estimate Equation 1. The results remain qualitatively and quantitatively similar to the baseline findings, showing an increased likelihood of managers above the cap leaving their job and transitioning to the private sector. Second, we matched each treated manager to two control managers (instead of one) within each age*gender cell, effectively doubling the size of the control group. The re-estimation of Equation 1 using this expanded sample again yields results consistent with the baseline, reinforcing the observed effects. Third, we estimate Equation 1 on the unmatched sample,

comparing all public managers earning above €240,000 in 2014 to all public managers earning between €150,000 and €240,000 in 2014. This approach confirms the baseline results and additionally reveals a positive and significant effect on retirement probabilities: by 2020, treated managers were nearly 20 percentage points more likely to retire than control managers. This heightened retirement likelihood is likely explained by the higher mean age of the treatment group (as shown in Table 1, Columns 5–6). These robustness checks collectively support the validity of our baseline findings.

Placebo reform. As an additional robustness test, we replicate our main design but assume that the reform occurred in 2010 rather than in 2014. In this placebo exercise, we compare public managers earning more than €240,000 in 2010 to those earning between €150,000 and €240,000 in 2010. As in the main design, we perform a one-to-one matching within age*gender cells, with descriptive statistics for the matched sample reported in Table B4 (Columns 13–14). Appendix Figure B9 shows no significant differences in the career trajectories of public managers around the €240,000 threshold before and after 2010, reinforcing the validity of our findings.

Rest of public sector. As noted earlier, our analysis focuses on managers of state-owned companies, where we can reasonably assume that equivalent jobs exist in the private sector. In an additional exercise, we shift our focus to managers in the rest of the public sector, including public administration, health, education, and defense. Unfortunately, since we observe these workers only from 2014 onwards (as the related INPS dataset only became available starting in 2014), we cannot estimate pre-reform coefficients. As in the baseline analysis, we mimic the matching process within age*gender cells and report descriptive statistics for the matched sample in Table B4 (Columns 15–16). Appendix Figure B10 shows that treated public managers in these sectors are more likely to leave their job for the private sector in response to the cap, but the coefficients are small and not significant. This is

consistent with the idea that managers in public administration jobs have fewer outside options in the private sector than managers of public companies, and that wage caps likely have limited impacts on the careers of managers in more traditional public sector roles.

5. Productivity of Public Sector Employment

Our analysis has demonstrated that managers affected by the cap are more likely to leave public companies and transition to private sector jobs. This raises an important question: are the managers who move to the private sector in response to the wage cap positively selected? Additionally, are the replacements for these departing managers equally productive, or do they differ in terms of productivity?

5.1. Managers' quality

Measuring quality. To measure manager quality, we first estimate AKM regressions (Abowd et al., 1999) based on the population of Italian workers in the INPS database between 2005 and 2013 (the years before the reform). We then use the estimated worker fixed effects, standardized between zero and one, as proxies for worker productivity (Bombardini et al., 2019; Baltrunaite et al., 2020). Appendix C provides more detail.

Event-study results. We repeat the event study estimation separately for treated managers above and below the median AKM worker effect, comparing each group with all matched control managers. Figure 3 illustrates that the increased likelihood of leaving the public sector and transitioning to a private firm following the introduction of the cap is almost entirely driven by high-AKM managers (Figure 3, Panel A and Panel C). In contrast, the estimated treatment effects for low-AKM managers are small and not statistically significant.

5.2. Costs and Benefits

Costs. Based on our estimated AKM effects, we calculate that the departure of certain public managers in response to the cap resulted in a 10% standard deviation reduction in public management productivity relative to pre-reform levels.[16] We next assess whether the decline in productivity has been offset by leaving managers being replaced by equally (or more) productive counterparts. To evaluate this, we examine a leaving manager's cell—defined as the intersection of the manager's firm and municipality, serving as our closest proxy for a manager's team—and compute the mean AKM effect of any new manager(s) hired in that cell after the cap was introduced. We then compare the AKM of new hires to that of the leaving manager in each cell. Our findings indicate that leaving managers were not replaced by equally productive peers, confirming the overall decline in public management productivity resulting from the cap.

A key question is then whether the estimated reduction in the productivity of public management led to worse performance of public companies and to a deterioration in public services. These outcomes are notoriously hard to measure (Fenizia, 2022). We attempt to tackle this question by leveraging information on firm value added from balance sheet data (Section 2.2). Unfortunately, our not very large individual-level sample, together with the imperfect matching rate with the balance sheet data, deliver a firm sample that is too small to perform event-study analyses for firms as we did for workers.[17] We thus perform a simple "reduced-form" analysis that relates firm labor productivity (measured as value added per worker) to the share of managers earning above the €240,000 cap in 2014. This is motivated by our worker-level evidence, which suggests that public companies with higher share of

[16] This number is calculated by comparing the mean AKM fixed effect of all treated managers with the mean AKM fixed effect of treated managers who remain in the public sector after the introduction of the salary cap.
[17] Our 219 treated managers are employed at 59 distinct public companies. Of these firms, only 17 are matched with the Cerved balance sheet data, preventing us from running event-study estimations.

treated managers are more likely to lose managers to the private sector after 2014 (a "first stage"). We show these correlations over time in Appendix Figure B11. Before 2014, we document a positive relationship between firm productivity and the share of managers earning above the cap. In the years after the cap was introduced (2015-2018), the correlation lines tend to flatten, consistent with possible productivity losses in public companies with more treated managers. We refrain from making any quantification exercise based on this (quite suggestive) evidence.

Benefits. To estimate the policy's benefits, we perform simple back-of-the-envelope calculations. The mean annual wage reduction for treated public managers was €47,381. Multiplying this figure by the total number of treated managers in the broader public sector (657) yields annual public expenditure savings of approximately €31 million. Over the six years between 2015 and 2020, this results in total savings of around €186 million, which corresponds to 0.1% of the total costs of public employment (Corte dei Conti, 2020).

5.3. Co-worker effects

We have established that wage cap policies increase the likelihood that high-quality managers of public firms move to the private sector, with direct negative repercussions on public sector productivity. Our analysis has so far not considered that the careers of the co-workers of leaving managers may also be impacted. Ex-ante, these effects are ambiguous. Once their manager leaves, public employees might be more willing to retain their jobs if there are more chances of promotion. On the other hand, they may find themselves demotivated due to their manager's resignation and may leave the firm.[18]

[18] Public managers typically lack the incentive tools commonly used in the private sector, such as bonuses or performance pay. Given the absence of traditional motivation mechanisms, good public managers may be better at allocating tasks to workers or at finding alternative means to stimulate effort (Fenizia, 2022).

Figure 4 shows event-study results comparing co-workers of treated managers who leave their jobs to observationally similar co-workers of treated managers who stay in the public sector (see Section 3.2 for details). Among co-workers, we further distinguish between those in management and middle-management positions and other co-workers in blue- and white-collar jobs. We find that the co-workers of managers who leave are also about 10 percentage points more likely to leave their job and switch to the private sector, though coefficients are rather imprecisely estimated. Interestingly, roughly one fifth of treated managers moving to a private firm are "followed" by their co-workers in the ensuing years. We again find no effect on retirement probabilities. The higher propensity to leave is more pronounced among co-workers in management positions, for which we also document a (non-significant) reduction in weeks worked – perhaps as they had closer professional ties with the treated (leaving) manager and are more affected by their quit.

Interestingly, the effects we just described are larger among high-productivity co-workers in management positions (Figure 5), where we proxy productivity again using AKM worker fixed effects. In turn, this suggests that the negative impact of public wage caps on public sector productivity may go beyond the direct effects documented in Section 5.2 and extend to the (high-productivity) peers of managers who leave. We do not find such heterogenous patterns among co-workers in blue- and white-collar occupations, for which the estimated effects do not differ much between high- and low-AKM individuals (Appendix Figure B12).

6. Conclusion

Despite their wide diffusion, wage caps in the public sector have not been explored in economic research. This paper studies a reform implemented in Italy in 2014 that imposed a cap to the salaries of top public managers. Using a matched event study design, we investigate the impact of this policy on the careers of public managers, focusing in particular

on managers of state-owned firms. Managers affected by the cap are more likely to leave the public sector and move to a private job. These effects are driven by managers with higher productivity, which are on average not replaced by equally productive managers leading to an overall drop in the productivity of public management. We also observe larger propensity to quit public firms among the high-productivity co-workers of leaving managers, suggesting broader implications of salary caps on the overall quality of public employment.

We caution that our analysis has focused on managers of public companies operating in market-like industries, with likely outside options in the private sector. For managers in traditional public jobs (e.g., public administration, education, health, defence), our results suggest limited effects of salary caps. Still, state-owned companies account for a large share of public employment – about 7 percent in the case of Italy – and provide key services to citizens. Our study suggests that caps to public wages may have unintended negative consequences on the management of public companies – and, in turn, also on these companies' output and productivity, while leading to relatively small reductions in public spending. Future research and suitable data sources will better assess how wage cap policies may affect the provision of public services and citizens' welfare.

