Mind the Cap?
The Effects of Regulating Bankers' Pay

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Abstract

In this paper we investigate how restrictions to the possibility of paying large bonuses affect employees' pay schemes and firms' ability to attract and retain workers. We focus on the Dutch bonus cap, introduced in the aftermath of the financial crisis, which limits variable pay to no more than 20% of fixed pay for all employees in the banking sector. Using administrative data from Statistics Netherlands, we compare outcomes for banks subject to the cap with those for financial institutions not covered by the regulation. We find that affected employees experience a sharp decline in variable pay, which is almost entirely offset by an increase in fixed pay. The policy is also associated with a rise in involuntary separations from banks and a decline in hiring. All these effects are broadly consistent across the skill distribution. Overall, given the adjustment in compensation structure, our findings do not support the notion that the bonus cap impaired banks' ability to retain their most talented employees.

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1 The Dutch bonus cap policy

Before the outbreak of the Great Financial Crisis, financial regulations typically restricted banks choices concerning investments, lending, and capital reserves. However, following the crisis, regulators and government started introducing rules regulating also pay structure in an attempt to foster the stability of the entire financial system. Aligning the pay incentive of the financial sector with sustainable economic performance was a goal shared by all member states of the European Union in the aftermath of the financial crisis.[1] In 2014, the European Commission implemented the Capital Requirement Directive (CRD) IV, which was introduced with the overarching goal to strengthen the resilience of the European banking sector by implementing the Basel III recommendations.[2]

Although the primary objective of CRD IV was to implement the Basel III capital requirements, it also introduced additional reforms, including a European cap on bonus pay. Specifically, the CRD IV stipulates that variable remuneration cannot exceed 100% of the annual fixed remuneration for *identified staff* of banks and investment firms, where the group of identified staff includes individuals whose professional activities affect the risk profile of the financial institution. However, this 100% bonus cap is not considered particularly stringent, as the regulation allows financial institutions to raise the cap to 200% conditional on shareholder approval.[3]

While the CRD IV is binding for all member states, the regulation allows individual member states to introduce more restrictive bonus cap provisions. The Dutch government took advantage of this flexibility to pass a more restrictive regulation aimed at enhancing the stability of systemically important financial institutions, particularly banks. Specifically, in October 2012, the Dutch coalition government announced its intention to lower the cap on variable pay from 100% to 20% of fixed remuneration through the introduction of the Act on Remuneration Policies of Financial Undertakings (in Dutch *Wet beloningsbeleid financiële ondernemingen*; henceforth Wbfo). The Dutch Lower House adopted the proposal for the bonus cap in October 2014, and the regulation came into effect in February 2015.

The Dutch bonus cap was introduced gradually: it applied immediately to employ-

[1] See Appendix B.1,vwhich provides an overview of the key policies introduced after the crisis to regulate pay schemes in the financial sector, both at the European level and specifically in the Netherlands.

[2] Differently from the CRD IV, the Basel III regulatory framework on the maintenance of capital conservation and counter-cyclical capital buffers is not a law but rather a voluntary set of internationally agreed standards developed by supervisors and central banks.

[3] Increasing the cap beyond 100% requires a vote in favor from at least 66% of shareholders representing a quorum of at least 50% of voting shares. If that quorum is not achieved, then a 75% majority is required. According to the Global Financial Services Executive Compensation Snapshot Survey by Mercer (2014), 70% of EU-based banks requested approval from shareholders or the parent company to increase the variable pay cap to 200% of total fixed compensation.

ees hired in 2015, while existing employees were granted a transition period, with the cap becoming binding for them from 2016. Compared to other EU member states, the Dutch bonus cap is stricter in two key ways. First, the maximum variable-to-fixed pay ratio is five times lower than the 100% limit imposed by CRD IV.[4] Second, the Dutch regulation applies not only to *identified staff*, but to all employees in firms covered by the law, allowing us to analyze its effects across the entire workforce.[5] Importantly, the Dutch bonus cap does not apply to all firms in the financial sector, a distinction we leverage in our identification strategy.

2 Data

The administrative microdata from Statistics Netherlands covers the entire population of Dutch employees. Our primary data source, the Job and Wages Register, is derived from employees' monthly income statements and contains detailed information on earnings, hours worked, contract type, job start and end dates, and firm identifiers. We use the individual identifier and firm identifier to link the Job and Wages Register to additional data sources that provide information on individual and household characteristics (including gender, age, country of birth, marital status, and the presence of children) as well as firm characteristics (such as industry and size). Using this data, we construct a panel dataset that tracks firms and individuals over the period 2008–2019. To account for the timing of performance payments, which are typically disbursed in April, May, or December rather than evenly throughout the year, we aggregate the monthly data from the Job and Wages Register to the annual level.

The following subsections define our analysis samples, outline the specific sectors included in the treatment and control groups, describe the key variables, and present descriptive statistics.

2.1 Incumbent sample and full sample

Our analysis focuses on yearly employment spells where individuals are aged 18 or older, work full-time, hold a permanent contract, and are employed in the financial sector. While the bonus cap policy applies to all employees in treated firms, its primary objective was to influence the behavior of risk-taking bankers rather than lower-skilled employees and administrative staff. Unfortunately, there are no administrative microdata from Statistics Netherlands available that contains the worker's occupation. In order to ensure that our analysis examines the effects of the bonus cap on bankers, as

[4]Only three other European countries - Cyprus, Denmark, and Iceland - have bonus caps lower than CRD IV's threshold. However, all of them set a higher limit than the Netherlands' 20% cap, and in Cyprus and Denmark, the cap can be raised to 100% or more with shareholder approval.

[5]See Appendix B.2 for a list of employee categories exempt from the Wbfo.

opposed to administrative staff or workers employed in peripheral functions, we only retain employees who earned at least 50,000 euro or more in total pay at least once during the pre-policy period 2008-2011.[6]

For most of our analysis, we use a sample of workers who were employed in the financial sector in 2011, which we refer to as the *incumbent sample*. Defining incumbent workers based on their employment status in 2011 accounts for the time lag between the policy announcement in 2012 and its implementation in 2015–2016. This approach enables us to capture potential anticipatory behaviors and examine the full dynamics of the bonus cap's effects. Indeed, although the policy became fully effective for incumbents only in 2016, anecdotal evidence - supported by our data - suggests that banks began adjusting their compensation schemes in advance in preparation for the new regulation.[7] Thus, using 2011 as the reference year for defining the sample allows us to capture individuals who may have exited in anticipation of the bonus cap, ensuring that early responses to the policy are included in our analysis.

To assess the policy's impact on the bank's ability to attract workers, we analyze a full sample of workers rather than limiting our focus to incumbents. This full sample is an annual panel that includes all individuals employed in the financial sector at any point during the 2008–2019 period.[8] As a result, this dataset captures not only the job spells of incumbent workers, i.e. those employed in the financial sector in 2011, but also those of other workers entering the sector at different points in time.

Several other sample selections were applied. First, firm-year observations on when the firm was founded or exited the market were excluded, as this would otherwise confound our analysis on pay and hiring. Second, firms that did not exist in 2011 were excluded. Third, we only include the firms that had at least 10 employees in each year the firm is observed in the pre-policy period 2008-2011. We impose this minimum size threshold to increase comparability between treatment group and control group by excluding from the latter boutique financial services firms. Fourth, given the nature of the regulation, we include in the firms for which we observe an employee's variable to fixed pay ratio above 20% at least once in the pre-policy period 2008-2011.

2.2 Treated and control sectors

Our analysis focuses on firms operating in the financial sector, which, according to the Dutch Firm Industry Classification (Standaard Bedrijfsindeling; SBI), includes all

[6] Amount adjusted for inflation and expressed in 2015 euro.

[7] For example, the ING Group began discussing possible adjustments to remuneration practices as early as 2014, as documented in their Annual Report for that year (ING, 2014).

[8] We apply the same selection criteria as defined above, including only job spells where the individual was employed full-time, held a permanent contract, earned at least 50,000 euro, and was aged 18 or older.

5-digit industries with codes ranging from 64000 to 66300.[9]. Within these financial industries, we distinguish between sectors affected by the 20% bonus cap policy, referred to as treated sectors, and those not covered by the policy, referred to as control sectors. Our classification follows Weitzel et al. (2018), who collaborated with the Dutch Ministry of Finance to define these groups at the 5-digit industry level. According to Weitzel et al. (2018), the bonus cap policy applies to the banking sector, the insurance sector, and a small, heterogeneous category of other financial firms. In contrast, firms not covered by the Wbfo include pension funds and the majority of investment firms.[10] A complete list of 5-digit financial industries, indicating whether they fall under the Wbfo, is provided in Table A1 in Appendix A.

Starting from the classification by Weitzel et al. (2018), we refine the definition of treatment and control groups by excluding certain financial industries to ensure that the *only* regulatory difference between treated and control sectors is the Wbfo. Specifically, we remove from the control group pension funds, as firms in this category fall under the Pensions Act, which includes specific regulations on sound remuneration implemented in 2014 and updated in 2018 that could interfere with the identification of the bonus cap's effects.[11]. After these refinements, our final control group consists of all remaining financial industries labeled as "other control sectors" in Table A1. .

Similarly, we refine the treated group to exclude financial industries subject to additional regulatory frameworks that could confound the identification of the bonus cap's effects. Specifically, we exclude the insurance sector from the treatment group, as it falls under the European Solvency II regulation, which imposes additional rules on remuneration practices that could make it difficult to isolate the impact of the Wbfo. Furthermore, we exclude the miscellaneous category of treated industries (labeled as "other treated sectors" in Table A1) to ensure that our treatment group remains more homogeneous, consisting exclusively of banks. This refinement allows us to more clearly isolate and discuss the effects of the bonus cap policy on bankers' careers, without potential confounding influences from smaller, less comparable financial firms. Importantly, this exclusion does not affect our results, as these industries are

[9]The Dutch Industry Classification follows the EU industry classification (Nomenclature statistique des activités économiques; NACE Rév. 2) up to the 4-digit level. Each firm entity is assigned a single industry code, meaning that for firms engaged in multiple activities, Statistics Netherlands imputes the industry corresponding to the activity that contributes the most to the firm's overall 'value'. This value is determined based on various factors, including staff size, revenues, and the firm's control structure. In the case of large firms comprising multiple entities, each entity is treated as a separate firm in our dataset. A firm entity is considered separate if it has legal control over its own activities, as defined by Statistics Netherlands in accordance with the Eurostat recommendations manual on business registers (Eurostat, 2010).

[10]For more details, see footnote 32 of the policy report by Weitzel et al. (2018).

[11]Additionally, we exclude the Central Banking sector and Trust Offices sector, as these industries do not fall under the Dutch Sound Remuneration Policies (in Dutch: Regeling beheerst beloningsbeleid (Rbb) Wet op het financieel toezicht (Wft)) implemented in 2011. See Appendix B.1 for a summary of regulations before the Wbfo)

significantly smaller than the banking sector. Indeed, as shown in Table A1), in 2011, they employed only around 4,000 workers, compared to more than 38,500 employees in the banking industry.

Given the above selection of industries, Table 2 provides information about the relative importance of each 5-digit sector included in the treatment and control group. Within the banking industry, which makes up our entire treatment group, the most relevant category is that of general retail banks under which 86% of the treated firms fall. The remaining firms are either cooperative banks, saving banks, or stock credit companies. The control group is more heterogeneous in terms of sectoral composition: about 38% of firms are identified as investment firms, 22% are institutions dealing with risk and damage evaluation, and 21% are participation companies such as private equity and venture capital firms. The remaining firms in the control group provide other financial services, such as managing pension funds, or are financial holdings. Finally, firms in the treatment and control groups differ significantly in size. The treatment group, consisting exclusively of banks and comprising 43 distinct firm entities, employs an average of 895 individuals per firm. In contrast, the control group, which includes 154 distinct financial institutions, is considerably smaller, with an average of only 23 employees per firm.