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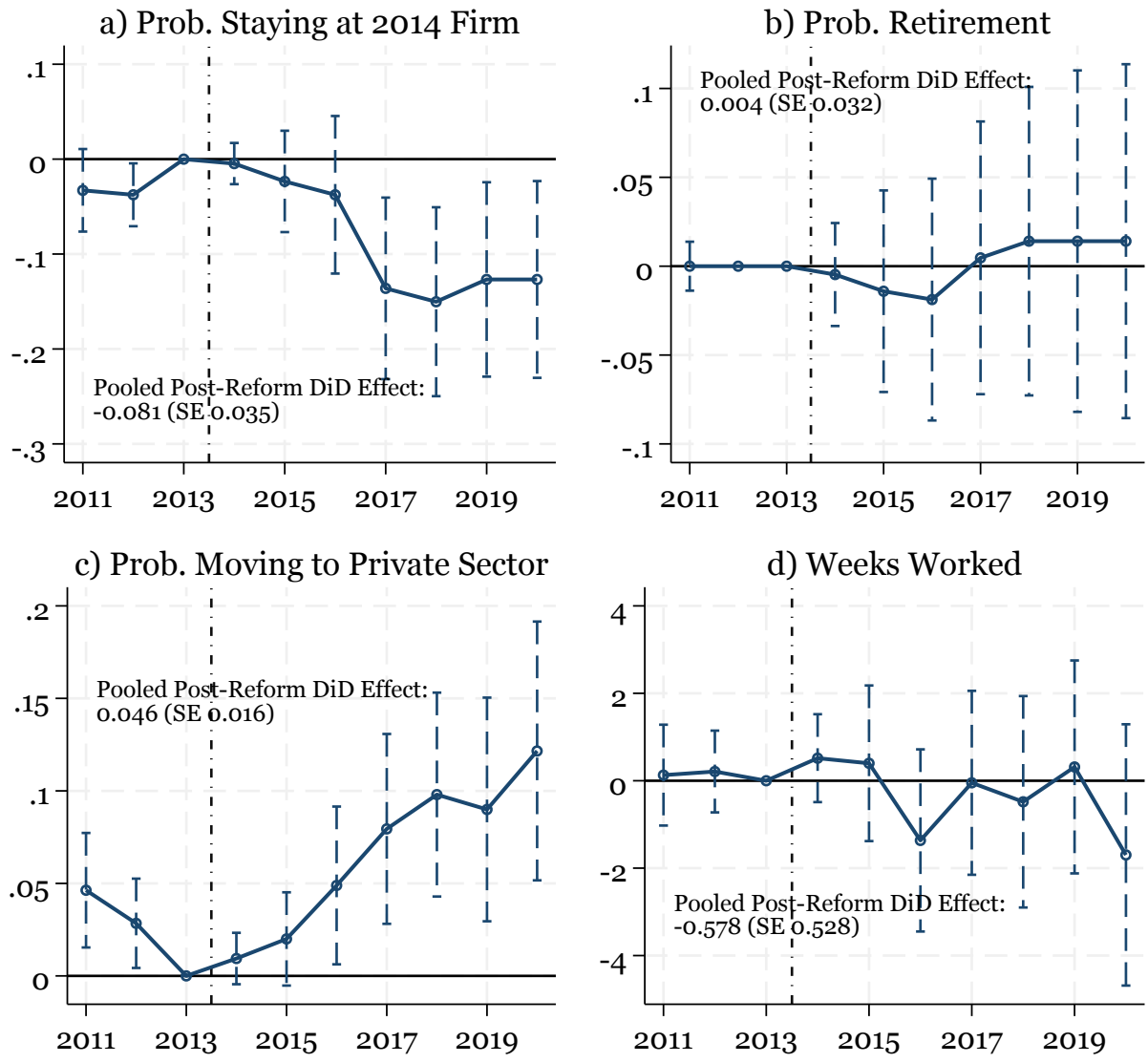
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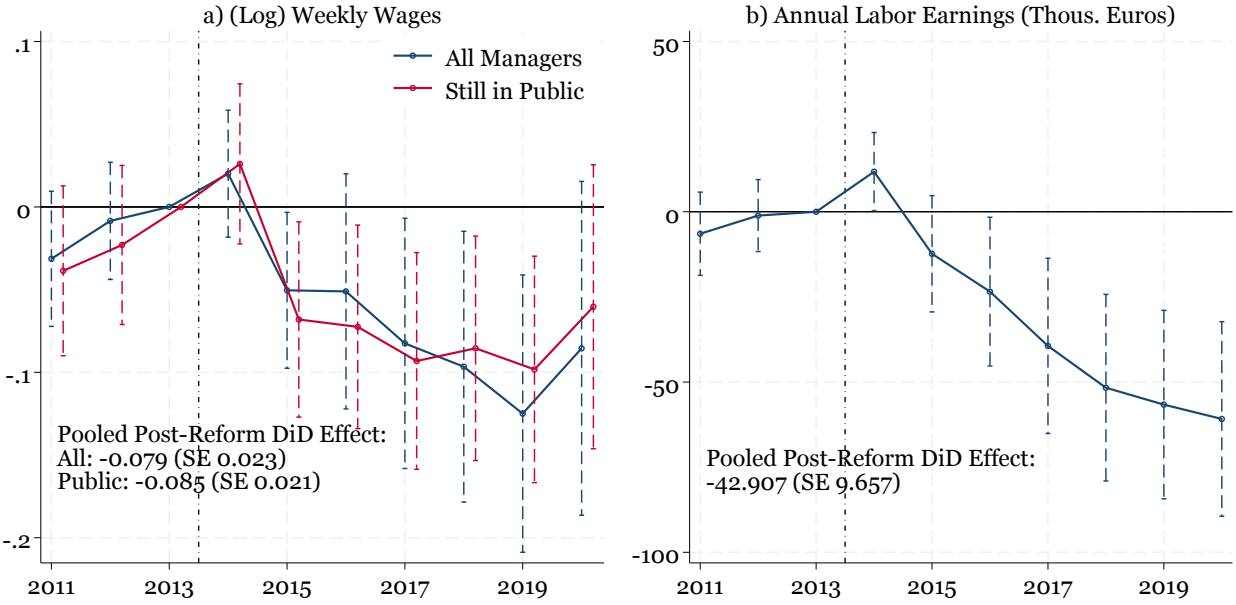
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Figure 1. Baseline event study results, employment outcomes



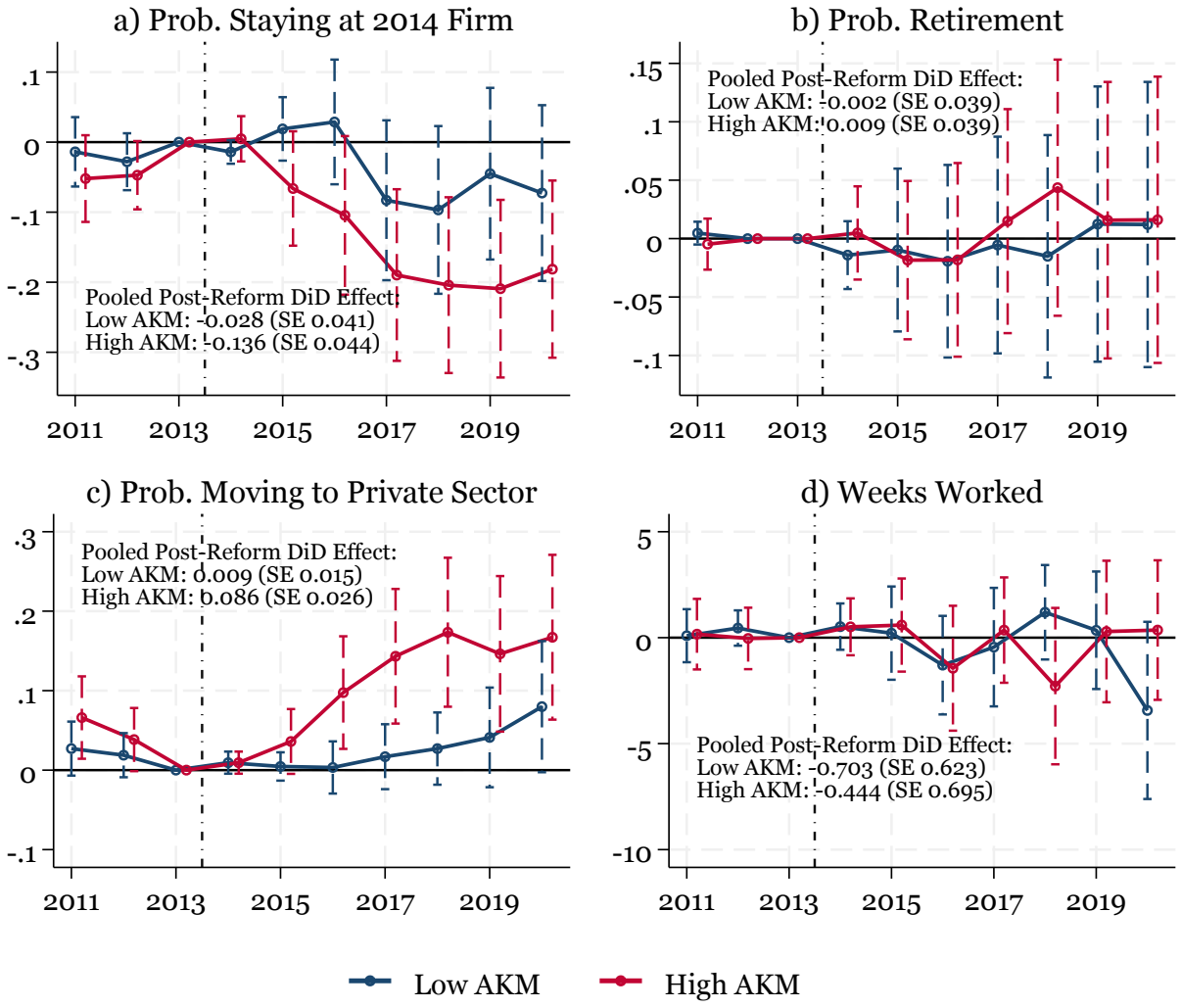
Notes: Event study coefficients resulting from the estimation of Equation 1 on the matched sample of top public managers described in Section 3.2. Panel a): The outcome is an indicator variable equal to one if the worker stays at the 2014 employer and zero otherwise. Panel b): The outcome is an indicator variable equal to one if the worker is retired in that year. Panel c): The outcome is an indicator variable equal to one if the worker is employed in the private sector, conditional on being employed. Panel d): The outcome is the number of weeks worked in that year, conditional on being employed. Standard errors clustered at the worker level. The dashed lines are 95% confidence intervals.

Figure 2. Baseline event study results, earnings



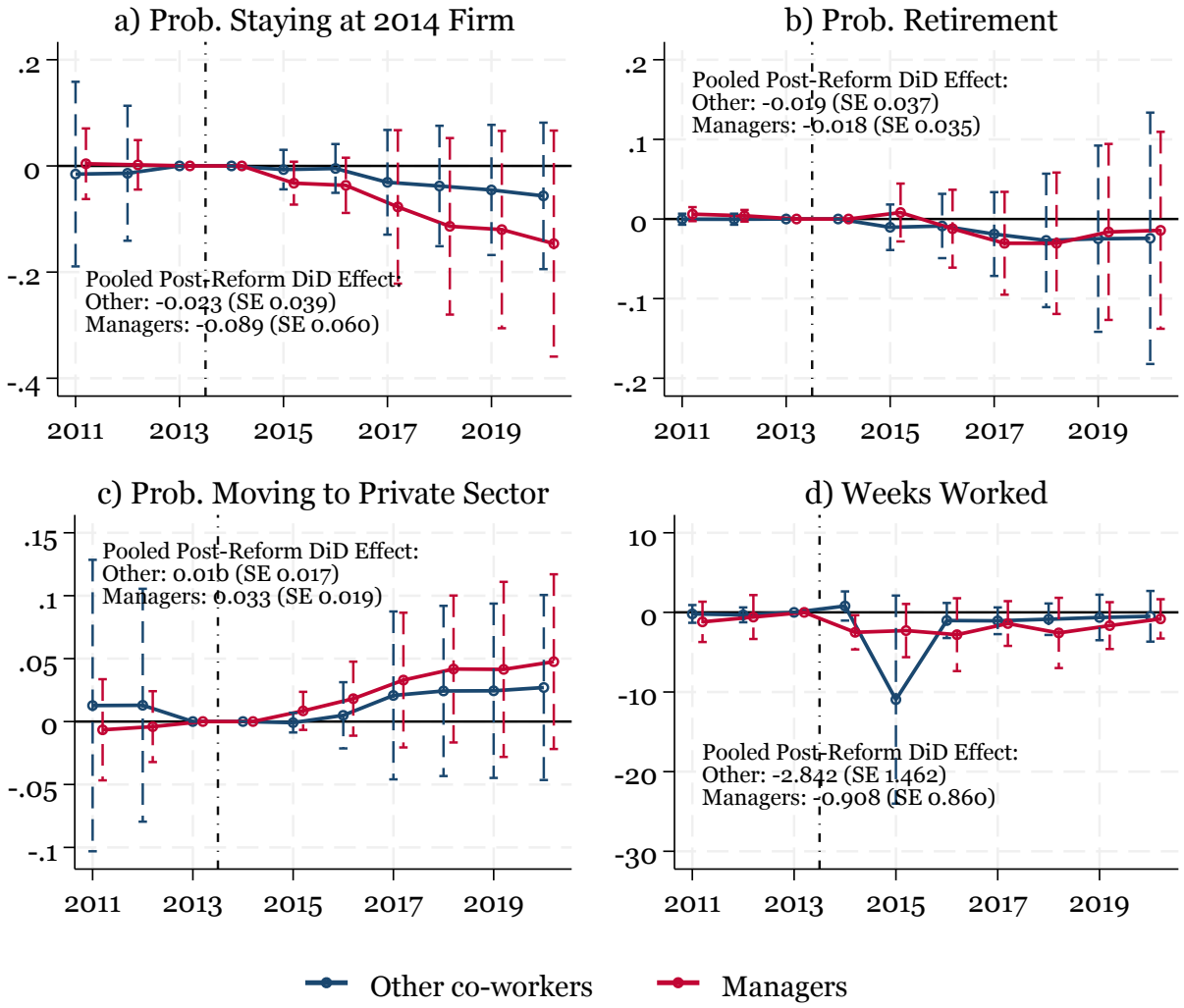
Notes: Event study coefficients resulting from the estimation of Equation 1 on the matched sample of top public managers described in Section 3.2. Panel a): The outcome is the (log) weekly wage of a worker in that year. Panel b): The outcome is total labor earnings (in thousand euros, 2010 prices) of a worker in that year and is equal to zero if the worker is not employed. For these outcomes, the sample excludes six treated managers with a large wage jump between 2013 and 2014 (wage below €150,000 in 2013 and above the cap in 2014). The baseline results in Figure 1 are identical when these six managers are excluded. Standard errors clustered at the worker level. The dashed lines are 95% confidence intervals.