2.3 Key outcome variables

2.3.1 Pay composition variables

The Dutch registry data offers two key advantages in recording employees' pay. First, it is not top-coded, allowing us to observe the full distribution of earnings. Second, it distinguishes between different pay components, which is crucial for analyzing how banks adjust their compensation schemes following the introduction of the 20% bonus cap. [12]

Variable pay in our dataset is measured as the sum of two types of payment: variable cash payments and variable in-kind payments. Variable cash payments include bonuses paid in cash, while variable in-kind payments consist of bonuses awarded in financial instruments such as stocks. These stock-based bonuses are recorded in our data because they trigger an income tax event, which appears on the employee's income statement at the time they are awarded. However, an important exception applies to stock options. Unlike regular stock bonuses, stock options are not taxed when awarded but rather when exercised by the employee. This tax event occurs

[12]Each pay component is deflated and expressed in real terms using the 2015 Dutch consumer price index. For job spells lasting fewer than 12 months within a calendar year, we adjust fixed pay, total pay, and hours worked to an annual full-time equivalent. After selecting employees in the finance industry, we apply winsorization to all pay components, capping extreme values at the 1st and 99th percentiles of the respective variable to mitigate the influence of outliers.

regardless of whether the stocks are immediately tradable. As a result, stock options are generally unobserved in our dataset at the time of the award, posing a limitation in our analysis.

Nevertheless, in the Netherlands, employees often choose to exercise their stock options early. The key reason is tax-related: by exercising options sooner, employees can ensure that any future increase in stock value is subject to the wealth tax rather than the income tax. The Dutch wealth tax is relatively low compared to the income tax and is based on fictitious rates of return, rather than actual capital gains.

There are two other limitations to the variable pay data. A first limitation is that 40% to 60% of the variable pay is deferred, consistent with the 2011 CRD III regulation (see Appendix B.1). Importantly, this only holds for identified staff.[13] The tiered vesting is up to five years for senior management and up to three years for other identified staff members. Generally, deferred income is taxed in the year it is awarded. This creates a timing problem for our analysis as vested income is paid out several years later. We argue this limitation is of limited importance for two reasons: i) only a small proportion of all employees is part of identified staff. For example, for the largest bank of the Netherlands, ING, about 1% of employees are considered 'identified staff' in 2012 (from this site and this site). ii) in our unique Dutch bonus cap context, we analyse all employees instead of only identified staff. Hence, the number of individuals affected by this mismeasurement is relatively low.

A second limitation of the data is that not all variable pay is performance-related. For example, non-performance-related bonuses paid in the year a job ends - such as separation bonuses - cannot be separately identified in the data. To address this, we exclude observations from the final year of each job spell in our main pay analysis. In Section 4.3, we present a series of robustness checks to assess the sensitivity of our results to this exclusion. In addition, small non-performance-related in-kind benefits, such as company phones or cars, are included in the in-kind payment component but are not separately itemized. We mitigate this issue by including job-spell fixed effects, which absorb any time-invariant variation in such non-performance-related in-kind transfers within a given job spell.

Fixed pay is defined as the sum of standard labor income and standard non-performance pay, in line with the European Banking Authority's guidelines on sound remuneration policies (EBA/GL/2015/22). According to these guidelines, any benefit specified in the employee's contract and earned independently of performance - regardless of whether it is labeled as variable - should be considered part of fixed pay. Total pay is defined as the sum of fixed and variable pay.

Based on the pay components defined above, we construct a measure of the variable-

[13] Identified staff include: Executives, Senior management, Staff responsible for independent control functions; and Other risk takers.

to-fixed pay ratio, calculated as the ratio of variable pay to fixed pay. This measure is directly targeted by the Wbfo regulation, which imposes a 20% cap on its value. Accordingly, we also define a binary indicator that equals one if the variable-to-fixed pay ratio is 20% or higher, and zero otherwise.

2.3.2 Employee retention variables

We exploit the richness of our data to analyze the effects of the bonus cap policy on banks' ability to retain employees. To this end, we define several outcome variables. First, we examine overall separations by studying job-spell dissolution, measured using a binary indicator that equals zero prior to separation and one in the year the job spell ends.

Next, to assess how the regulation affects workers' career trajectories, we distinguish among several exit states based on the first employment status observed in the year following job separation (where we define a year as the 365 days after separation). In particular, we differentiate between voluntary and involuntary leavers, depending on the worker's post-separation outcome. Involuntary leavers are defined as individuals who experience job-spell dissolution and remain non-employed throughout the entire 365-day period. This includes individuals who receive unemployment benefits and, depending on the operationalisation, may also include individuals who retire early. Specifically, early retirement is considered an involuntary separation and is defined as occurring when the individual is younger than 65 years and receives retirement benefits as their main source of income.

Voluntary leavers are individuals who do not become unemployed during the year following the separation. Among voluntary leavers, we distinguish several exit states based on the first observed employment status. These include normal retirement, defined as individuals aged 65 or older whose main source of income is retirement benefits; employment outside the financial sector (change of industry); and various types of employment transitions within the financial sector. The latter group includes individuals who change employer but retain the same treatment status within the industries included in our analysis (e.g., treated-to-treated or control-to-control), those who retain the same treatment status but move to a financial industry not included in our analysis (e.g., treated to insurance or control to pension), and those who change treatment status either within or outside our industry sample (e.g., treated to control, or treated to pension). We also identify a residual group comprising transitions to financial industries for which treatment status cannot be determined.[14] Finally, we identify individuals who become self-employed, and those who are no longer registered in the Netherlands. In the latter case, since we cannot distinguish between emigration

[14]These cases are labeled as transitions to the residual finance sector, and include sector codes 64921, 64922, 66110, and 66193.

and mortality, we classify both under the "abroad" category. However, given our sample restrictions that retain the active workforce, we assume most cases in this category represent moves abroad. These exit states are all mutually exclusive.

2.3.3 Employee attraction variables

In a similar way to the employee retention variables, we operationalise employee attraction by defining several distinct entry states based on individuals' employment status in the year preceding a new job spell (where we define a year as the 365 days prior to entry).

We first identify hires from unemployment, defined as individuals who were not employed and did not appear in any other observable category during the year before starting their new employment spell. Among those who are observed in a specific employment state prior to being hired, we distinguish several mutually exclusive categories based on their previous position.

Individuals who were previously employed outside the financial sector are classified as having changed industry. Those who change employer within the financial sector are further categorised according to their treatment status. We identify individuals who switch employer but retain the same treatment status within the industries covered in our analysis (e.g., treated-to-treated or control-to-control), and those who retain the same treatment status but move to a financial industry outside the scope of our analysis (e.g., treated to insurance or control to pension). We also distinguish individuals who change employer within the financial sector and change treatment status within the industries we analyse (e.g., treated to control or vice versa), and those who change treatment status while moving to a financial industry outside our sample (e.g., treated to pension or control to insurance). In addition, we include a residual group consisting of entries from finance sectors for which we cannot determine treatment status; this group includes transitions such as treated to residual or control to residual.

Finally, we identify individuals who were previously self-employed, based on self-employment being their main source of income in the year prior to entry, and those who enter from abroad, defined as individuals who were not registered in our data in the year prior to being hired.

2.3.4 Firm-level attraction and retention variables

To study attraction and retention of employees we also use an annual panel of firms in the period 2008-2019.

The outcome variable is defined as the ratio between the cumulative joiners at the firm and the baseline staff, where cumulative joiners represent all joiners from 2008 onwards up to the given year and baseline staff represents the total number of workers employed in the firm in 2011. We compute similar outcome variables based

for cumulative leavers relative to baseline staff as well as for total staff relative to baseline staff. We divide the respective categories by the baseline staff to facilitate the comparison between firms who differ a lot in staff size. Similarly, we focus on cumulative joiners and cumulative leavers since 2008 instead of year-specific joiners and leavers to reduce the variation that small firms experience. Using the similar strategy to categorize voluntary separation and involuntary separation as discussed in the previous section, we also analyse cumulative voluntary separations relative to baseline staff and cumulative involuntary separations over baseline staff.

2.4 Heterogeneity analysis based on employees' talent

We are interested in whether the effects of the bonus cap on pay structures differ for the most talented workers. To study this, we adopt two complementary approaches.

The first approach defines talent using an absolute measure of earnings, based on the employee's maximum total pay observed during the baseline period 2008–2011. Specifically, we group individuals into three income categories: those earning between 50,000 and 100,000 euro, between 100,000 and 175,000 euro, and above 175,000 euro at baseline. Compared to a relative measure of talent that identifies top workers within a firm, using absolute earnings captures the idea that top talent in a smaller firm may differ from top talent in a larger firm. Thus, this approach also has the advantage of accounting for differences in the average level of talent across firms and industries.

The second approach categorizes employees as talented or non-talented in the spirit of Abowd et al. (1999), Card et al. (2013), and Bender et al. (2018), by defining talent based on estimated worker fixed effects. Our strategy proceeds as follows.

We begin by constructing a sample of all employees who worked at a bank or control firm at any point between 2008 and 2019. We then identify all firms where these employees were ever employed during the same period, including those outside the financial sector. This yields a connected worker-firm sample suitable for estimating worker fixed effects. Since individuals may appear more than once in a given year, due to job changes, we follow Card et al. (2013) and retain only the observation with the highest annual total pay.

A key advantage of our data for the application of the AKM framework is the availability of detailed information on working hours. This allows us to construct a log hourly wage variable, defined as the logarithm of annual total pay divided by annual working hours, which we use as the dependent variable. We estimate worker fixed effects over the period 2008–2012. This window is chosen to maximise sample coverage while ensuring identification: since worker fixed effects are estimated only for individuals with at least two job-spell-year observations, using this period allows us to retain individuals who entered employment as late as January 2011.

We estimate the worker fixed effects η using the following model

$$log(hourlywage)_{ift} = \alpha + AGE_i * EDUCATION_i\lambda + \eta_i + \tau_f + \phi_t + \epsilon_{ift} \quad (1)$$

where η represents worker fixed effects and τ represents firm fixed effects. Following Card et al. (2013), we interact age categories (< 25, $[25, 30)$, $[30, 35)$, $[35, 40)$, $[40, 45)$, $[45, 50)$, $[50, 55)$, $[55, 60)$, ≥ 60) with educational attainment (four categories including one for missing education) as well as year fixed effects ϕ.

We define talent as a binary indicator equal to one for individuals who are in the top 5% of the worker fixed effect distribution within their firm at least once during the period 2008–2011. This relative measure allows for differences in the average level of talent across firms and industries, as it captures an individual's standing within their firm rather than across the entire labor market. By focusing on the within-firm distribution of estimated skills, this measure enables us to examine whether the effects of the bonus cap on pay schemes differ for the most talented workers within each company. A further advantage of the relative measure, compared to the absolute measure based on earnings, is that it implicitly controls for age-by-education effects, as these are absorbed in the estimation of worker fixed effects.

2.5 Descriptive statistics

Table 1 presents descriptive statistics for the sample of incumbent employees working at treated and control firms.[15] The table reports individual-level mean values of compensation and personal characteristics for the year 2011, the year before the bonus cap was announced, which serves as our baseline year.

The table shows that there were some differences in pay structures between treated and control firms prior to the policy announcement. In particular, average variable pay in the treatment group was lower than in the control group (8,329 euro versus 10,071 euro), and the same holds for fixed pay (77,238 euro versus 79,702 euro) and total pay (85,577 euro versus 89,773 euro). We also report variable pay broken down in its two components: cash and stocks. Overall, cash-based variable pay is the dominant category in our data, while stock-based variable pay is negligible, especially in the treatment group. The average variable pay ratio, defined as the employee's variable pay relative to fixed pay, equals 10.5% for the treated group and 12% for the control. Finally, the share of employees with a variable-to-fixed pay ratio exceeding 20% is 10.5% among treated individuals and 20% among controls. Overall, these sample means highlight that although the Dutch bonus cap was targeted at bankers, employees

[15] While treatment status is defined at the sector level, for brevity we refer to treated (control) individuals or treated (control) firms to indicate workers employed in firms operating within treated (control) sectors.

in other financial firms—those in our control group—received higher variable pay ratios on average.

In terms of workforce characteristics, treated firms employed a higher share of female workers (25.1% for treated employees versus 16.8% for control employees). Compared to control firms, treated firms also employed a workforce that is slightly more highly educated and more likely to be born in the Netherlands.[16]. The average age of employees is similar across both groups, at approximately 45 years. Table 1 also shows that, in line with the descriptive statistics on pay, the share of employees identified as talented is slightly lower in treated firms compared to control firms both when using the absolute and relative measure.

3 Empirical strategy

3.1 Identification challenges and strategy

There are several identification challenges in the context of our policy evaluation.