Figure 3. Event study results, high vs. low AKM effect



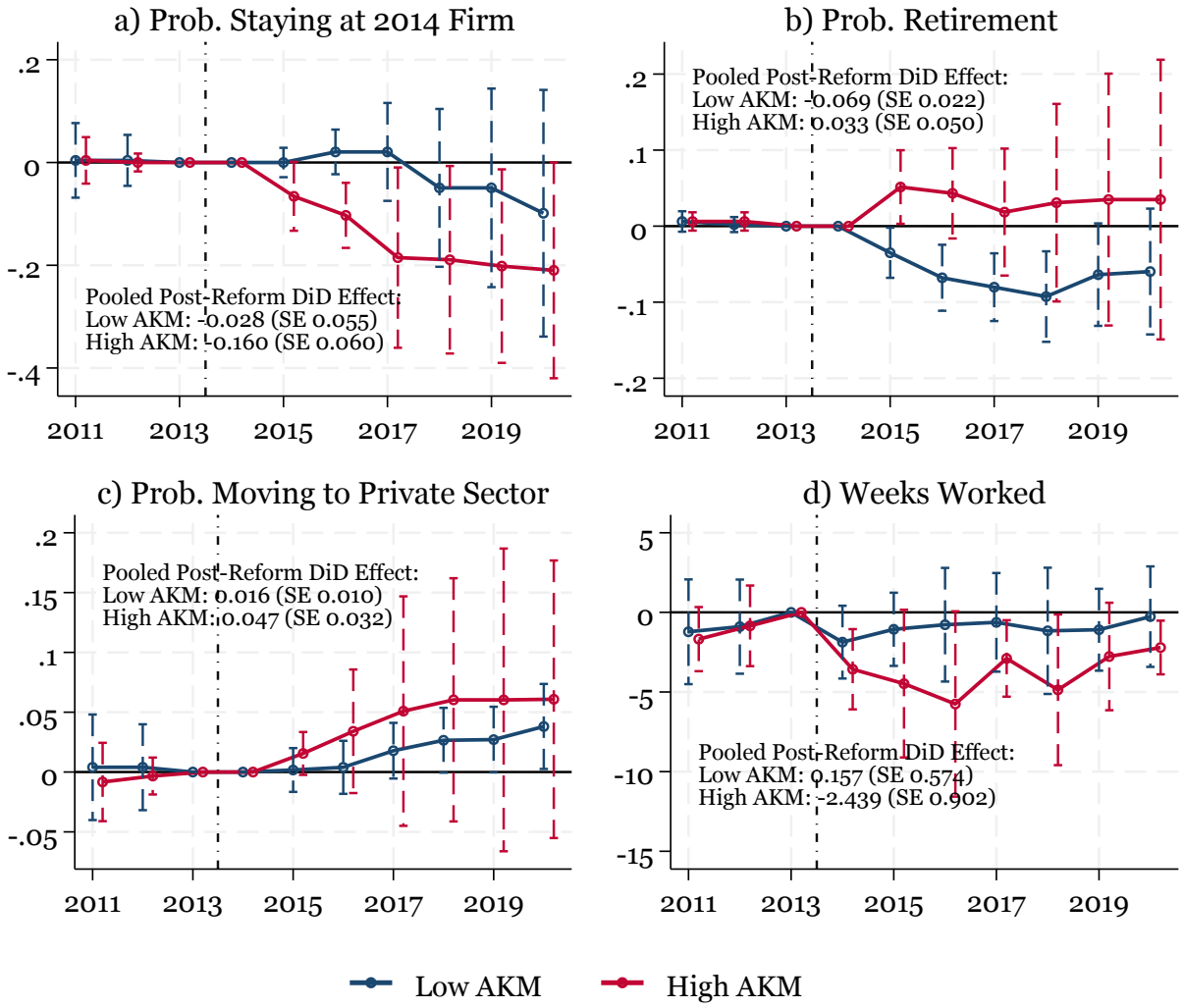
Notes: Event study coefficients resulting from the estimation of Equation 1 on the matched sample of top public managers described in Section 3.2, separately for treated managers with above-median and below-median AKM worker fixed effect. Standard errors clustered at the worker level. The dashed lines are 95% confidence intervals. See Figure 1 and Section 5.1 for details.

Figure 4. Event study results, co-workers



Notes: Event study coefficients resulting from the estimation of Equation 2 on the matched sample of the co-workers of treated managers who leave the public sector after the cap versus the co-workers of treated managers who stay, separately for managers and mid-managers (in red) and other co-workers (white-collars and blue-collars, in blue). Standard errors clustered at the firm*municipality level. The dashed lines are 95% confidence intervals. See Figure 1 and Section 5.2 for details.

Figure 5. Event study results, high vs. low AKM effect, co-workers (managers)



Notes: Event study coefficients resulting from the estimation of Equation 2 on the matched sample of the co-workers of treated managers who leave the public sector after the cap versus the co-workers of treated managers who stay, only for managers and mid-managers, and separately for co-workers with above-median and below-median AKM worker fixed effect. Standard errors clustered at the firm*municipality level. The dashed lines are 95% confidence intervals. See Figure 1 and Section 5.2 for details.

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Table 1. Descriptive statistics for public sector workers in 2014

	All public			Public-in-private		
	(1)	(2)	(3)	(4)	(5)	(6)
	Total	Wage $\in [150k, 240k)$	Wage $\geq 240k$	Total	Wage $\in [150k, 240k)$	Wage $\geq 240k$
Annual wage	31,634	179,543	287,381	30,944	180,602	293,292
	(19,591)	(22,635)	(80,178)	(27,329)	(23,263)	(87,603)
Female	0.57	0.28	0.17	0.29	0.22	0.09
	(.50)	(.45)	(.37)	(.45)	(.41)	(.29)
Age	49.09	57.47	59.02	45.68	54.57	57.48
	(9.50)	(6.57)	(6.36)	(10.67)	(5.90)	(5.60)
Temporary	0.05	0.03	0.03	0.12	0.03	0.07
	(.21)	(.16)	(.18)	(.33)	(.18)	(.25)
Part-time	0.05	0.00	0.01	0.06	0.00	0.00
	(.22)	(.05)	(.09)	(.24)	(.04)	(0)
Private job pre-2014	0.04	0.01	0.03	0.09	0.03	0.05
	(.19)	(.11)	(.17)	(.28)	(.16)	(.23)
Working in Rome	0.15	0.36	0.52	0.18	0.60	0.74
	(.35)	(.48)	(.50)	(.38)	(.49)	(.44)
Experience	25.62	31.02	32.71	22.59	29.07	32.14
	(10.63)	(9.00)	(9.48)	(12.36)	(8.48)	(9.02)
Observations	3,159,828	7,333	657	240,907	1,389	219

Notes: Descriptive statistics for public sector workers in 2014. Columns (1)-(3) refer to the whole public sector, Columns (4)-(6) refer to public-in-private workers as described in Section 2.2. Columns (1) and (4) show statistics for all workers, Columns (2) and (5) for workers earning between 150,000 and 240,000 euros in 2014 and Columns (3) and (6) for workers earning above the 240,000 euros cap in 2014. "Annual wage" is measured in euros. "Female", "Temporary", "Part-time", "Private job pre-2014" and "Working in Rome" are all indicator variables. "Age" and "Experience" are measured in years. Standard deviations in parentheses.

Table 2. Employment status for top public managers after 2014

	Wage $\in [150k, 240k)$ (1)	Wage $\geq 240k$ (2)
Stay in public	881	87
	0.63	*0.40*
Retirement	336	91
	0.24	*0.41*
Non-employment	106	14
	0.08	*0.06*
Move to private	66	27
	0.05	*0.13*
Total	1,389	219

Notes: Employment status for public-in-private top managers after 2014, separately for workers earning between 150,000 and 240,000 euros in 2014 (Column (1)) and workers earning above 240,000 euros in 2014 (Column (2)). "Stay in public" denotes managers who remain in the public sector between 2014 and 2020. The other indicators denote whether a manager retired ("Retirement"), is not observed anymore in the INPS data ("Non-employment", which can denote either unemployment or self-employment) or moved to a private job ("Move to private"). Shares are denoted in italics.

Appendix

A. Dataset

This Appendix describes how we build our dataset. As mentioned in the text, we leverage the archives of the Italian Social Security Institute (INPS) for both the public and private sectors. If a worker is employed in more than one job in a given year, we assign each worker to their "dominant" job, defined as the one with the highest annual earnings (or weeks worked, if earnings are the same across jobs).

To construct our main "public-in-private" dataset, we draw from matched employer-employee data for the Italian private sector ("Uniemens" data). These archives include a group of workers employed in public companies and state subsidiaries that, for reasons related to social security contributions, feature in the private firms' data. To identify public employees in the Uniemens data, we consider the following information: i) a worker's collective bargaining agreement; ii) contribution type, which classifies special worker categories based on their social security contributions; iii) a social security code assigned by INPS ("codice statistico contributivo"); and iv) authorization code, which integrates the previous code and is specific to certain employers. Each of these variables takes a specific value to denote workers in public companies, which we use to identify public-in-private workers. To these, we add workers employed in companies within the public administration, collected in the so-called "S13 List" available at the Italian National Institute of Statistics (Istat).[19]

This selection may still include workers employed in public companies listed on the stock market, which were exempt from the salary cap. We compile a list of such companies

[19] See https://www.istat.it/it/archivio/190748.

drawing from Cassa Depositi e Prestiti archives and merge it into the INPS data.[20] This allows us to obtain a final list of public (in private) employees subject to the cap from the private sector archives, amounting to roughly 240,000 workers in 2014. Of these, 219 earned above the cap in 2014 and will constitute the main treated group.

Our dataset also includes employees in the broader Italian public sector, including roughly 3 million workers. These matched employer-employee data are also available at INPS ("PosPa" data) and refer to workers in central and local administration, health, defense, and education (schools and universities). As described in the main text, the PosPa data only started in 2014 and is hence not best suited for our empirical design estimating difference-in-differences around the cap's implementation.

[20] See https://www.cdp.it/sitointernet/it/cdp_equity_portafoglio.page.