A first challenge is the potential for anticipation effects, as the Dutch bonus cap policy was announced in 2012 but only came fully into effect in 2016. To address this, we employ a dynamic differences-in-differences (DiD) estimator, which allows us to flexibly examine the evolution of employee and firm outcomes from 2008 to 2019. This approach enables us to inspect changes both around the 2012 policy announcement and during the 2015–2016 implementation period, thus capturing potential anticipatory effects in the 2012–2015 window. The key identifying assumption of the DiD methodology is the absence of differential pre-trends between treated and control units. Although this parallel trends assumption is not directly testable, the non-parametric nature of our dynamic DiD approach allows us to visually assess its plausibility using multiple years of pre-reform data.

A second challenge is that changes in pay structures between treated and control firms may be driven by both changes in incumbent workers' pay and changes in workforce composition. To isolate the effects driven by pay changes, we analyse outcomes for incumbents, thereby holding constant workforce composition on the hiring margin. However, composition changes due to separations could still influence results. To address this, we complement the incumbent analysis with an investigation of hiring and retention patterns for the full sample, disaggregated by ability group.

[16]Following the International Standard Classification of Education (ISCED), education is categorised into lower education (ranging from less than primary to lower secondary), secondary education (from upper secondary to post-secondary non-tertiary), and tertiary education (from tertiary to doctoral studies). As educational attainment is mismeasured for individuals graduating before 1995, the proportions in Table 1 exclude missing values. In the AKM estimation, we include education as four categories, with one category capturing missing data.

A third challenge relates to job-to-job transitions between treated and control firms, which could lead to spillover effects. Since the bonus cap applies only to treated firms, employees may be more likely to move to firms not subject to the cap, potentially "contaminating" the control group. These spillovers are particularly relevant if treatment and control sectors are closely interconnected through labour mobility. We assess the extent of these spillovers in our separation analysis, where we directly examine whether the introduction of the bonus cap affects the likelihood of transitioning from a treated to a control firm. Moreover, we also examine job-to-job transitions in the pre-policy period (2008–2011), identifying the main industries from which employees entered the treatment or control sectors and the main industries they moved to upon separation. Table A2 shows that treated and control sectors were not highly interconnected in terms of labour flows in the pre-policy period.

A final concern is the presence of bank-specific shocks that could explain our results but are unrelated to the bonus cap policy, and yet may still be attributed to it because our control group consists of financial institutions that are not banks. To address this, in Section 4.3.2 we assess the robustness of our findings using an alternative identification strategy that exploits only within-bank variation.

3.2 Analysis at the individual level

3.2.1 Empirical models on pay

For each incumbent employee i working in firm f in year t we estimate the following models:

$$Y_{ift} = \alpha + \sum_{t=2008}^{2019} \beta_t \, Year_t + \gamma_{if} + \epsilon_{ift} \tag{2}$$

$$Y_{ift} = \alpha + \sum_{t=2008}^{2019} \delta_t \left(Bank_{if} * Year_t \right) + \gamma_{if} + \phi_t + \epsilon_{ift} \tag{3}$$

where Y_{ift} represents one of our pay variable. γ_{if} denotes job-spell fixed effects, capturing all time-invariant characteristics of the worker, the firm, and their match. By including job-spell fixed effects, the analysis focuses exclusively on within-job-spell variation, that is, changes in pay that occur while an employee remains with the same employer. Standard errors are clustered at the job-spell level.[17]

[17] A more conservative approach would be to cluster at the treatment group level - i.e., two clusters - or at the 5-digit SBI or firm level. However, the number of unique clusters in these cases is too limited to ensure reliable inference. We therefore cluster at the job-spell level, which is appropriate given that treatment assignment depends on an individual's variable-to-fixed pay ratio. Employees with a ratio below 20% are unlikely to be affected by the bonus cap, as we show in our analysis.

Equation (2) is estimated separately for treated and control employees, and the coefficients β_t trace how the outcome variable evolves for each group over the period 2008–2019, relative to the reference year 2011. To assess whether compensation schemes at firms subject to the bonus cap policy evolved differently from those at control firms, we estimate the dynamic DiD specification outlined in equation (3). The coefficients of interest, δ_t, capture the dynamic treatment effects over time.[18] We consistently present results for treated and control groups separately, based on Equation (2), to transparently assess whether the DiD effects are primarily driven by changes in the treatment group rather than shifts in the control group.

3.2.2 Empirical model on retention and attraction of employees

Comparable models to Equations (2) and (3) are used to analyse the retention of incumbent workers and the attraction of new hires, though these models do not include job-spell fixed effects.

To study changes in the retention of incumbent workers around the bonus cap policy, we define the outcome variable as a binary indicator that takes the value one in the year the job spell ends, and zero in all preceding years. Given the nature of this outcome - where there is no within-job-spell variation a part for the single year in which a worker leaves the firm - job-spell fixed effects are not included. The observation window spans from 2011 to 2019, since, by definition, incumbent workers cannot exit their job before 2012. Each individual remains in the sample until they experience job-spell dissolution.

To analyse changes in workers' probability of joining treated or control firms over time, we use data from the full sample period, 2008 to 2019, and retain only the observation corresponding to the year in which the employee starts a job. As only one observation per worker is used, job-spell fixed effects are again not included.

3.3 Analysis on attraction and retention at the firm level

To investigate the effect of the bonus cap on banks' ability to attract and retain workers we use a panel of firm-year observations computed using the full sample of employees (including those joining the firm after or leaving the firm before the policy announcement). We estimate the following model:

$$Y_{ft} = \alpha + \sum_{t=2008}^{2019} \delta_t \left(Bank_f * Year_t\right) + \gamma_f + \phi_t + \epsilon_{ft} \tag{4}$$

[18] In a robustness check, we add a vector of individual characteristics and job characteristics X_{ift} to the empirical models. This set includes binary indicators for age (in eight 5-year bins), marital status (married or in a de facto partnership versus single), parenthood, and firm size (1–19; 20–199; 200–499; 500 or more employees).

where f and t index firm and year. The coefficient of interest δ_t captures the dynamic differential effect of the bonus cap policy on the ability of treated firms, as opposed to control firms, to attract or retain workers. γ_f denotes firm fixed effects and ϕ_t represents year fixed effects. Analytical weights are used based on staff size in 2011 to take the number of employees by firm into account, making larger companies more important. Standard errors are clustered at the firm level.

4 Effects on Pay Structure

In this section we estimate the effects of the Dutch bonus cap policy on workers' remuneration structure. In sections 4.1 we show our baseline results, which are based on the estimation of Equation (2) and (3) for the sample of incumbents. In Section 4.2 we present our heterogeneity analysis depending on our relative and absolute measure of workers' talent. Finally, in section 4.3, we perform a number of robustness tests to assess the sensitivity of our results on pay.

4.1 Baseline Results

We begin by examining the extent to which banks complied with the Dutch bonus cap regulation. Although the policy formally applies to all employees working in treated sectors, compliance may be less than complete if banks choose to circumvent the regulation - accepting the risk of regulatory sanctions[19] - or if employees qualify for one of the few existing exceptions.[20] To answer this question, we define our outcome variable of interest as a binary indicator equal to one if an employee's variable-to-fixed pay ratio exceeds 20%, and zero otherwise.

Results are showed in Panel (a) of Figure 1, which plots the β_t coefficients from equation (2) for employees in the treated and control group separately, as well as the δ_t coefficients from equation (3) with 95% confidence intervals.

The first notable observation is the absence of differential pre-trends in the probability that an employee's variable-to-fixed pay ratio exceeds 20% between those working in banks and those in control firms. Starting in 2013—the year following the policy announcement—the probability of exceeding the 20% threshold peaks upward by approximately 20 percentage points for treated employees. This increase is consistent with banks compensating employees ahead of the policy's implementation, potentially in anticipation of the forthcoming restrictions on variable pay. In line with the policy

[19]For example, Rabobank was recently fined by the Dutch Central Bank for violating the CRD IV regulation on the 100% bonus cap in 2014 (NOS, 2020).

[20]We report these exceptions in Appendix B.2. While we are not able to fully identify which employees fall under these exceptions in our data, based on Ministerie van Financiën (2018), they are estimated to apply to less than 1% of the sample used in our analysis.

objective, from 2014 onward, the probability of having a variable-to-fixed pay ratio above 20% declines for treated employees relative to control employees. Specifically, in the period 2015–2019, treated employees were between 4 and 11 percentage points less likely than control employees to exceed the 20% threshold. Given that the baseline probability for bankers to have a variable-to-fixed pay ratio above 20% was 10.5% (see Table 1), and that the estimated coefficient for treated employees in 2019 is –5.7 percentage points, our results imply a 54.3% reduction in this probability by 2019.

There are multiple ways in which banks can reduce the relative importance of variable pay to comply with the Dutch bonus cap regulation. Specifically, banks may lower variable pay, increase fixed pay, or implement a combination of both, with potentially ambiguous effects on total compensation. How firms adjust their pay structures in response to a bonus cap is therefore an empirical question, and our analysis contributes novel evidence on this issue.

To assess how the policy affected the composition of compensation, we begin by estimating our baseline regressions where the outcome variables are defined as the shares of variable and fixed pay relative to total pay. Since the policy may also affect the level of total pay itself, we normalize the components of pay by the individual's total pay in 2011, our baseline year and the year prior to the policy announcement. By indexing both variable and fixed pay to a time-invariant measure of total pay, we can trace how the relative importance of each component evolved in response to the regulation. We report our estimates for changes in variable and fixed pay relative to 2011 total pay in Panel (b) and Panel (c) of Figure 1. Three key observations emerge. First, for both outcomes, the figures show no evidence of significantly different pre-trends between treated and control employees during the pre-policy period, lending support to the parallel trends assumption.

Second, Panel (b) of Figure 1 shows a sharp increase in bankers' variable pay in 2013, followed by a steady decline beginning in 2014 and continuing through 2019. This pattern is consistent with the dynamics observed in Panel (a) of the same figure. In contrast, variable pay in the control group follows a steady upward trend over the entire period. As a result, by 2019, variable pay relative to 2011 total pay is nearly 10 percentage points lower for treated employees compared to controls. This is a substantial effect, especially considering that at baseline, the difference in variable pay as a share of total pay between control and treated employees was only 0.8 percentage points (see Table 1). Moreover, since variable pay accounted for 8.9% of total pay among bankers in 2011, the 6.1 percentage point decline observed in 2019 represents a 68.5% drop relative to the pre-policy level.

Third, Panel (c) of Figure 1 shows that while fixed pay increases over time for both treated and control employees, starting in 2014 it grows significantly faster for bankers. By 2019, fixed pay relative to 2011 total pay is approximately 5 percentage

points higher for treated employees than for those in the control group. This is a substantial difference, especially considering that the baseline gap in fixed-to-total pay between treated and control employees in 2011 was only 0.7 percentage points (see Table 1). This result is relevant for at least two reasons. First, it suggests that banks responded to the reduction in variable pay by compensating employees through higher fixed pay. Second, it helps rule out the possibility that the observed decline in bankers' variable pay is driven by deteriorating firm performance, as a downturn in bank profitability it's unlikely to result in rising fixed compensation for employees.

Finally, we complete this part of the analysis by examining how the changes in variable and fixed pay affect overall compensation. Specifically, we define our outcome of interest as the ratio between total pay in a given year and total pay measured at baseline, i.e., in 2011. The estimates from Equation (3) are reported in Panel (d) of Figure 1. As with the previous outcomes, we find no evidence of differential pre-trends: prior to the policy, total pay evolves along a similar upward trajectory for both treated and control employees.[21] In the post-policy period, however, total pay for treated employees is consistently lower than for control employees - with the exception of 2018 - with magnitudes ranging from -1 percentage point to -3.8 percentage points.

While the graphical representation of the sub-panels in Figure 1 offers a transparent view of pre-trends and the dynamics of the policy effects, we also present more aggregated results using a standard difference-in-differences specification. In this approach, we define three distinct time periods: (i) pre-policy (2008–2011); (ii) post-announcement but pre-implementation (2012–2015); and (iii) post-implementation (2016–2019). The corresponding results for the full sample of incumbents are reported in Panel (A) of Table 3, and are similar to those we estimate with the fully dynamic specification. In particular, we find that in the post-implementation period, workers in treated sectors experience a 5 percentage point decline in the relative importance of variable pay over total baseline compensation, and a 4.6 percentage point increase in the relative importance of fix pay, compared to employees in control sectors. These changes are accompanied by a statistically insignificant reductions in total compensation.

4.2 Heterogeneous Effects

The results documented in the previous section are estimated on the full sample of incumbent workers. We now turn to examining whether the changes in pay structures induced by the policy vary depending on workers' talent, as defined by the absolute and relative talent measures described in Section 2.4.

[21] The absence of a decline in bankers' total pay during the financial crisis aligns with the broader narrative that bankers did not "pay" for their mistakes, consistent with evidence from other countries, such as Bell and Van Reenen (2014).

To ease exposition, we primarily focus on heterogeneity by workers' baseline pay levels, distinguishing between employees whose maximum total pay in the pre-policy period fell into one of three brackets: between 50,000 and 100,000 euro, between 100,000 and 175,000 euro, and above 175,000 euro. The corresponding results are presented in Figure 2. We obtain similar results when categorizing workers based on the relative talent measure—that is, whether their individual fixed effect, estimated according to Equation (1), falls in the top 5% of the within-firm distribution. These results are reported in Appendix Figure A2. Finally, Panels (B) to (F) of Table 3 present the corresponding estimates by income group and worker fixed effects, obtained using a standard difference-in-differences specification rather than the fully dynamic model.

Panel (a) of Figure 2 examines the effects of the policy on incumbent workers' probability of having a variable-to-fixed pay ratio above 20%. Specifically, the figure plots the estimated δ_t coefficients from Equation (3), along with 95% confidence intervals, separately for workers in each of the three income groups. The results show that as a result of the bous cap policy treated workers are consistently less likely than control workers to exceed the 20% threshold, regardless of their income group. However, the reduction is notably larger for workers in higher income brackets. By 2019, bankers in the top income group are 35.8 percentage points less likely than their counterparts in the control group to have a variable-to-fixed pay ratio above 20%, while the corresponding figure for the middle-income group is 20.8 percentage points. These are substantial effects when compared to the baseline differences in 2011, which were 9.9 and 13.6 percentage points for the top and middle income groups, respectively (see columns (7) and (8) of Table 1).

The results show that treated workers are consistently less likely than control workers to exceed the 20% threshold, regardless of income group. However, the reduction is notably larger for workers in higher income brackets. By 2019, bankers in the top income group are 35.8 percentage points less likely than their counterparts in the control group to have a variable-to-fixed pay ratio above 20%, while the corresponding figure for the middle-income group is 20.8 percentage points. These effects are sizable. At baseline (in 2011), control employees were already more likely than treated employees to exceed the 20% threshold by 9.9 and 13.6 percentage points, respectively for the top and middle income groups (see columns (7) and (8) of Table 1). Thus, the policy led to a significant widening of the pre-existing gap in the probability of having a variable-to-fixed pay ratio above 20% between treated and control workers at the top of the earnings distribution.

Panel (b) of Figure 2 displays the effects of the policy on the ratio between variable pay and the baseline level of total pay. The results mirror those observed in Panel (a): the decline in the relative importance of variable pay is generally larger for higher-

income groups. This pattern is also evident in the estimates reported in column (2) of Table 3, Panels B, C, and D. These findings are consistent with the fact that high-income workers had a higher variable-to-fixed pay ratio at baseline, as shown in Table 1.

Turning to Panel (c), which reports the impact of the policy on the ratio of fixed pay to baseline total pay, we find less clear evidence of heterogeneity by income group, particularly in the post-implementation period. A comparison of the aggregate effects in Panels C and D of column (3) in Table 3 suggests that, if anything, the increase in fixed pay relative to baseline total pay is smaller for treated workers in the highest income group than for those in the lower and middle income groups, both during the post-announcement and post-implementation periods.

In line with these differential effects on variable and fixed pay, Panel (d) of Figure 2 shows that the overall impact of the policy on total pay is most pronounced for high-income workers. Specifically, the aggregate estimates reported in column (4) of Table 3 indicate small and statistically insignificant effects on total pay for low- and middle-income workers, while treated workers in the highest income group experience significant negative effects in the post-implementation period.

All of these results are confirmed when we categorize workers using our relative measure of talent. In particular, comparing the estimates reported in Panels E and F of Table 3, we find that the changes in pay schemes between treated and control workers in the top 5% of the within-firm talent distribution are more pronounced than the difference in pay schemes between treated and control workers outside the top 5%. Specifically, high-talent treated workers experience larger reductions in variable pay, smaller increases in fixed pay, and a negative and statistically significant effect on total compensation relative to their control counterparts.

Taken together, the findings presented in this and the previous section indicate that the bonus cap policy affected not only the composition of pay but also the level of compensation for the most talented sample of treated workers.[22] Specifically, we show that—regardless of whether talent is defined using absolute earnings or estimated worker fixed effects, high-ability workers in the treatment group experience more negative effects on total earnings as a result of the bonus cap policy compared to their control counterparts and to lower-talented workers.

4.3 Robustness checks

In this section, we assess the robustness of our results along two dimensions. First, we examine the sensitivity of our baseline empirical approach to alternative sample selections and model specifications. Specifically, for each of the four pay components

[22] In a robustness check, we also analyze the impact of the policy on hours worked. We do not find evidence that hours constitute a relevant margin of adjustment.

analyzed in the previous sections, we re-estimate Equation (3) under the following modifications: (i) including the year in which a job spell ends; (ii) adding a vector of individual- and job-level control variables; (iii) clustering standard errors at the firm level. Second, we replicate our analysis using an alternative identification strategy that exploits only within-bank variation. We present the first set of robustness checks in Section 4.3.1, and the results from the within-bank strategy in Section 4.3.2.

4.3.1 Robustness checks: baseline specification

Include year of dissolution of job spell: As explained in Section 2.3, our baseline analysis excludes observations on pay from the year in which a job spell ends. This choice addresses the concern that our measure of variable pay may capture non-performance-related bonuses, such as severance payments, which are potentially paid upon job termination. Appendix Figure A3 compares the baseline estimates from our fully dynamic specification to those obtained when including the year of job termination in the analysis. The figure shows that, across all four components of the pay structure, the results remain both qualitatively and quantitatively similar.

Adding covariates: Our main specification includes job-spell fixed effects, which allow us to control for all time-invariant characteristics of the worker, the employer, and the match between the two. As a robustness check, we extend the specification by adding a set of time-varying controls at the individual and firm level. These include nine age categories, a dummy for being married or in a de facto relationship (versus being single), a dummy indicating whether the employee has at least one child, fixed effects for contract type (fixed-term, permanent, or other), and four firm size categories (1–19, 20–199, 200–499, and 500 or more employees). While we prefer to exclude these controls from our baseline specification—since some of them could be endogenously affected by the policy—the inclusion of these covariates serves as a useful robustness check, allowing us to assess whether our results hold when conditioning on a rich set of individual and firm characteristics. As shown in Appendix Figure A4, the results for all four components of the pay scheme remain both quantitatively and qualitatively similar to our baseline estimates.

Clustering standard errors: In light of the fact that the bonus cap policy applies only to workers employed in treated industries whose variable-to-fixed pay ratio exceeds 20%, we cluster standard errors at the job-spell level in our baseline specification. As a robustness check, we re-estimate our models using a more conservative approach in which standard errors are clustered at the firm level.[23] Despite the increase in the confidence intervals due to the significant reduction in the number of clusters - we

[23] An even more conservative approach would be to cluster at the 5-digit industry level; however, this is not feasible, as it would result in only 14 distinct clusters.

have only 202 distinct firms - Appendix Figure A5 shows that several of our estimates remain significant at the 5% or 10% level.

4.3.2 Robustness checks: within-bank specification

Thus far, our results are based on an identification strategy that compares changes in the pay structure of bankers to those of employees working in financial firms not subject to the bonus cap. A potential concern with this approach is that other shocks or regulations specific to the banking sector—rather than the bonus cap itself—could have negatively affected banks' performance and, in turn, employees' compensation. Indeed, in the aftermath of the financial crisis, the banking sector—more so than other parts of the financial industry—was subject to heightened regulatory scrutiny (e.g., through stricter capital requirements), and was disproportionately affected by the 2011–2013 sovereign debt crisis.

We argue, however, that this concern is not supported by the evidence presented in Section 4.1. While a deterioration in banks' performance due to concurrent sector-specific shocks could explain the observed decline in variable pay for bankers (as shown in Panel (b) of Figure 1), it would not account for the substantial increase in fixed pay displayed in Panel (c) of the same figure. These patterns are instead more consistent with the interpretation that banks adjusted their compensation structures in response to the bonus cap—reducing variable pay while increasing fixed pay to maintain overall pay levels for affected employees.

We further address this concern using an alternative empirical strategy that exploits only *within-bank* variation in workers' exposure to the policy. Focusing on the sample of incumbent employees in banks, we define a binary exposure variable that equals one for individuals whose variable-to-fixed pay ratio exceeded 20% at least once during the 2008–2011 period, and zero otherwise. We estimate the following model:

$$Y_{ift} = \alpha + \sum_{t=2008}^{2019} \delta_t \left(Exposure_{if} * Year_t \right) + \gamma_{if} + \phi_t + \epsilon_{ift} \tag{5}$$

The coefficient of interest is δ_t and it captures whether bankers who are expected to be more directly affected by the bonus cap policy exhibit differential trends in their pay structure compared to bankers whose compensation is less likely to be influenced by the regulation.

Results from the dynamic specification are shown in Figure A8 while aggregate DiD effects are shown in Table 4. Both output report results in line with our baseline specification, i.e. a decline in the relative importance of variable pay, which is largely compensated by an increase in the relative importance of fixed pay.

5 Effects on retention and hiring

Following the introduction of the bonus cap policy, banks expressed particular concern that the regulation would restrict their ability to retain and attract qualified employees. To assess the impact of the policy on worker flows, we begin by examining its aggregate effects on separations and hiring at the firm level. We then turn to an analysis of worker transitions at the individual level.

5.1 Firm-level analysis

In this section, we present the results obtained by estimating Equation (4) at the firm-level on the full sample of employees. To study worker flows, we define our outcomes as the cumulative number of joiners and the cumulative number of leavers relative to baseline employment, measured as the total number of employees observed in 2011. In other words, these outcomes expresses the total number of joiners or leavers since 2011 as a percentage of the number of employees a firm had in 2011. To assess overall changes in workforce size, we measure the ratio of total yearly employment to baseline employment. These outcomes are presented in Panels (a), (b), and (c) of Figure 3, respectively.

Figure 3 shows that, compared to control firms, banks experienced a significant slowdown in both hiring and separation rates following the introduction of the policy. The cumulative number of joiners at banks began to diverge from that of control firms in 2013, with a gap of 12.5 percentage points, reaching 63.7 percentage points by 2019. A similar pattern emerges for separations: starting in 2014, the cumulative number of separations at banks is 10.5 percentage points lower, a difference that widens to 52.8 percentage points by 2019. Importantly, while the reduction in separations positively affects staff size at banks relative to the control group, the decline in hiring is even more pronounced. Because the absolute reduction in separations is smaller than the reduction in joiners, the total staff size relative to baseline levels declines more for banks than for control firms, as shown in Panel (C).

Figure **??** distinguishes between voluntary and involuntary separations, where the latter are defined as separations in which, within 365 days of termination, the worker is observed to either enter unemployment or retire early (i.e., before age 65). Taken together, the figure shows that the overall slowdown in separations at banks relative to control firms, previously documented in Panel (b) of Figure 3, is primarily driven by a decline in voluntary separations. After the announcement of the bonus cap policy, voluntary exits grow at a slower rate in banks than in control firms. In contrast, there is some indication that involuntary separations initially increase more rapidly at banks in the years immediately following the policy announcement, but then stabilize, with the difference in cumulative involuntary separations between treated and control firms

remaining statistically insignificant during the post-implementation period.

5.2 Individual-level analysis on retention

We analyze employee retention and attraction at the individual level to assess whether talented bank employees are disproportionately affected by the policy, and whether it creates an uneven playing field for talent across treated and control firms.

Table 5 presents results on whether the policy differentially affected the probability that incumbent workers leave the firm in treated versus control industries. The analysis further distinguishes separations into voluntary (including and excluding early retirement) and involuntary, where the latter refers to individuals who remain non-employed for the entire 365-day period following job-spell dissolution.