B. Additional Analysis and Robustness Results

Table B1. Within-industry descriptive statistics, public vs. private top managers

	Private (1)	Public (2)
Annual wage	374,128	294,020
	(50,513)	(23,498)
Female	0.09	0.10
	(.05)	(.03)
Age	52.05	57.46
	(2.68)	(2.15)
Temporary	0.02	0.07
	(.01)	(.08)
Part-time	0.00	0.00
	(.01)	(.00)
Rome	0.28	0.80
	(.32)	(.11)
Experience	26.65	31.33
	(2.82)	(3.22)

Notes: Descriptive statistics for managers earning more than 240,000 euros in 2014. Column (1) refers to private sector managers, Column (2) to public-in-private managers. Each statistic is computed within industry, then averaged across industries with weights based on industry employment distributions in the public sector. See Table 1 and text for details.

Table B2. Sector distribution of public-in-private workers in 2014

	Total (1)	Wage $\in [150k, 240k)$ (2)	Wage $\geq 240k$ (3)
Agriculture, Forestry and Fishing	0.03	0.01	0.02
Mining	0.00	0.00	0.00
Manufacturing	0.00	0.00	0.01
Electricity, Gas, Steam and Air Conditioning Supply	0.00	0.02	0.02
Water Supply; Sewerage, Waste Management	0.01	0.00	0.00
Construction	0.01	0.01	0.01
Wholesale and Retail Trade; Repair of Motor Vehicles	0.00	0.00	0.00
Transportation and Storage	0.01	0.09	0.16
Accommodation and Food Service Activities	0.00	0.00	0.00
Information and Communication	0.00	0.08	0.14
Financial and Insurance Activities	0.31	0.39	0.33
Real Estate Activities	0.00	0.00	0.00
Professional, Scientific and Technical Activities	0.00	0.03	0.03
Administrative and Support Service Activities	0.02	0.06	0.05
Public Administration and Defence	0.04	0.24	0.09
Education	0.00	0.00	0.00
Human Health and Social Work Activities	0.02	0.01	0.01
Arts, Entertainment and Recreation	0.07	0.02	0.02
Other Service Activities	0.32	0.05	0.10
Activities of Households as Employers	0.00	0.00	0.00
Activities of Extraterritorial Organisations and Bodies	0.00	0.00	0.00

Notes: Sector shares of public-in-private workers in 2014, separately for all workers (Column (1)), workers earning between 150,000 and 240,000 euros in 2014 (Column (2)) and workers earning above 240,000 euros in 2014 (Column (3)).

Table B3. Sector distribution of other public employees in 2014

	Total (1)	Wage $\in [150k, 240k)$ (2)	Wage $\geq 240k$ (3)
Education	0.32	0.00	0.00
Health	0.22	0.10	0.10
Central administrations	0.07	0.73	0.72
Universities	0.04	0.05	0.06
Local administrations	0.21	0.07	0.03
Defense	0.10	0.01	0.02
Other	0.04	0.03	0.06

Notes: Sector shares of workers in the public sector in 2014, separately for all workers (Column (1)), workers earning between 150,000 and 240,000 euros in 2014 (Column (2)) and workers earning above 240,000 euros in 2014 (Column (3)).

Table B4. Balancing of matched samples, 2013

	Baseline		Spillovers		Controls $\geq 100k$		Controls $\geq 200k$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	C	T	C	T	C	T	C	T
Annual wage	178,175	273,189	165,492	217,267	133,614	273,331	208,027	272,349
	(42,703)	(93,391)	(26,695)	(184,957)	(35,539)	(93,948)	(43,854)	(96,400)
Female	0.09	0.09	0.18	0.18	0.09	0.09	0.07	0.07
	(.29)	(.29)	(.38)	(.38)	(.29)	(.29)	(.26)	(.26)
Age	56.43	56.43	54.55	54.55	56.43	56.43	56.07	56.07
	(5.28)	(5.28)	(5.31)	(5.31)	(5.55)	(5.55)	(5.00)	(5.00)
Temporary	0.04	0.06	0.02	0.04	0.03	0.07	0.04	0.07
	(.19)	(.25)	(.13)	(.19)	(.16)	(.25)	(.20)	(.25)
Part-time	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)
Rome	0.64	0.74	0.63	0.57	0.57	0.74	0.57	0.75
	(.48)	(.44)	(.48)	(.49)	(.50)	(.44)	(.50)	(.44)
Experience	31.66	31.19	29.63	29.17	31.52	31.17	30.76	30.50
	(7.50)	(8.55)	(7.85)	(7.47)	(8.01)	(8.73)	(7.28)	(8.43)
Observations	213	213	311	311	216	216	189	189

Continues next table.

Table B4. Balancing of matched samples, 2013 (continued)

	1 : 2 Matching		No Matching		2010 Placebo		Other Public	
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	C	T	C	T	C	T	C	T
Annual wage	179,275	272,360	177,006	273,298	173,281	260,927	179,984	284,426
	(39,083)	(92,823)	(93,918)	(93,732)	(44,780)	(106,094)	(23,985)	(76,127)
Female	0.09	0.09	0.22	0.09	0.11	0.11	0.20	0.20
	(.29)	(.29)	(.41)	(.29)	(.31)	(.31)	(.40)	(.40)
Age	56.38	56.38	53.57	56.48	55.24	55.24	59.78	59.78
	(5.24)	(5.24)	(5.90)	(5.60)	(6.50)	(6.50)	(6.58)	(6.58)
Temporary	0.04	0.07	0.03	0.07	0.03	0.04	0.02	0.01
	(.19)	(.25)	(.17)	(.26)	(.18)	(.20)	(.13)	(.12)
Part-time	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01
	(.05)	(.00)	(.05)	(.00)	(.00)	(.00)	(.05)	(.11)
Rome	0.62	0.74	0.60	0.73	0.57	0.67	0.31	0.42
	(.48)	(.44)	(.49)	(.44)	(.50)	(.47)	(.46)	(.49)
Experience	31.45	31.13	28.07	31.14	30.44	30.16	31.45	33.14
	(7.57)	(8.52)	(8.48)	(9.02)	(8.74)	(8.81)	(10.87)	(10.34)
Observations	212	424	219	1,389	249	249	440	440

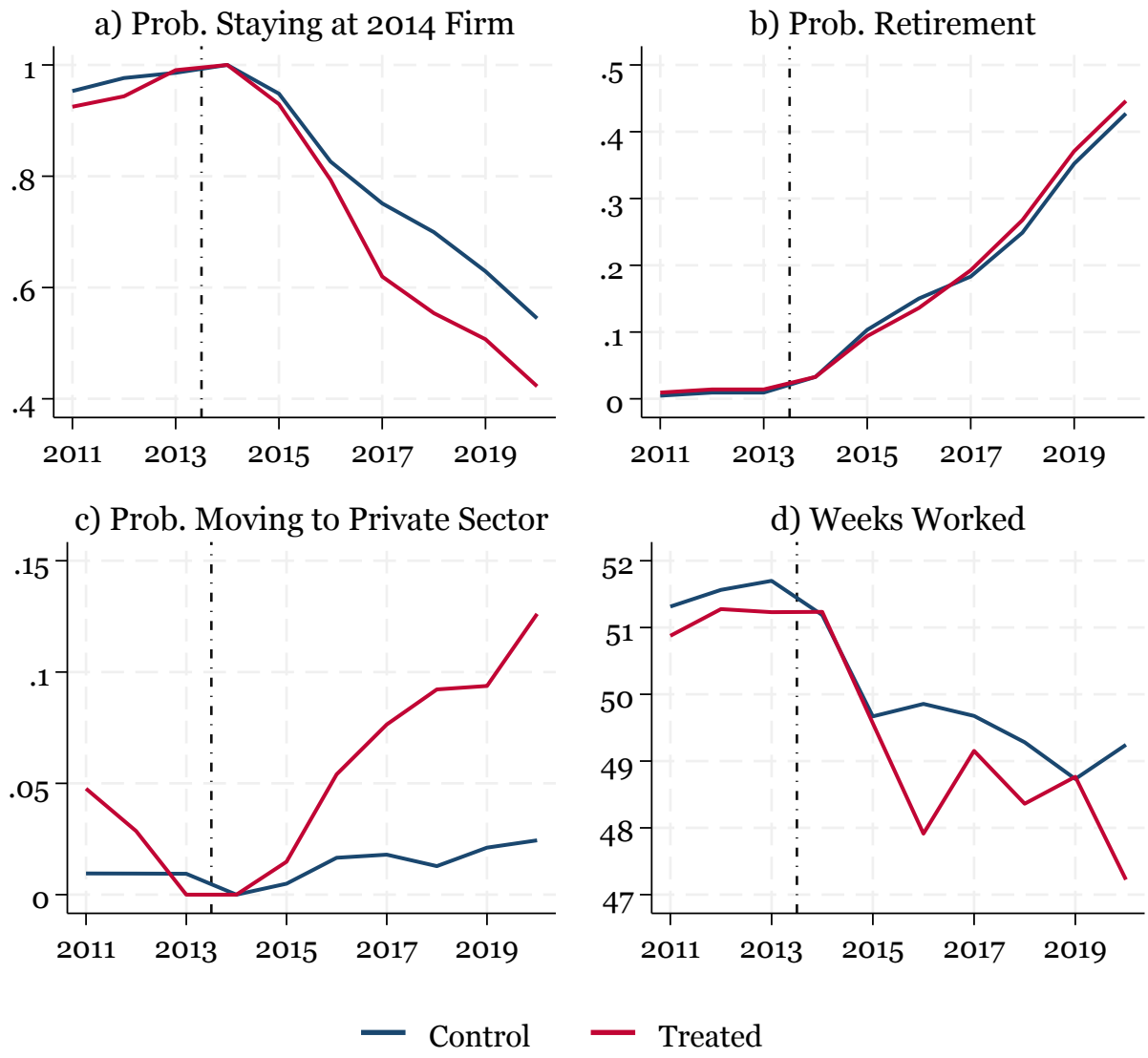
Notes: Balancing properties of matched samples separately for the control (C) and treated (T) groups, statistics as of 2013. Columns (1)-(2) refer to the baseline analysis where each public manager earning above 240,000 euros in 2014 is matched with a public manager earning between 150,000 and 240,000 euros in 2014 using a one-to-one matching within age*gender cells. Columns (3)-(4) refer to the matched sample of the spillover analysis, where each public manager earning between 200,000 and 240,000 euros in 2014 is matched with a public manager earning between 150,000 and 200,000 euros in 2014. Columns (5)-(6) and (7)-(8) replace potential controls in the baseline design (public managers earning between 150,000 and 240,000 euros in 2014) with public managers earning between 100,000 and 240,000 euros in 2014 or with public managers earning between 200,000 and 240,000 euros in 2014, respectively. Columns (9)-(10) replicate the baseline matching but with a one-to-two (rather than one-to-one) matching. Columns (11)-(12) show descriptive statistics when no matching is performed and all public managers earning above 240,000 euros in 2014 are compared with public managers earning between 150,000 and 240,000 euros in 2014. Columns (13)-(14) refer to the matched sample for the placebo reform exercise, where each public manager earning above 240,000 euros in 2010 is matched with a public manager earning between 150,000 and 240,000 euros in 2010 using a one-to-one matching within age*gender cells. Columns (15)-(16) refer to the rest of public sector workers (not those in the public-in-private group), where again each public manager earning above 240,000 euros in 2014 is matched with a public manager earning between 150,000 and 240,000 euros in 2014 using a one-to-one matching within age*gender cells. Standard deviations in parentheses. See Table 1 and text for details.