The 2011 means are provided to give context on the incidence of exit states and are based on the full sample instead of the analysis sample. Columns (3) and (4) are mutually exclusive and add up to Column (1).

Panel A of Table 5 shows that incumbent employees at banks were 2 percentage points more likely to leave their job compared to those in control firms. However, this higher separation rate is primarily driven by involuntary separations and early retirement. In contrast, when early retirement is excluded from the definition of voluntary separations, the remaining voluntary separation rate is similar between bank and control employees (see columns (2), (3), and (4) of Panel A). Note however that, during the post-announcement period from 2012-2015, voluntary separations excluding early retirement were relatively high at banks compared to controls, but this did not continue after 2016.

These results are broadly consistent across all ability groups, regardless of whether talent is measured using the absolute or relative definition (see Panels B-F). Taken together, the analysis of separations presented here and in the previous section suggests that, although banks experienced higher separation rates overall, these were primarily driven by involuntary separations, i.e. separations followed by either prolonged unemployment or early retirement. In contrast, we find no evidence of an increase in voluntary separations (excluding early retirement) among bank employees compared to controls following the policy.

Table 6 provides a more granular view of the effects of the bonus cap policy by examining specific exit states observed within 365 days of job-spell dissolution. Columns (1) and (3) represent involuntary separations, while columns (2) and (4) through (11) capture voluntary separations. Panel A shows that, across all incumbents, employees at banks were more likely than those in control sectors to experience unemployment (0.6 percentage points), early retirement (1.6 percentage points), and transitions to a different industry (1.3 percentage points) following job-spell dissolution. In contrast, bankers were less likely to change treatment status within our sample, i.e. to move

from a bank to a control sector within the financial industry (–0.9 percentage points; Column (7)).

Panels B–F reveal several noteworthy patterns across subgroups. First, early retirement rates are consistently higher for bankers than for control employees across all groups. This effect is particularly pronounced among the most talented workers—for example, employees in the top 5% of the worker fixed effect distribution are 6.5 percentage points more likely to retire early than their counterparts in the control group (Panel F). Second, transitions to other industries (Column (4)) are more common among bankers, but this effect is concentrated among less talented workers—specifically, those with relatively low baseline pay (Panel B) and those outside the top 5% of the worker fixed effect distribution (Panel E). Third, we find no evidence that bankers are more likely than control employees to pursue strategic job-to-job moves within the financial sector in order to escape the bonus cap—for example, by switching to a non-capped financial industry to regain access to higher variable pay. This is reflected in the lack of positive effects in Columns (7) and (8), which capture within-finance transitions that change bonus cap exposure. Fourth, differential exits into self-employment or migration abroad appear to be small in magnitude and statistically insignificant.

Taken together, these results do not indicate that banks faced greater difficulties in retaining talent as a result of the policy—not even among the most talented employees. This finding is consistent with two possible interpretations. First, the increase in fixed pay may have been sufficient to offset the reduction in bonuses, preventing dissatisfaction and voluntary departures—even among high-ability individuals, who are typically less risk-averse and more responsive to performance-based incentives. Second, reallocation across financial subsectors may not be straightforward. Even within the broader financial industry, different subsectors often require distinct skill sets, making it difficult for employees to transition from banking to other financial firms. This limited mobility may further reduce the likelihood of exits, even for workers who might have preferred a more bonus-oriented pay structure.

5.3 Individual-level analysis on hiring

We analyze whether it became more difficult for banks to attract employees and specifically talented employees.

Table 7 shows the results on banks' ability to attract employees and allows us to examine how hiring by banks, compared to control firms, changed over time. The sample of analysis, from 2008-2019, is based on the full sample. However, we retain only one observation per individual, which is the observation in which the individual was hired. The absolute measure of talent is based on the previous job, and therefore missing for a large share of workers, particularly those who come from self-employment

or abroad. For this reason, columns (8) and (9) are suppressed for panels B-D. The relative measure of talent is defined based on the worker fixed effects as discussed in 2.4.

For all employees together (Panel A), Table 7 shows that banks compared to control firms in the post-implementation period 2016-2019, relative to the pre-period 2008-2011, hired disproportionately individuals who were unemployed (Column (1)), came from outside the financial sector (Column (2)), came from a non-bonus-capped firm within the financial sector (columns (5) and (6)). In contrast, banks were less likely than control firms to hire from other bonus-capped firms within the financial sector (3.1 percentage points, Column (3)) and employees from abroad (18.3 percentage points, Column (9)). The descriptive means provide the opportunity to reflect on the size of these effects. The 18.3 percentage points difference in hiring from abroad reflects a doubling compared to the 14.4 percentage points difference between control firms and treated firms in 2011.

We are particularly interested in the extent to which the hiring of talent changed over the time since policy implementation. Panels B-F reveal a couple of interesting patterns. First, panels B, C and E show that new employees at banks compared to new employees at control firms were less likely to be previously employed at other bonus-capped firms within the financial sector (columns (3) and (4)), whereas they were more likely to be previously employed at non-bonus-capped firms within the financial sector (columns (5) and (6)). Second, Panels D and F show, unfortunately, that the number of 'talented' employees joining banks and control firms is too low to identify statistically significant impacts at this fine level. To overcome this problem, we use as outcome variables whether the previous job was a low-paying or high-paying job as per absolute measure of talent categories.

Table 8 shows results on this based on four mutually exclusive outcome variables. The table shows that banks hired 10.7 percentage points more individuals who earned between 50,000 and 100,000 euro than control firms (see Column (1)). Relative to the difference between banks and control firms in hiring this group in 2011 (a 6.2 percentage point difference given the 56.5% for banks and 62.1% for control firms), this suggests that banks disproportionately attracted individuals who were relatively low-paid in their previous job. Indeed, columns (2) and (3) show that banks experienced a reduction in hiring individuals who earned between 100,000 and 175,000 euro or over 175,000 euro compared to the control group in the post-implementation period. Importantly, Column (4) shows no difference between banks and control firms in hiring individuals who have missing information for income of the previous job. This finding suggests there is no difference in hiring people who join from entry states such as self-employment, abroad, or being a graduate.

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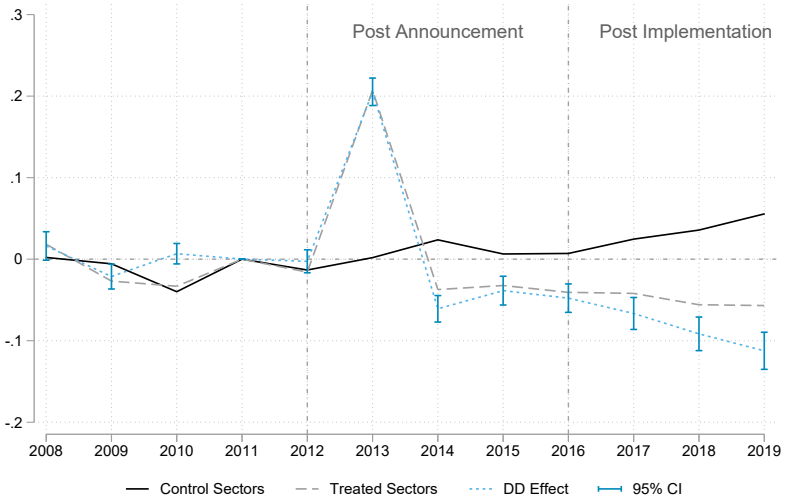
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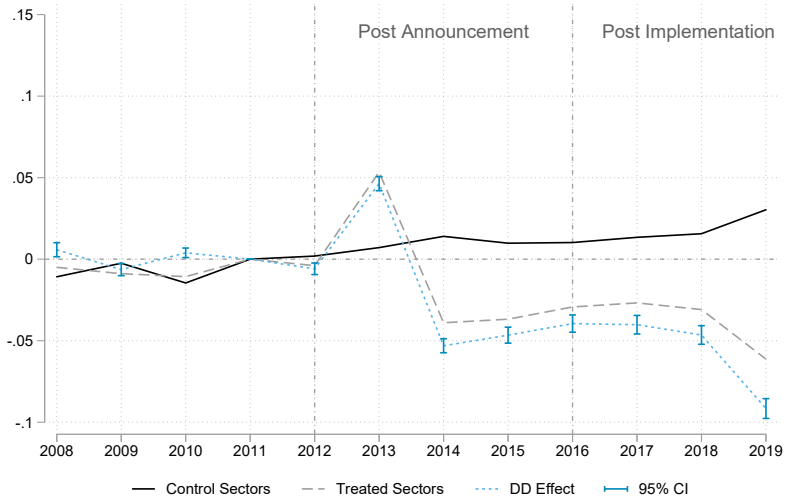
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Figures

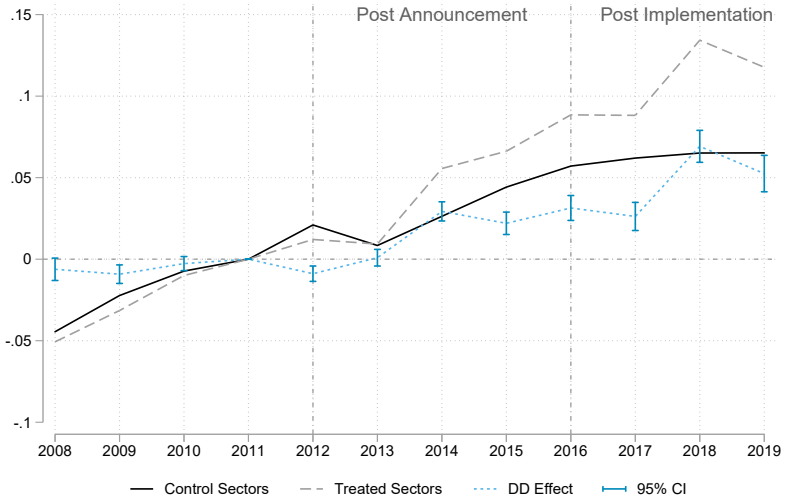
Figure 1: Effects on pay components - All workers