Table B5. Balancing of matched sample of co-workers, 2013

	Managers		Other co-workers	
	(1)	(2)	(3)	(4)
	C	T	C	T
Annual wage	94,945	92,832	36,370	34,902
	(53,447)	(50,809)	(13,653)	(9,020)
Female	0.39	0.39	0.66	0.64
	(.49)	(.49)	(.47)	(.48)
Age	50.75	50.50	48.27	48.11
	(7.29)	(7.54)	(8.62)	(8.13)
Temporary	0.02	0.02	0.05	0.03
	(.15)	(.14)	(.23)	(.16)
Part-time	0.01	0.01	0.11	0.07
	(.10)	(.12)	(.31)	(.25)
Rome	0.64	0.80	0.53	0.87
	(.48)	(.40)	(.50)	(.34)
Experience	24.62	24.93	25.13	25.87
	(9.64)	(10.08)	(11.23)	(9.83)
Observations	492	492	1,877	1,877

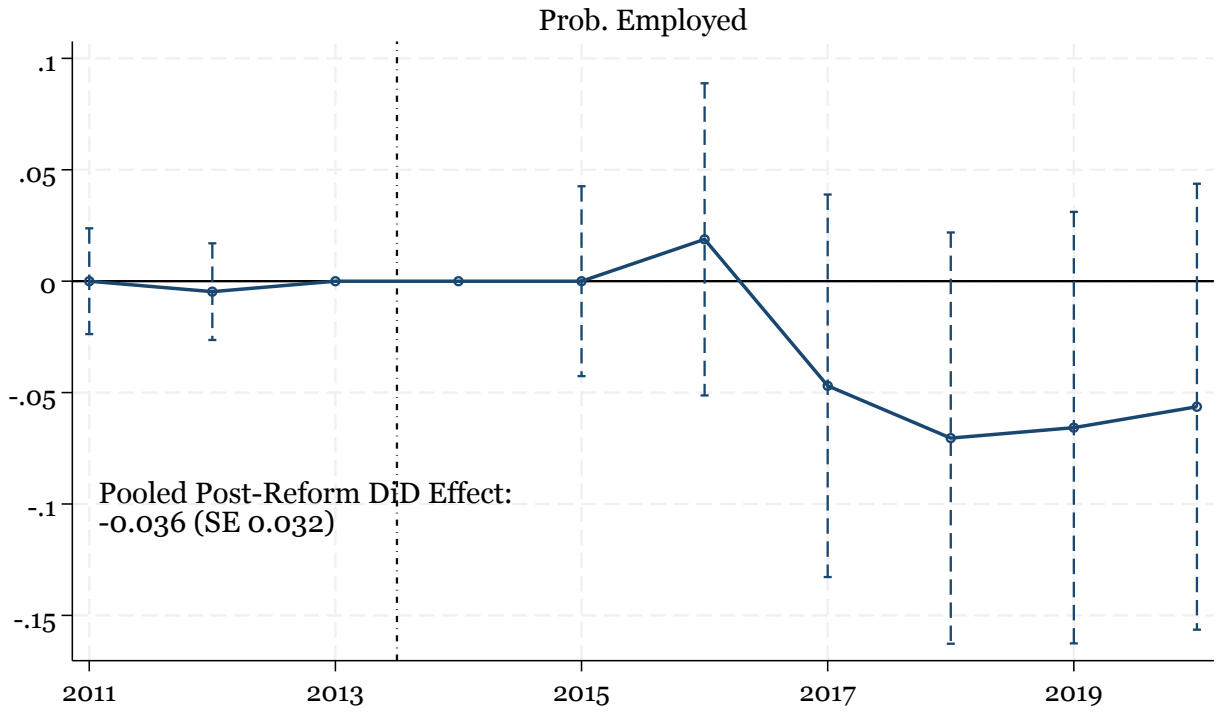
Notes: Balancing properties of the matched sample of co-workers of treated public managers who stay at their firm after the cap (Columns (1)-(3)) and of treated managers who leave their firm and move to the private sector after the cap (Columns (2)-(4)), statistics as of 2013. Co-workers are defined as other managers and mid-managers (Columns (1)-(2)) or blue- and white-collars (Columns (3)-(4)) earning below the cap in 2014 in the same firm*municipality cell as treated managers. The matched sample is obtained using a one-to-one propensity score matching within occupations, based on age, gender, tenure and wage in 2014. See Table 1 and text for details.

Figure B1. Baseline event study results, employment outcomes, raw means



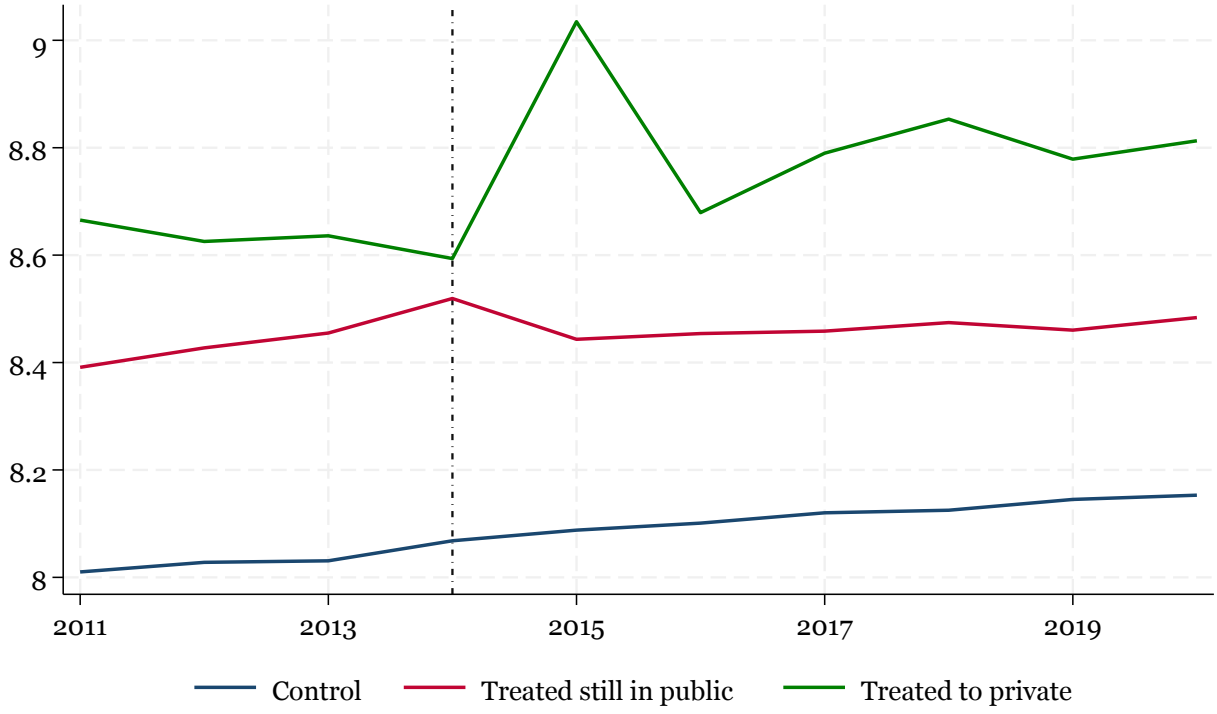
Notes: Raw means for the matched sample of top public managers described in Section 3.2. See Figure 1 and text for details.

Figure B2. Baseline event study results, employment



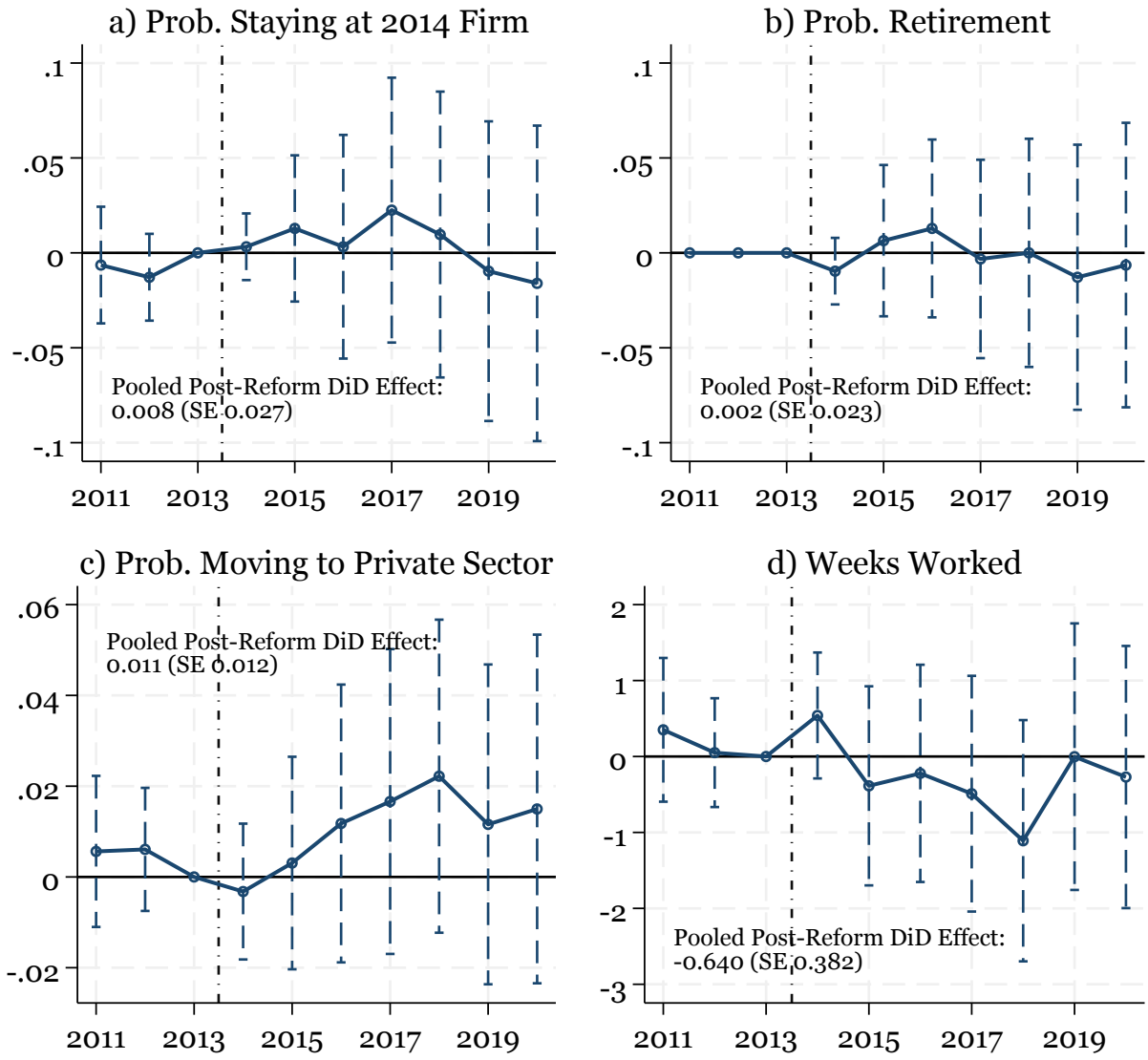
Notes: Event study coefficients resulting from the estimation of Equation 1 on the matched sample of top public managers described in Section 3.2. The outcome is an indicator variable equal to one if the worker is employed at any firm and zero otherwise. Standard errors clustered at the worker level. The dashed lines are 95% confidence intervals.

Figure B3. Baseline results, (log) weekly wage, raw means



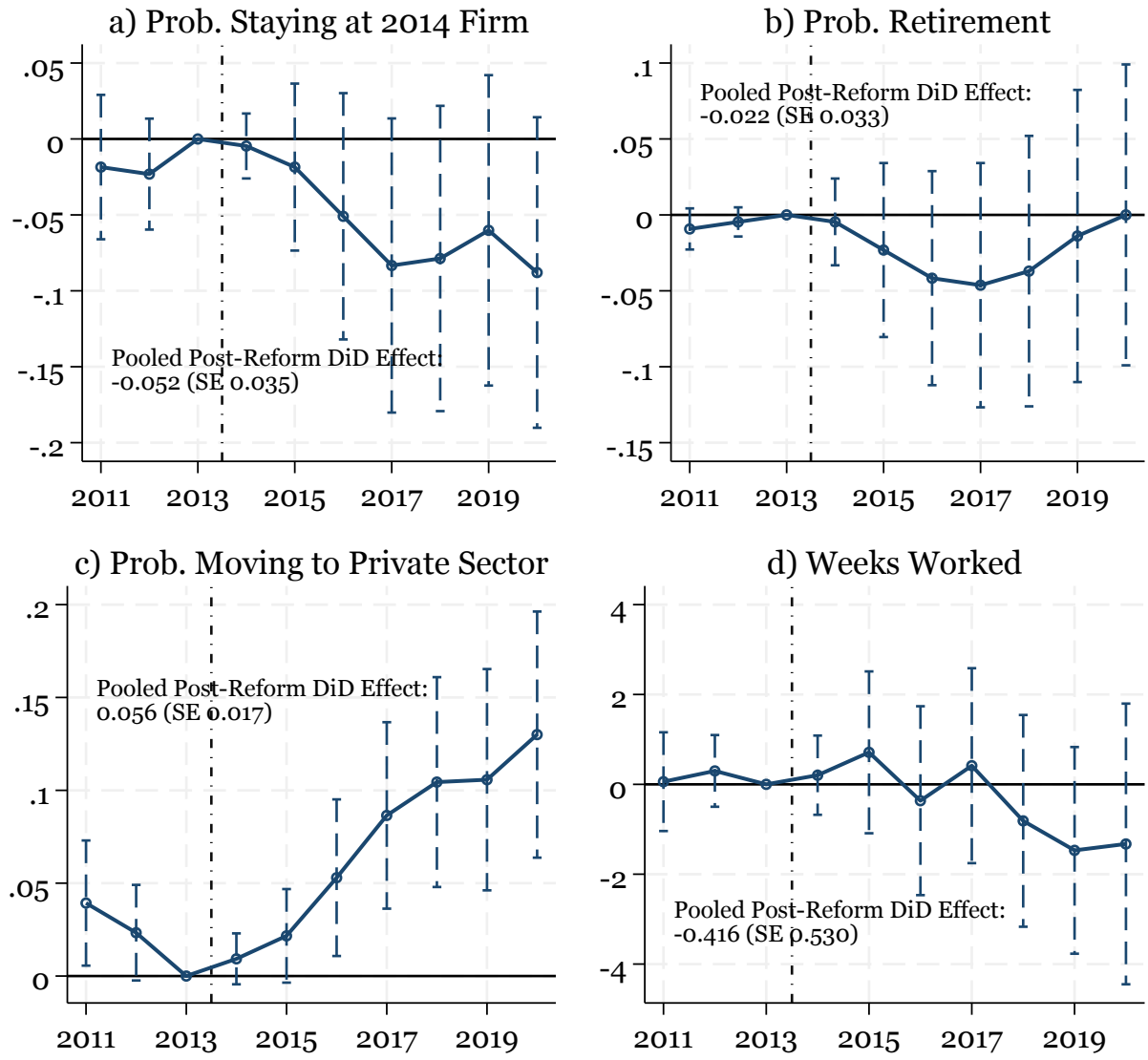
Notes: Raw means for the matched sample of top public managers described in Section 3.2. The outcome is the (log) weekly wage of a worker in that year. The Figure conditions on managers working 52 weeks in a year. See Figure 2 and text for details.

Figure B4. Event study results, spillover effects



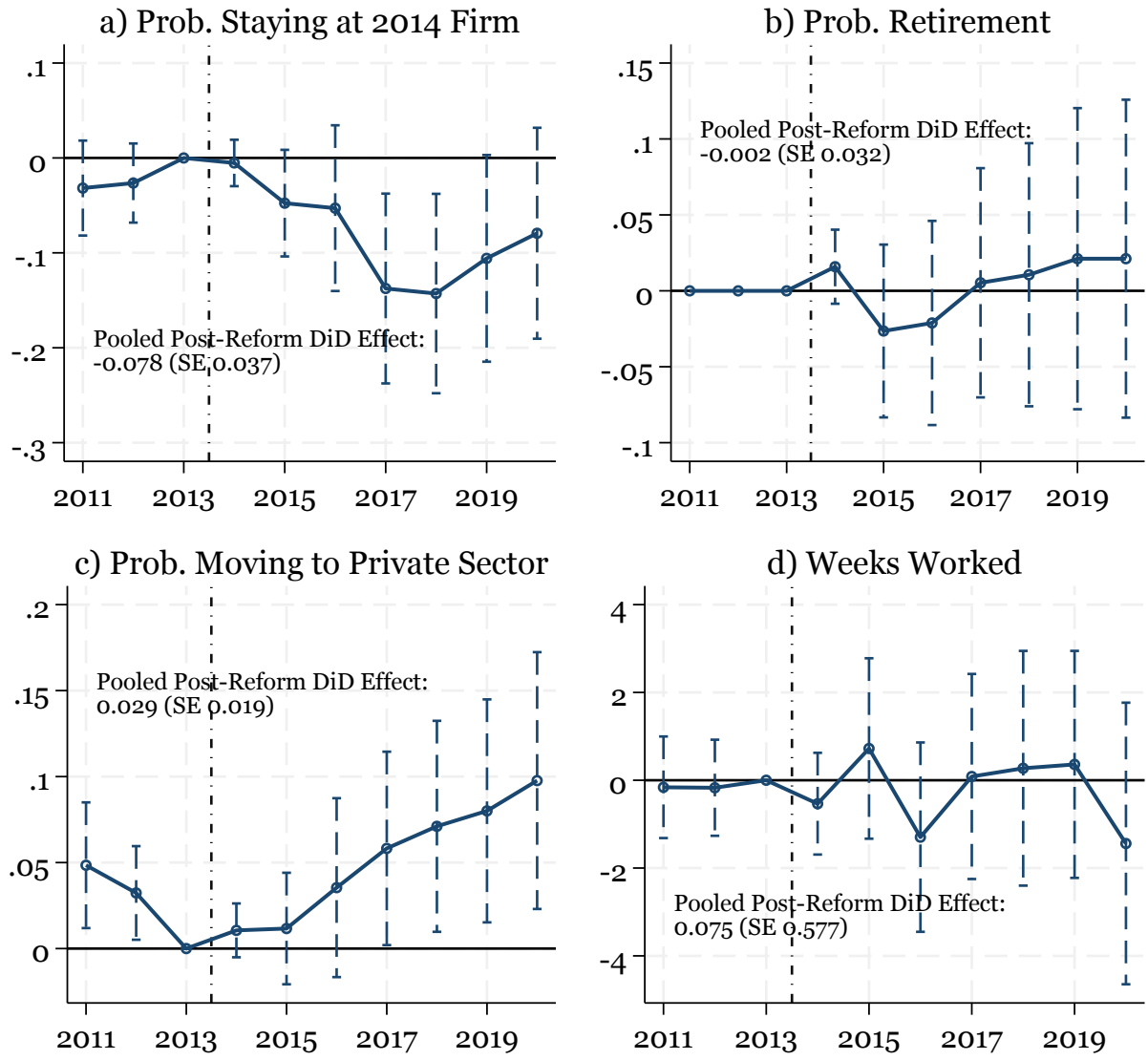
Notes: Event study coefficients resulting from the estimation of Equation 1 on a matched sample of public managers earning above versus below €200,000 in 2014. Standard errors clustered at the worker level. The dashed lines are 95% confidence intervals. See Section 4.2 and Figure 1 for details.

Figure B5. Event study results, controls above €100,000



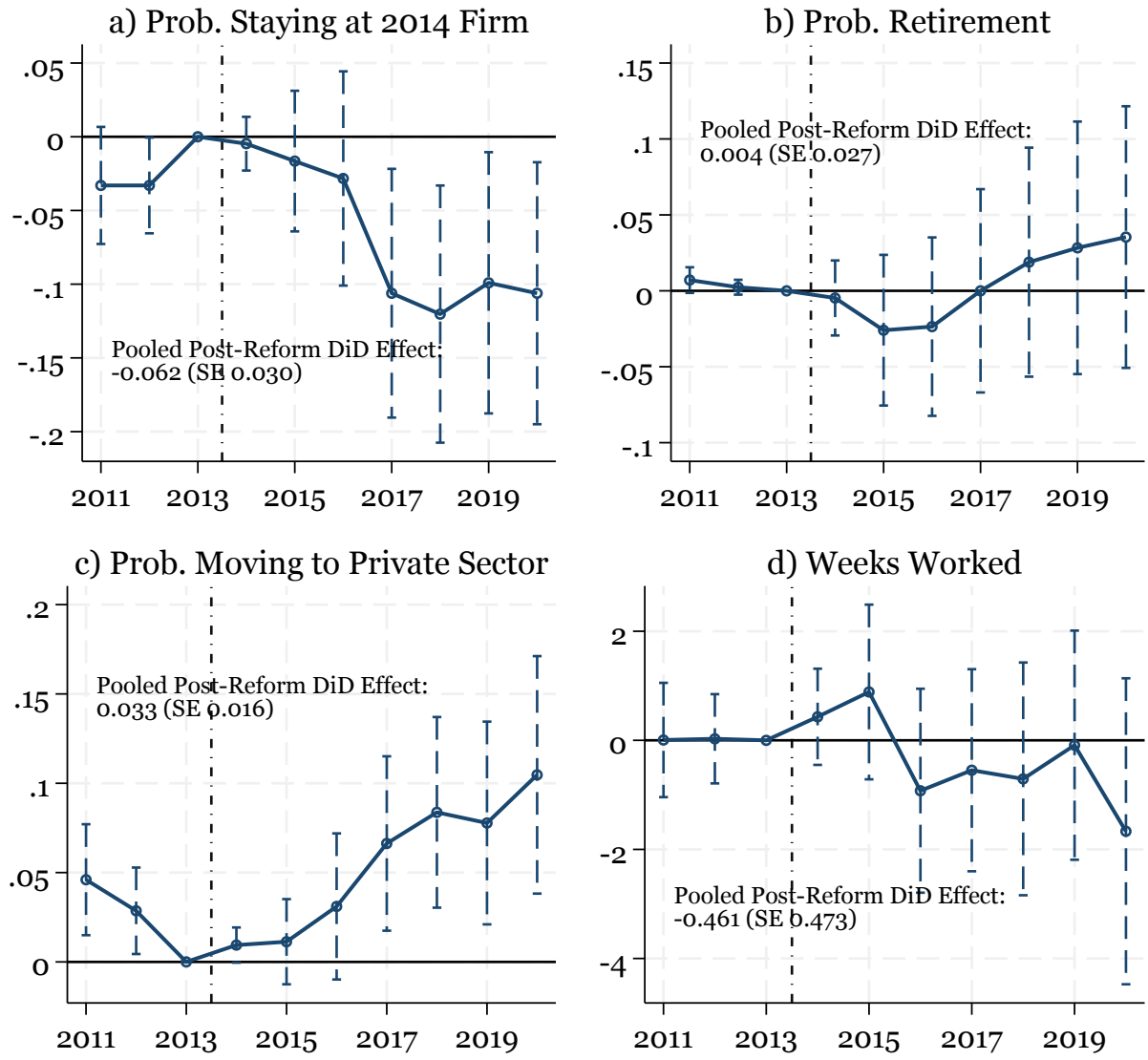
Notes: Event study coefficients resulting from the estimation of Equation 1 on a matched sample of public managers earning above versus below €240,000 in 2014. Before the matching, we select potential controls among public managers earning more than €100,000 in 2014 rather than €150,000 as in the baseline design. Standard errors clustered at the worker level. The dashed lines are 95% confidence intervals. See Section 4.2 and Figure 1 for details.