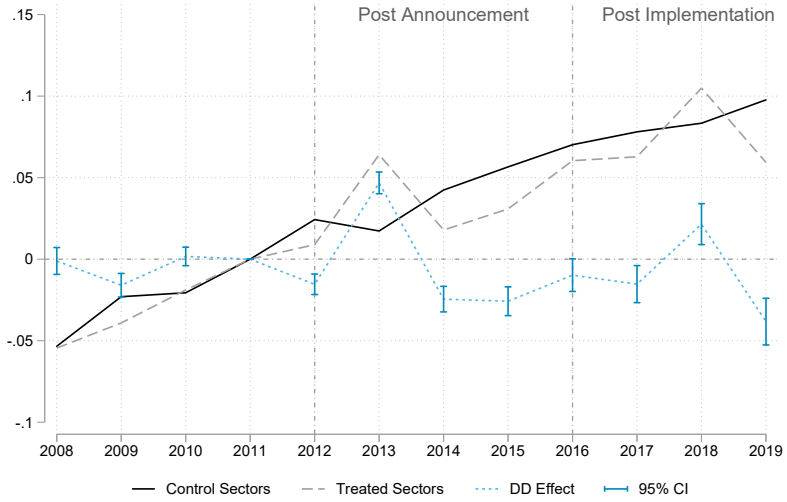
(a) Variable to fix pay ratio above 20%



(b) Variable pay over baseline total pay



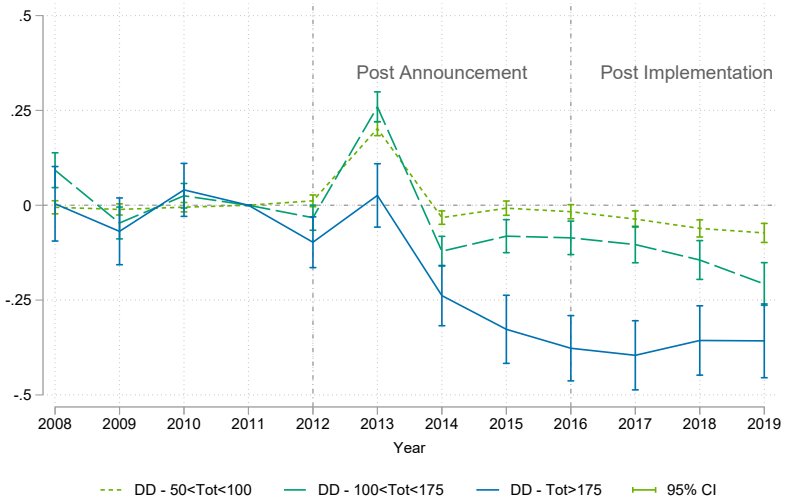
(c) Fixed pay over baseline total pay



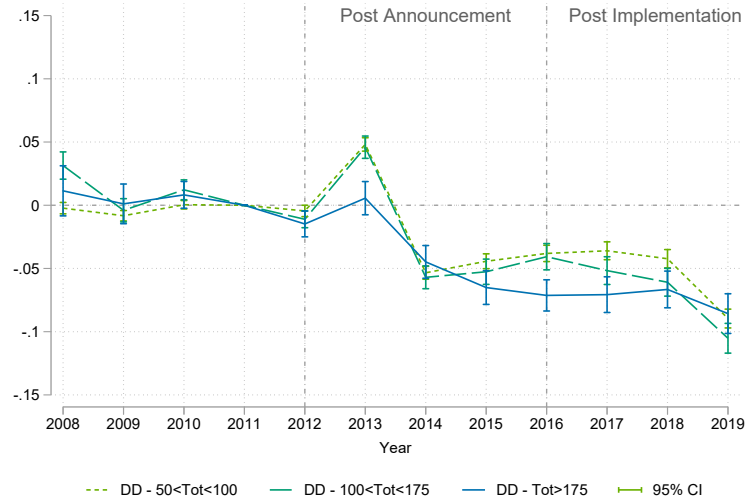
(d) Total pay over baseline total pay

Notes

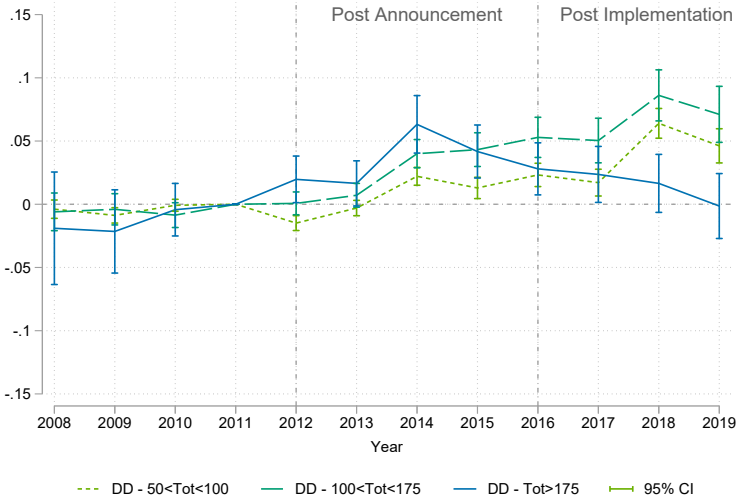
Figure 2: Effects on pay components - Heterogeneity analysis by workers' baseline pay



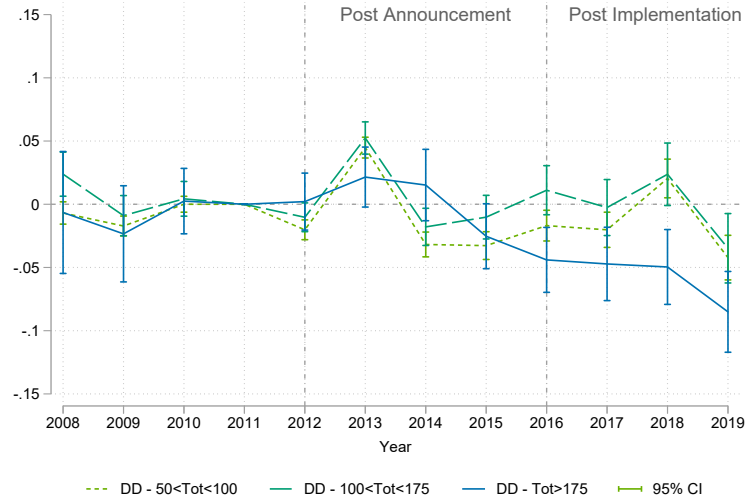
(a) Variable to fix pay ratio above 20%



(b) Variable pay over baseline total pay



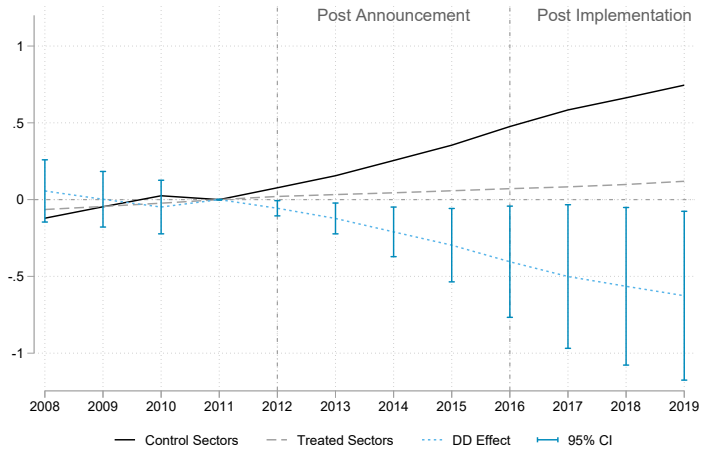
(c) Fixed pay over baseline total pay



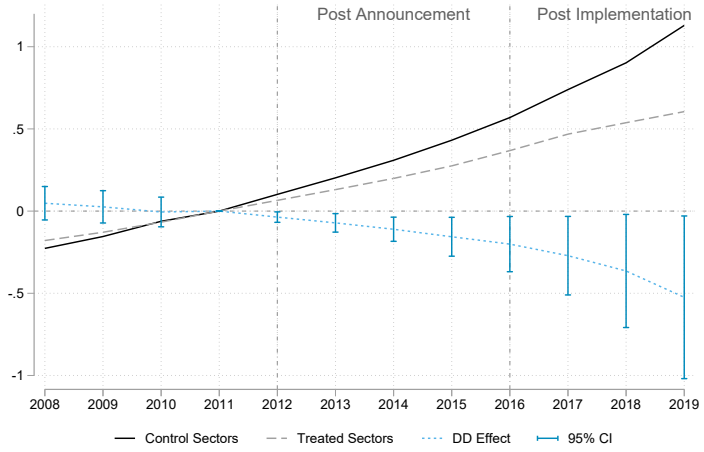
(d) Total pay over baseline total pay

Notes

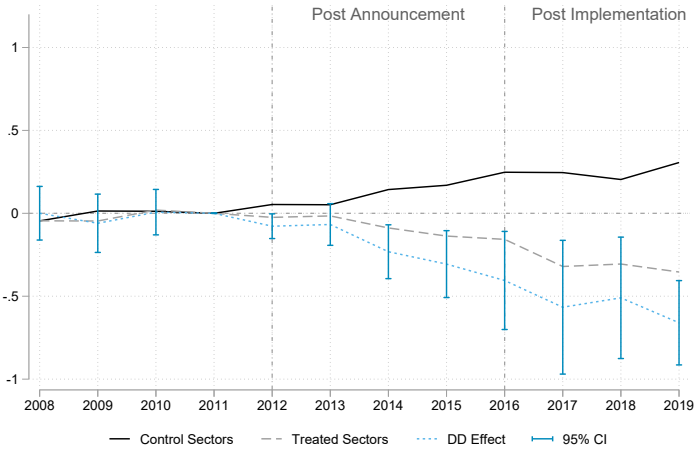
Figure 3: Worker flows at the firm level - full sample



(a) Cumulative hiring over baseline staff



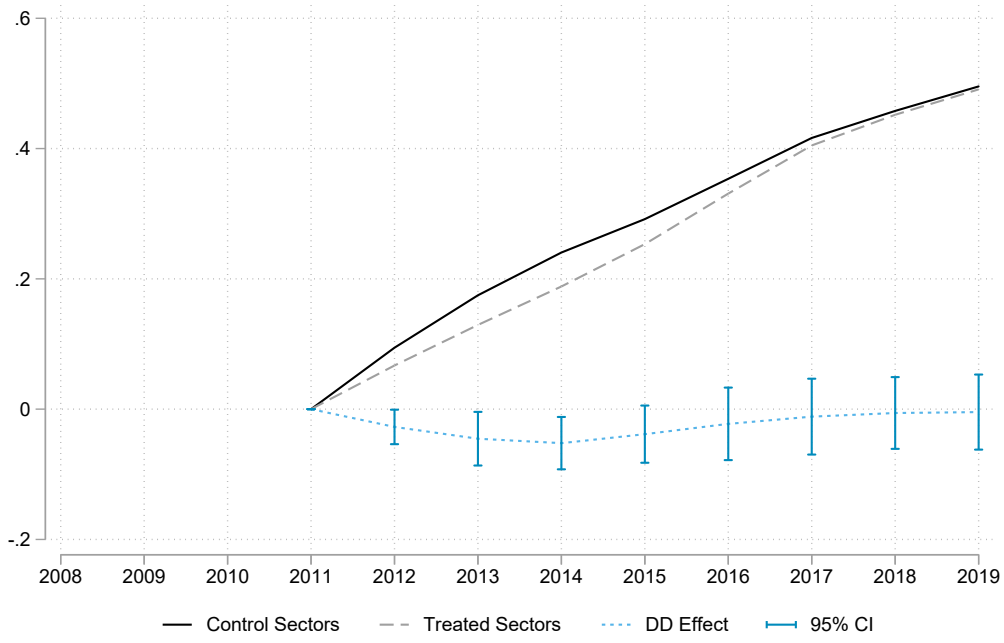
(b) Cumulative separations over baseline staff



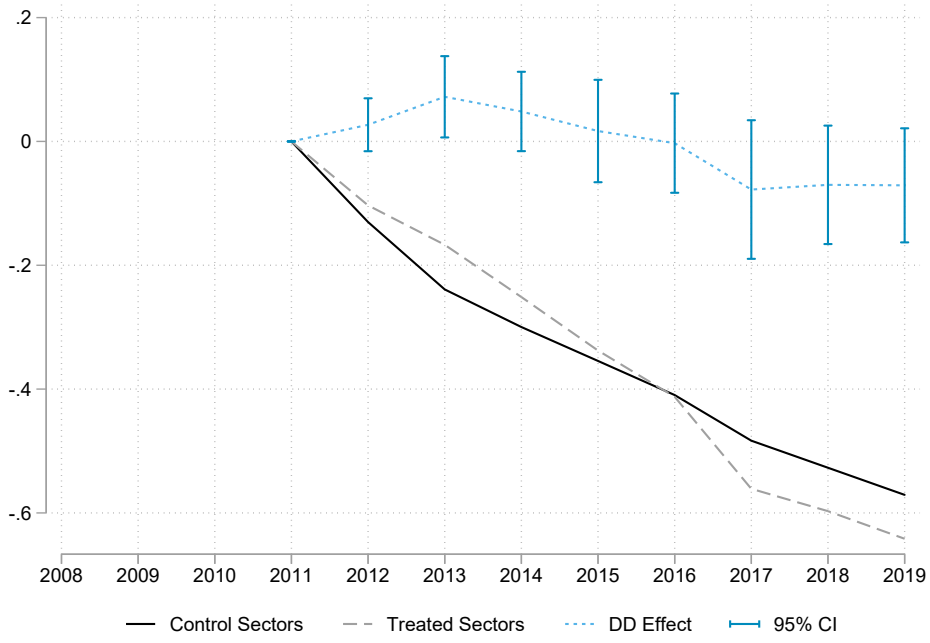
(c) Total staff over baseline staff

Notes.

Figure 4: Workers flows at the firm level - Incumbents sample



(a) Cumulative separations over incumbent baseline staff



(b) Total staff over incumbent baseline staff

Notes.