Figure B6. Event study results, controls above €200,000



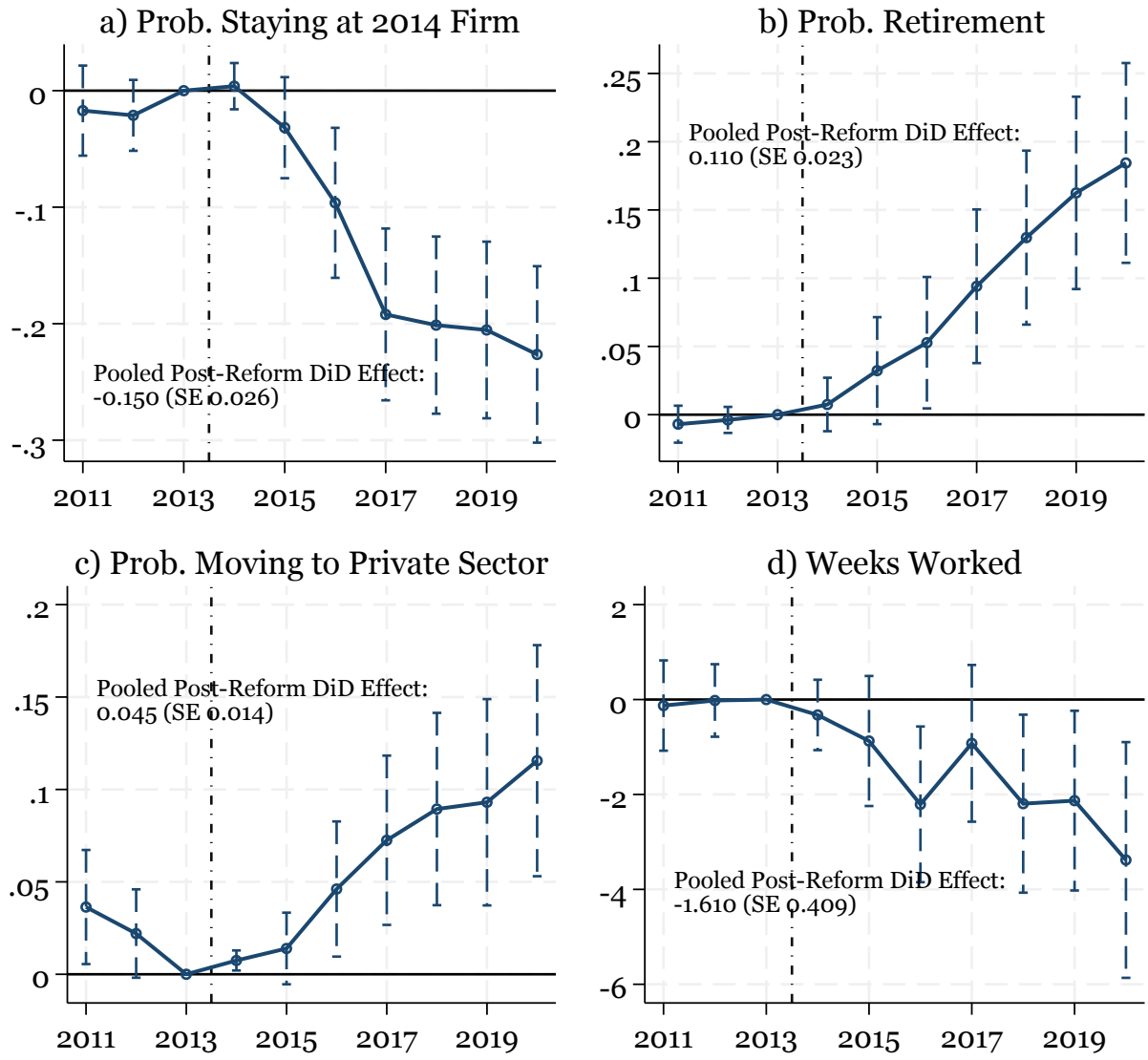
Notes: Event study coefficients resulting from the estimation of Equation 1 on a matched sample of public managers earning above versus below €240,000 in 2014. Before the matching, we select potential controls among public managers earning more than €200,000 in 2014 rather than €150,000 as in the baseline design. Standard errors clustered at the worker level. The dashed lines are 95% confidence intervals. See Section 4.2 and Figure 1 for details.

Figure B7. Event study results, 1:2 matching



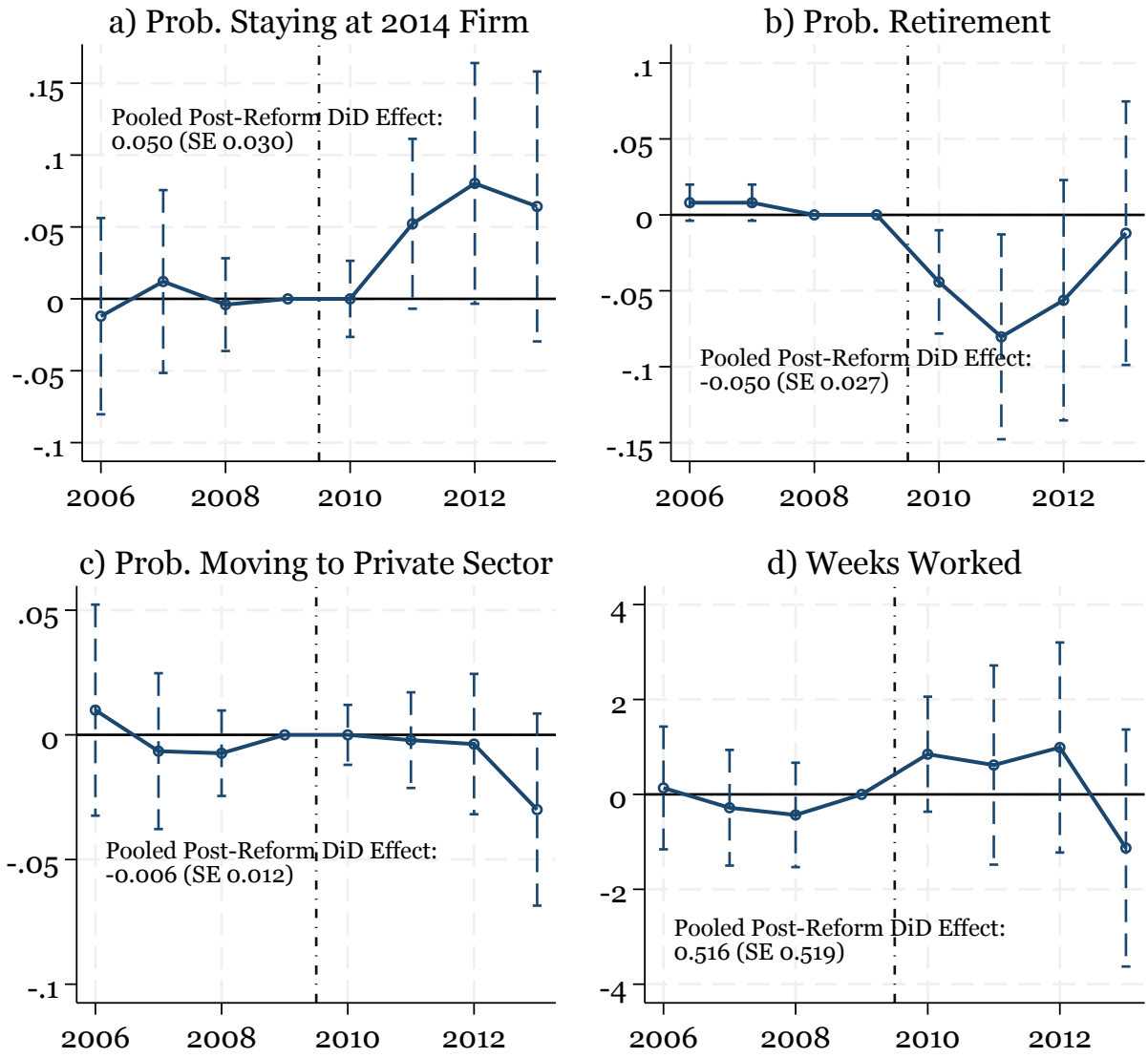
Notes: Event study coefficients resulting from the estimation of Equation 1 on a matched sample of public managers earning above versus below €240,000 in 2014. We perform a one-to-two matching within age*gender cells, rather than a one-to-one as in the baseline design. Standard errors clustered at the worker level. The dashed lines are 95% confidence intervals. See Section 4.2 and Figure 1 for details.

Figure B8. Event study results, no matching



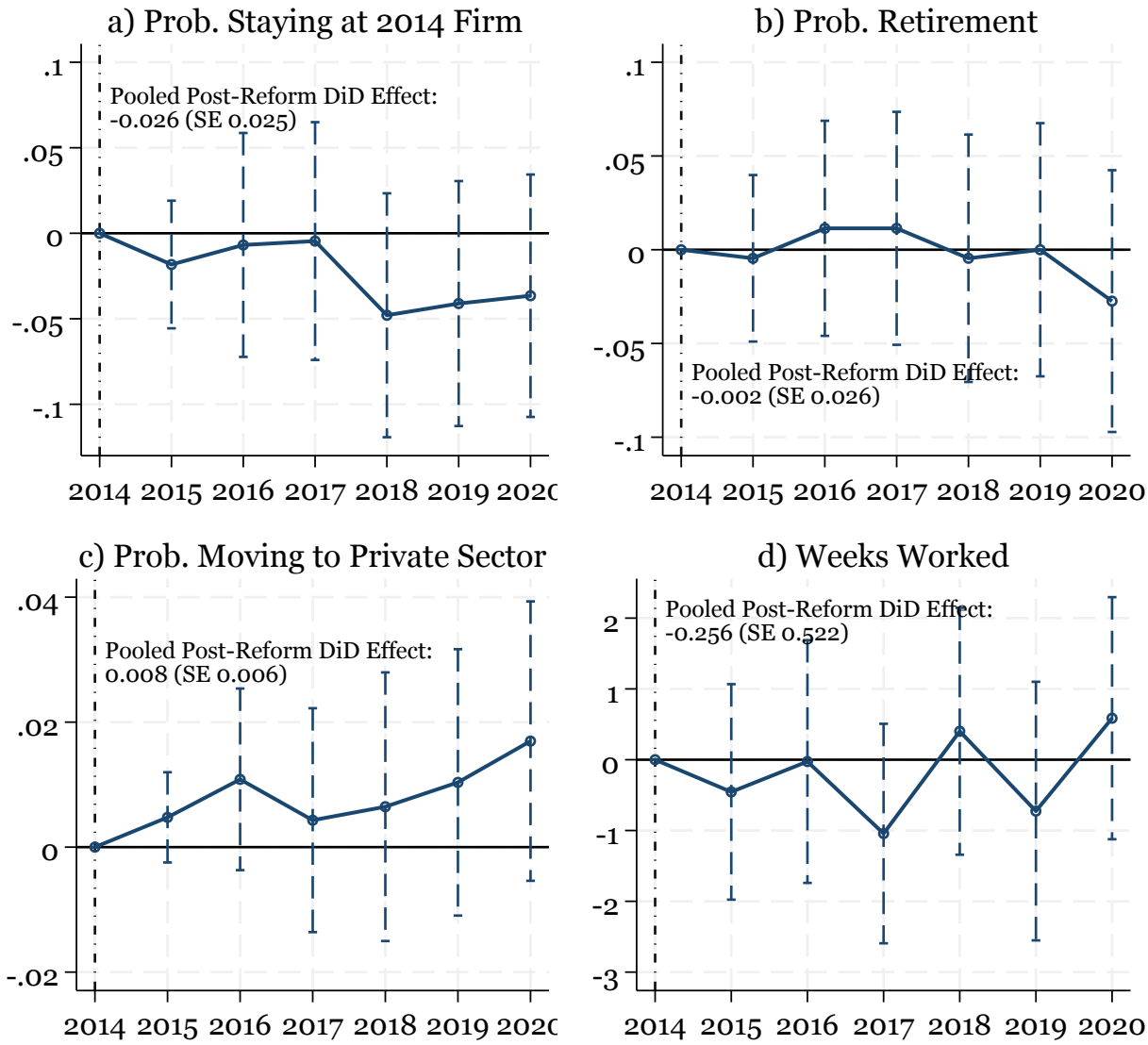
Notes: Event study coefficients resulting from the estimation of Equation 1 on sample of public managers earning above versus below €240,000 in 2014. The analysis is run on the unmatched sample. Standard errors clustered at the worker level. The dashed lines are 95% confidence intervals. See Section 4.2 and Figure 1 for details.

Figure B9. Event study results, placebo reform



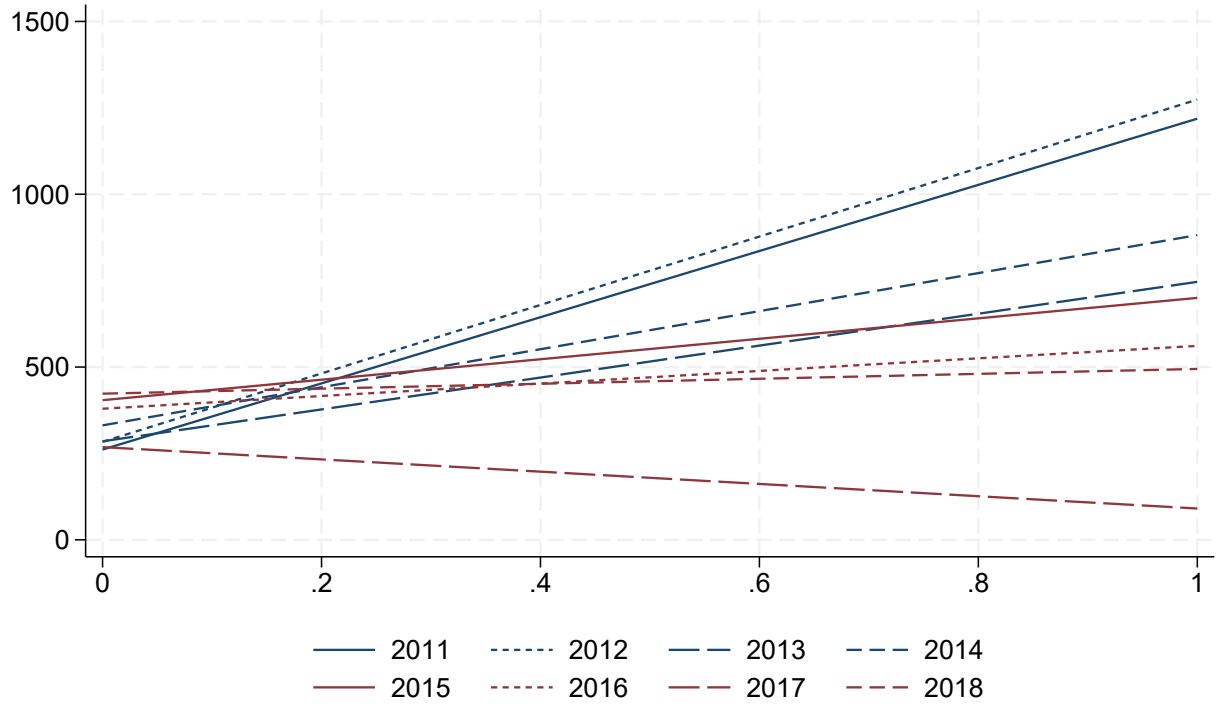
Notes: Event study coefficients resulting from the estimation of Equation 1 on a matched sample of public managers earning above versus below €240,000 in 2010. Standard errors clustered at the worker level. The dashed lines are 95% confidence intervals. See Section 4.2 and Figure 1 for details.

Figure B10. Event study results, other public managers



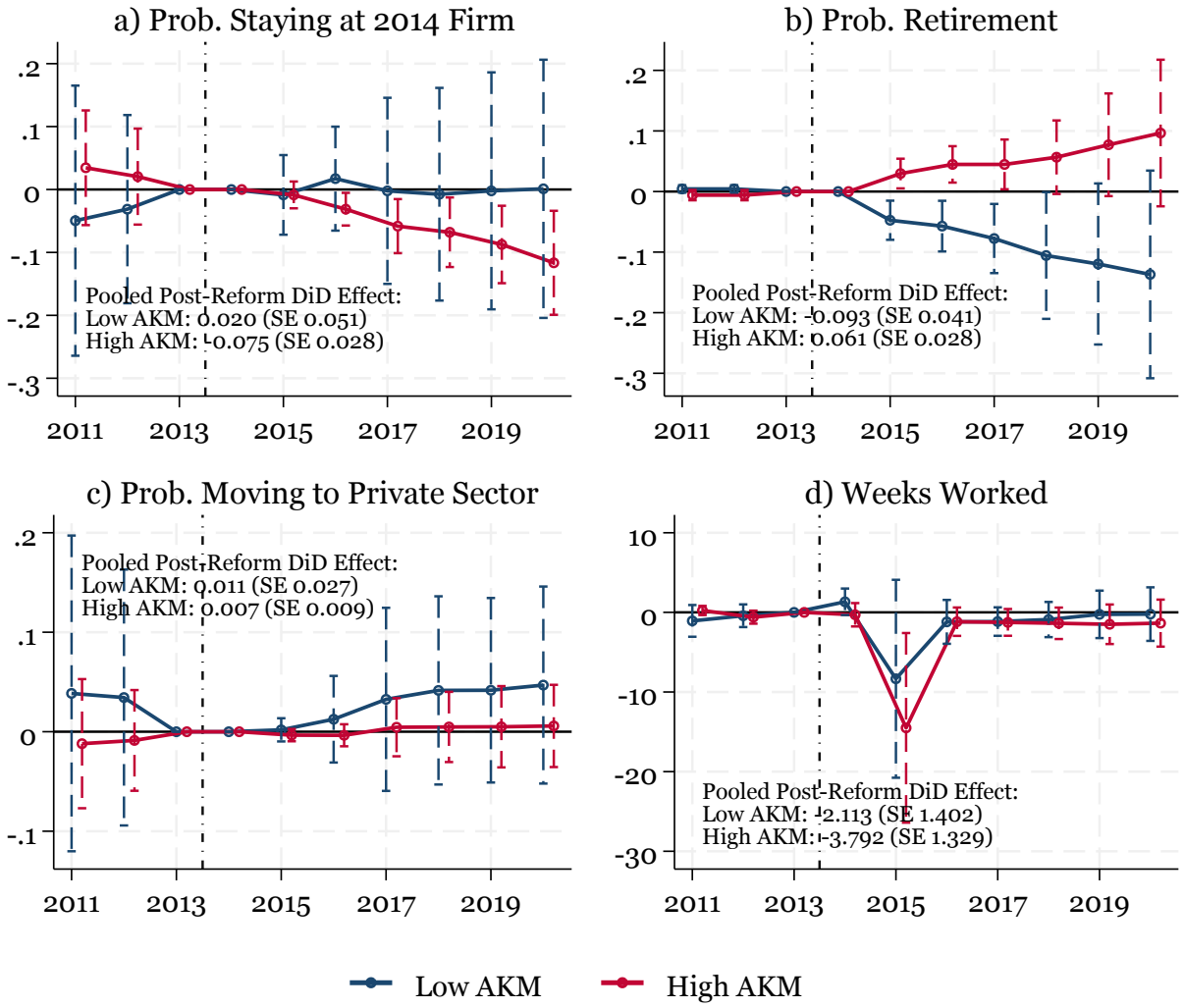
Notes: Event study coefficients resulting from the estimation of Equation 1 on a matched sample of public managers earning above versus below €240,000 in 2014. The sample includes the rest of public managers beyond the public-in-private sample considered in the main analysis. Standard errors clustered at the worker level. The dashed lines are 95% confidence intervals. See Section 4.2 and Figure 1 for details.

Figure B11. Firm-level productivity, descriptive evidence



Notes: The Figure shows fitted lines from yearly cross-sectional correlations between firm value added per worker in 2010 prices (on the y-axis) and firm's number of managers earning above the €240,000 cap in 2014 (as a share of managers earning above €150,000 in 2014, on the x-axis). The firm sample includes only public firms matched with the Cerved balance sheet data and observed trough the entire period (2011-2018).

Figure B12. Event study results, high vs. low AKM effect, co-workers (others)



Notes: Event study coefficients resulting from the estimation of Equation 2 on the matched sample of the co-workers of treated managers who leave the public sector after the cap versus the co-workers of treated managers who stay, only for blue and white collars, and separately for co-workers with above-median and below-median AKM fixed effect. Standard errors clustered at the firm*municipality level. The dashed lines are 95% confidence intervals. See Figure 1 and Section 5.2 for details.

C. Computing Manager Quality

To measure manager quality, we estimate two-way AKM regressions (Abowd et al., 1999) on the universe of Italian public and private workers between 2005 and 2013. As to public workers, the estimation sample only includes public-in-private workers and not workers in the broader Italian public sector (PosPa data, see Appendix A), as we did not observe these before 2014. We retain one yearly observation for each worker as the (dominant) employer with the highest annual earnings (or weeks worked, for equal earnings). We then restrict the estimation sample to the largest connected set of firms in our data, defined by workers moving across firms (Abowd et al., 2002). We then estimate the standard AKM specification:

$$log\ w_{i,t} = \alpha_i + \varphi_{j(i,t)} + X'_{i,t}\beta + r_{i,t} \qquad (3)$$

Here, $log\ w_{i,t}$ is the logarithm of the weekly wage of a worker i in year t and $j(i,t)$ denotes the dominant employer of a worker i in year t. The vector of controls includes year-fixed effects and a cubic in worker's age. The worker-fixed effect α_i is the component of worker i's wage that she receives regardless of her employer, while φ_j denote systematic pay premia at firm j once accounting for worker selection in that firm.[21] As noted in the main text, we will proxy worker quality with the estimated AKM effects α_i, standardized between zero and one.

[21] Endogenous worker mobility – a key identification concern with AKM-type models (Card et al., 2013) – has been rebutted by recent papers using Italian data (Casarico and Lattanzio, 2023; Di Addario et al., 2023).