Tables

Table 1: Descriptive statistics (incumbents sample)

	(1) Treated All	(2) Control All	(3) Treated 50<Tot≤100	(4) Control 50<Tot≤100	(5) Treated 100<Tot≤175	(6) Control 100<Tot≤175	(7) Treated Tot>175	(8) Control Tot>175	(9) Treated < 95% FE	Control < 95% FE	Treated Top 5% FE	Control Top 5% F
A: Pay composition												
Variable pay (th. €)	8.329	10.071	5.226	5.426	13.755	16.784	30.998	33.673	7.483	9.379	17.942	15.705
	(9.163)	(11.788)	(4.197)	(5.919)	(10.383)	(13.107)	(13.071)	(12.007)	(8.150)	(11.153)	(13.520)	(14.921)
Variable pay in cash (th. €)	8.316	10.004	5.219	5.380	13.735	16.729	30.940	33.352	7.471	9.322	17.928	15.562
	(9.151)	(11.749)	(4.197)	(5.920)	(10.370)	(13.116)	(13.056)	(11.958)	(8.137)	(11.135)	(13.509)	(14.778)
Variable pay in stocks (th. €)	0.013	0.067	0.007	0.046	0.020	0.055	0.058	0.321	0.012	0.058	0.014	0.144
	(0.117)	(0.312)	(0.077)	(0.257)	(0.152)	(0.269)	(0.289)	(0.657)	(0.115)	(0.287)	(0.136)	(0.461)
Fixed pay (th. €)	77.248	79.702	63.110	63.048	108.091	106.787	155.766	153.850	72.524	75.969	130.945	110.124
	(30.354)	(31.822)	(13.502)	(13.331)	(21.792)	(22.138)	(21.706)	(20.384)	(25.461)	(28.925)	(29.801)	(37.565)
Total pay (th. €)	85.577	89.773	68.336	68.473	121.845	123.571	186.765	187.523	80.007	85.348	148.887	125.829
	(36.357)	(39.133)	(14.438)	(14.127)	(23.654)	(22.195)	(24.264)	(22.092)	(30.523)	(35.546)	(37.357)	(47.538)
Variable over fixed pay (%)	0.105	0.120	0.088	0.091	0.138	0.174	0.208	0.226	0.102	0.119	0.138	0.130
	(0.106)	(0.129)	(0.091)	(0.109)	(0.126)	(0.155)	(0.115)	(0.098)	(0.104)	(0.130)	(0.115)	(0.117)
Variable over fixed pay > 20%	0.105	0.200	0.046	0.108	0.186	0.322	0.623	0.712	0.092	0.190	0.254	0.288
	(0.306)	(0.400)	(0.210)	(0.310)	(0.389)	(0.467)	(0.485)	(0.454)	(0.289)	(0.392)	(0.435)	(0.453)
Variable over total pay (%)	0.089	0.097	0.077	0.077	0.113	0.134	0.167	0.179	0.087	0.096	0.115	0.106
	(0.076)	(0.090)	(0.066)	(0.078)	(0.087)	(0.103)	(0.082)	(0.067)	(0.075)	(0.090)	(0.084)	(0.089)
Fixed over total pay (%)	0.910	0.903	0.923	0.923	0.887	0.866	0.832	0.821	0.913	0.904	0.885	0.894
	(0.078)	(0.090)	(0.068)	(0.078)	(0.089)	(0.103)	(0.085)	(0.067)	(0.076)	(0.090)	(0.087)	(0.089)
Total hours (Annual #)	1907.755	2006.009	1883.059	2000.378	1968.973	2018.275	2015.137	2020.271	1902.622	2007.250	1966.102	1995.898
	(201.428)	(148.016)	(193.511)	(149.374)	(199.904)	(135.690)	(229.362)	(170.717)	(198.512)	(143.071)	(223.917)	(183.328)
B: Workforce												
Female	0.251	0.168	0.296	0.201	0.133	0.099	0.077	0.079	0.267	0.179	0.070	0.079

	(0.433)	(0.374)	(0.456)	(0.400)	(0.339)	(0.299)	(0.267)	(0.270)	(0.442)	(0.383)	(0.255)	(0.271)
Age (#)	44.786	45.335	43.894	44.616	46.859	46.642	49.237	48.056	43.910	44.358	54.741	53.299
	(8.844)	(9.364)	(9.081)	(9.649)	(7.662)	(8.693)	(6.889)	(7.556)	(8.561)	(9.099)	(5.136)	(7.522)
Low-educated	0.014	0.020	0.016	0.026	0.006	0.010	0.007	0.000	0.013	0.017	0.031	0.076
	(0.116)	(0.141)	(0.126)	(0.159)	(0.075)	(0.098)	(0.084)	(0.000)	(0.113)	(0.128)	(0.174)	(0.267)
Average-educated	0.204	0.244	0.227	0.287	0.136	0.162	0.144	0.104	0.197	0.236	0.406	0.371
	(0.403)	(0.430)	(0.419)	(0.452)	(0.343)	(0.369)	(0.352)	(0.306)	(0.398)	(0.425)	(0.491)	(0.486)
High-educated	0.782	0.735	0.757	0.687	0.859	0.829	0.848	0.896	0.790	0.748	0.563	0.552
	(0.413)	(0.441)	(0.429)	(0.464)	(0.349)	(0.377)	(0.359)	(0.306)	(0.407)	(0.435)	(0.496)	(0.500)
Native	0.930	0.904	0.929	0.903	0.937	0.911	0.921	0.891	0.929	0.898	0.947	0.951
	(0.255)	(0.295)	(0.257)	(0.296)	(0.242)	(0.285)	(0.270)	(0.313)	(0.257)	(0.303)	(0.223)	(0.217)
50<Tot≤100	0.740	0.693	1.000	1.000	0.000	0.000	0.000	0.000	0.801	0.743	0.047	0.288
	(0.439)	(0.461)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.399)	(0.437)	(0.211)	(0.453)
100<Tot≤175	0.208	0.238	0.000	0.000	1.000	1.000	0.000	0.000	0.172	0.210	0.617	0.471
	(0.406)	(0.426)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.378)	(0.407)	(0.486)	(0.500)
Tot>175	0.051	0.069	0.000	0.000	0.000	0.000	1.000	1.000	0.026	0.047	0.336	0.241
	(0.221)	(0.253)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.160)	(0.213)	(0.473)	(0.428)
Top 10pct FE	0.174	0.219	0.039	0.112	0.501	0.431	0.778	0.563	0.101	0.123	1.000	1.000
	(0.379)	(0.414)	(0.194)	(0.316)	(0.500)	(0.496)	(0.416)	(0.497)	(0.301)	(0.329)	(0.000)	(0.000)
Top 5pct FE	0.081	0.109	0.005	0.045	0.239	0.216	0.529	0.384	0.000	0.000	1.000	1.000
	(0.273)	(0.312)	(0.071)	(0.208)	(0.427)	(0.412)	(0.499)	(0.487)	(0.000)	(0.000)	(0.000)	(0.000)
Average worker F.E.	0.650	0.723	0.508	0.563	1.016	1.045	1.207	1.229	0.592	0.664	1.308	1.207
	(0.371)	(0.400)	(0.297)	(0.323)	(0.238)	(0.314)	(0.191)	(0.262)	(0.327)	(0.369)	(0.123)	(0.306)
N of job spells in 2011	38,603	3,339	28,573	2,314	8,045	796	1,985	229	35,481	2,974	3,122	365

[1] *Notes.* Notes: Sample means for compensation variables and individual characteristics are provided for 2011 by treated and control group. The variable to fixed pay ratio indicates the average ratio of variable to fixed pay, while the 90th percentile indicates its 90th percentile value. Ratio over 20% indicates the share of employees whose ratio is above 20%, which is the bonus cap imposed by the policy. Educational attainment is categorized into lower, secondary, and

tertiary education following the International Standard Classification of Education.

Table 2: Descriptive statistics (firm sample)

	All - Treated	All - Control
C: Firm Characteristics		
General banks	0.860	n/a
Other banks	0.140	n/a
Risk and damage evaluation	n/a	0.221
Participation companies	n/a	0.214
Investment funds in financial assets	n/a	0.156
Investment funds in real estate	n/a	0.130
Other controls	n/a	0.104
Investment funds with restricted entry	n/a	0.091
Pension consultancy and funds	n/a	0.084
Firm size (#)	894.907	22.584
10-49 employees	0.093	0.539
50-99 employees	0.535	0.416
100+ employees	0.372	0.045
Number of unique firms in 2011	43	154

[1] *Notes*. This table shows the share of firms operating within the different treated and control sectors included in our analysis. Sectors are defined at the 5-digit level following the Dutch Industry Classification (Standaard Bedrijfsindeling; SBI).

Table 3: Payid

	(1) Var_t / Fix_t > 20%	(2) Var_t / Tot_{2011}	(3) Fix_t / Tot_{2011}	(4) Tot_t / Tot_{2011}
A: All incumbents				
Post Ann. (2012-15)	0.014**	0.014***	0.039***	0.053***
	(0.006)	(0.002)	(0.002)	(0.003)
Post Impl. (2016-19)	0.038***	0.022***	0.076***	0.100***
	(0.008)	(0.002)	(0.004)	(0.005)
T × Post Ann. (2012-15)	0.031***	-0.012***	0.014***	0.002
	(0.006)	(0.002)	(0.002)	(0.003)
T × Post Impl. (2016-19)	-0.073***	-0.050***	0.046***	-0.005
	(0.008)	(0.003)	(0.004)	(0.006)
N	348809	348809	348809	348809
Adj R2	0.319	0.300	0.481	0.477
Mean C Group 2011	0.200	0.098	0.902	1.000
Mean T Group 2011	0.104	0.090	0.910	1.000
B: 50<Tot≤100				
Post Ann. (2012-15)	0.004	0.014***	0.049***	0.065***
	(0.007)	(0.002)	(0.003)	(0.004)
Post Impl. (2016-19)	0.030***	0.024***	0.095***	0.120***
	(0.009)	(0.003)	(0.005)	(0.007)
T × Post Ann. (2012-15)	0.052***	-0.008***	0.007**	-0.001
	(0.007)	(0.002)	(0.003)	(0.004)
T × Post Impl. (2016-19)	-0.036***	-0.045***	0.038***	-0.008
	(0.009)	(0.003)	(0.005)	(0.007)
N	259142	259142	259142	259142
Adj R2	0.155	0.229	0.473	0.493
Mean C Group 2011	0.108	0.078	0.922	1.000
Mean T Group 2011	0.046	0.078	0.922	1.000
C: 100<Tot≤175				
Post Ann. (2012-15)	0.033**	0.015***	0.025***	0.040***
	(0.014)	(0.003)	(0.004)	(0.006)
Post Impl. (2016-19)	0.047**	0.021***	0.045***	0.067***
	(0.019)	(0.005)	(0.008)	(0.011)
T × Post Ann. (2012-15)	-0.001	-0.023***	0.026***	0.003
	(0.014)	(0.003)	(0.005)	(0.006)
T × Post Impl. (2016-19)	-0.138***	-0.067***	0.067***	-0.000
	(0.019)	(0.005)	(0.009)	(0.011)
N	72492	72492	72492	72492
Adj R2	0.284	0.342	0.464	0.430
Mean C Group 2011	0.319	0.134	0.866	1.000
Mean T Group 2011	0.184	0.113	0.887	1.000
D: Tot>175				
Post Ann. (2012-15)	0.058*	0.005	-0.033***	-0.025**
	(0.029)	(0.005)	(0.009)	(0.011)
Post Impl. (2016-19)	0.103***	0.012**	-0.028**	-0.014
	(0.034)	(0.006)	(0.011)	(0.013)
T × Post Ann. (2012-15)	-0.132***	-0.030***	0.041***	0.009
	(0.031)	(0.005)	(0.010)	(0.011)
T × Post Impl. (2016-19)	-0.356***	-0.074***	0.027**	-0.047***

	(0.037)	(0.006)	(0.012)	(0.014)
N	17175	17175	17175	17175
Adj R2	0.400	0.423	0.459	0.487
Mean C Group 2011	0.705	0.178	0.822	1.000
Mean T Group 2011	0.620	0.167	0.833	1.000
E: Bottom 95pct FE				
Post Ann. (2012-15)	0.020***	0.016***	0.044***	0.061***
	(0.006)	(0.002)	(0.002)	(0.003)
Post Impl. (2016-19)	0.045***	0.025***	0.087***	0.113***
	(0.008)	(0.003)	(0.004)	(0.006)
T × Post Ann. (2012-15)	0.027***	-0.013***	0.013***	-0.000
	(0.006)	(0.002)	(0.003)	(0.004)
T × Post Impl. (2016-19)	-0.073***	-0.051***	0.043***	-0.009
	(0.008)	(0.003)	(0.005)	(0.006)
N	323082	323082	323082	323082
N job spells	37768	37768	37768	37768
Adj R2	0.295	0.290	0.488	0.488
Mean C Group 2011	0.187	0.097	0.903	1.000
Mean T Group 2011	0.091	0.087	0.912	1.000
Job spell F.E.	Yes	Yes	Yes	Yes
F: Top 5pct FE				
Post Ann. (2012-15)	-0.033	-0.002	-0.005	-0.006
	(0.020)	(0.004)	(0.007)	(0.007)
Post Impl. (2016-19)	-0.023	-0.004	-0.024*	-0.025*
	(0.030)	(0.006)	(0.012)	(0.013)
T × Post Ann. (2012-15)	0.051**	-0.014***	0.007	-0.008
	(0.021)	(0.004)	(0.007)	(0.008)
T × Post Impl. (2016-19)	-0.126***	-0.061***	0.029**	-0.032**
	(0.031)	(0.006)	(0.013)	(0.014)
N	25727	25727	25727	25727
Adj R2	0.399	0.393	0.382	0.373
Mean C Group 2011	0.292	0.105	0.895	1.000
Mean T Group 2011	0.252	0.115	0.884	1.000

[1] *Notes*. All specifications include job-spell fixed-effects.

Table 4: Within-bank specification

	(1) Var_t / Fix_t > 20%	(2) Var_t / Tot_{2011}	(3) Fix_t / Tot_{2011}	(4) Tot_t / Tot_{2011}
A: All incumbents				
Post Ann. (2012-15)	-0.000	-0.002***	0.048***	0.047***
	(.)	(0.000)	(0.001)	(0.001)
Post Impl. (2016-19)	0.000	-0.016***	0.119***	0.103***
	(.)	(0.001)	(0.002)	(0.002)
T × Post Ann. (2012-15)	0.084	0.007***	0.009***	0.016***
	(.)	(0.001)	(0.001)	(0.001)
T × Post Impl. (2016-19)	-0.070	-0.023***	0.007***	-0.016***
	(.)	(0.001)	(0.003)	(0.003)
N	323084	323084	323084	323084
N job spells	38032	38032	38032	38032
Adj R2	0.295	0.265	0.478	0.481
Mean C Group 2011	0.000	0.062	0.938	1.000
Mean T Group 2011	0.193	0.113	0.886	1.000
Job spell F.E.	Yes	Yes	Yes	Yes
B: 50<Tot≤100				
Post Ann. (2012-15)	-0.000	-0.000	0.051***	0.050***
	(.)	(0.000)	(0.001)	(0.001)
Post Impl. (2016-19)	0.000	-0.015***	0.125***	0.110***
	(.)	(0.001)	(0.002)	(0.002)
T × Post Ann. (2012-15)	0.122	0.015***	0.012***	0.027***
	(.)	(0.001)	(0.002)	(0.002)
T × Post Impl. (2016-19)	-0.019	-0.015***	0.019***	0.005
	(.)	(0.001)	(0.003)	(0.003)
N	241074	241074	241074	241074
N job spells	28144	28144	28144	28144
Adj R2	0.137	0.189	0.471	0.497
Mean C Group 2011	0.000	0.061	0.939	1.000
Mean T Group 2011	0.098	0.097	0.903	1.000
Job spell F.E.	Yes	Yes	Yes	Yes
C: 100<Tot≤175				
Post Ann. (2012-15)	0.000	-0.011***	0.034***	0.023***
	(.)	(0.001)	(0.002)	(0.003)
Post Impl. (2016-19)	0.000	-0.024***	0.086***	0.062***
	(.)	(0.001)	(0.004)	(0.005)
T × Post Ann. (2012-15)	0.046	0.003***	0.024***	0.027***
	(.)	(0.001)	(0.003)	(0.003)
T × Post Impl. (2016-19)	-0.130	-0.031***	0.037***	0.006
	(.)	(0.002)	(0.005)	(0.006)
N	66502	66502	66502	66502
N job spells	7924	7924	7924	7924
Adj R2	0.255	0.315	0.457	0.434
Mean C Group 2011	0.000	0.068	0.932	1.000
Mean T Group 2011	0.261	0.132	0.867	1.000
Job spell F.E.	Yes	Yes	Yes	Yes
D: Tot>175				
Post Ann. (2012-15)	-0.000	-0.018***	-0.017	-0.035***
	(.)	(0.003)	(0.011)	(0.012)
Post Impl. (2016-19)	-0.000	-0.033***	-0.031	-0.062***

	(.)	(0.007)	(0.020)	(0.021)
T × Post Ann. (2012-15)	-0.079	-0.007*	0.026**	0.021*
	(.)	(0.004)	(0.012)	(0.012)
T × Post Impl. (2016-19)	-0.264	-0.031***	0.031	0.000
	(.)	(0.008)	(0.020)	(0.022)
N	15508	15508	15508	15508
N job spells	1964	1964	1964	1964
Adj R2	0.390	0.413	0.463	0.495
Mean C Group 2011	0.000	0.067	0.933	1.000
Mean T Group 2011	0.662	0.174	0.826	1.000
Job spell F.E.	Yes	Yes	Yes	Yes
E: Bottom 95pct FE				
Post Ann. (2012-15)	0.000	-0.001***	0.050***	0.049***
	(.)	(0.000)	(0.001)	(0.001)
Post Impl. (2016-19)	0.000	-0.016***	0.122***	0.107***
	(.)	(0.001)	(0.002)	(0.002)
T × Post Ann. (2012-15)	0.091	0.009***	0.013***	0.022***
	(.)	(0.001)	(0.001)	(0.002)
T × Post Impl. (2016-19)	-0.059	-0.020***	0.015***	-0.005*
	(.)	(0.001)	(0.003)	(0.003)
N	300214	300214	300214	300214
N job spells	34955	34955	34955	34955
Adj R2	0.270	0.254	0.485	0.492
Mean C Group 2011	0.000	0.062	0.938	1.000
Mean T Group 2011	0.175	0.111	0.889	1.000
Job spell F.E.	Yes	Yes	Yes	Yes
F: Top 5pct FE				
Post Ann. (2012-15)	-0.000	-0.013***	-0.010***	-0.023***
	(.)	(0.001)	(0.004)	(0.004)
Post Impl. (2016-19)	-0.000	-0.028***	-0.002	-0.029***
	(.)	(0.003)	(0.007)	(0.007)
T × Post Ann. (2012-15)	0.024	-0.004**	0.016***	0.012**
	(.)	(0.002)	(0.004)	(0.005)
T × Post Impl. (2016-19)	-0.187	-0.046***	0.009	-0.035***
	(.)	(0.004)	(0.008)	(0.009)
N	22870	22870	22870	22870
N job spells	3077	3077	3077	3077
Adj R2	0.386	0.370	0.370	0.373
Mean C Group 2011	0.000	0.060	0.940	1.000
Mean T Group 2011	0.333	0.133	0.867	1.000
Job spell F.E.	Yes	Yes	Yes	Yes

[1] *Notes*. All specifications include job-spell fixed-effects.

Table 5: Sepid

	(1) All Separations	(2) Voluntary	(3) Voluntary excl. early retirement	(4) Involuntary
A: All incumbents				
Post Ann. (2012-15)	0.094***	0.077***	0.068***	0.026***
	(0.003)	(0.003)	(0.002)	(0.002)
Post Impl. (2016-19)	0.097***	0.079***	0.071***	0.026***
	(0.004)	(0.003)	(0.003)	(0.002)
T × Post Ann. (2012-15)	-0.015***	-0.023***	-0.031***	0.015***
	(0.003)	(0.003)	(0.003)	(0.002)
T × Post Impl. (2016-19)	0.021***	0.015***	-0.001	0.022***
	(0.004)	(0.004)	(0.003)	(0.002)
N	269765	269765	269765	269765
N job spells	41619	41619	41619	41619
Adj R2	0.020	0.017	0.014	0.007
Mean C Group 2011	0.113	0.086	0.078	0.035
Mean T Group 2011	0.061	0.041	0.031	0.030
Job spell F.E.	No	No	No	No
B: 50<Tot≤100				
Post Ann. (2012-15)	0.088***	0.072***	0.064***	0.023***
	(0.003)	(0.003)	(0.003)	(0.002)
Post Impl. (2016-19)	0.091***	0.077***	0.070***	0.021***
	(0.004)	(0.004)	(0.004)	(0.002)
T × Post Ann. (2012-15)	-0.011***	-0.021***	-0.028***	0.018***
	(0.003)	(0.003)	(0.003)	(0.002)
T × Post Impl. (2016-19)	0.026***	0.016***	0.001	0.025***
	(0.004)	(0.004)	(0.004)	(0.002)
N	200505	200505	200505	200505
N job spells	30641	30641	30641	30641
Adj R2	0.019	0.017	0.014	0.007
Mean C Group 2011	0.077	0.064	0.055	0.021
Mean T Group 2011	0.056	0.037	0.028	0.028
Job spell F.E.	No	No	No	No
C: 100<Tot≤175				
Post Ann. (2012-15)	0.106***	0.086***	0.075***	0.030***
	(0.006)	(0.006)	(0.005)	(0.003)
Post Impl. (2016-19)	0.106***	0.086***	0.075***	0.031***
	(0.008)	(0.007)	(0.007)	(0.005)
T × Post Ann. (2012-15)	-0.026***	-0.029***	-0.035***	0.009**
	(0.006)	(0.006)	(0.006)	(0.004)
T × Post Impl. (2016-19)	0.011	0.009	-0.006	0.017***
	(0.009)	(0.008)	(0.007)	(0.005)
N	56024	56024	56024	56024
N job spells	8773	8773	8773	8773
Adj R2	0.020	0.017	0.013	0.007
Mean C Group 2011	0.072	0.060	0.054	0.018
Mean T Group 2011	0.062	0.043	0.034	0.029
Job spell F.E.	No	No	No	No
D: Tot>175				
Post Ann. (2012-15)	0.117***	0.095***	0.084***	0.033***
	(0.012)	(0.011)	(0.011)	(0.007)

Post Impl. (2016-19)	0.134***	0.083***	0.073***	0.061***
	(0.017)	(0.013)	(0.013)	(0.012)
T × Post Ann. (2012-15)	-0.021*	-0.025**	-0.035***	0.014*
	(0.013)	(0.012)	(0.011)	(0.007)
T × Post Impl. (2016-19)	0.015	0.035**	0.007	0.008
	(0.018)	(0.015)	(0.014)	(0.013)
N	13236	13236	13236	13236
N job spells	2205	2205	2205	2205
Adj R2	0.027	0.021	0.015	0.012
Mean C Group 2011	0.084	0.064	0.060	0.024
Mean T Group 2011	0.081	0.049	0.037	0.044
Job spell F.E.	No	No	No	No
E: Bottom 95pct FE				
Post Ann. (2012-15)	0.089***	0.073***	0.067***	0.023***
	(0.003)	(0.003)	(0.003)	(0.002)
Post Impl. (2016-19)	0.092***	0.075***	0.069***	0.023***
	(0.004)	(0.003)	(0.003)	(0.002)
T × Post Ann. (2012-15)	-0.015***	-0.023***	-0.029***	0.014***
	(0.003)	(0.003)	(0.003)	(0.002)
T × Post Impl. (2016-19)	0.022***	0.015***	0.001	0.021***
	(0.004)	(0.004)	(0.003)	(0.002)
N	250475	250475	250475	250475
N job spells	38156	38156	38156	38156
Adj R2	0.019	0.016	0.013	0.007
Mean C Group 2011	0.116	0.087	0.081	0.034
Mean T Group 2011	0.056	0.038	0.031	0.025
Job spell F.E.	No	No	No	No
F: Top 5pct FE				
Post Ann. (2012-15)	0.134***	0.109***	0.084***	0.051***
	(0.010)	(0.010)	(0.008)	(0.007)
Post Impl. (2016-19)	0.160***	0.121***	0.096***	0.064***
	(0.015)	(0.014)	(0.012)	(0.010)
T × Post Ann. (2012-15)	-0.002	-0.010	-0.040***	0.038***
	(0.011)	(0.010)	(0.009)	(0.007)
T × Post Impl. (2016-19)	0.035**	0.045***	-0.020	0.054***
	(0.016)	(0.015)	(0.013)	(0.012)
N	19290	19290	19290	19290
N job spells	3463	3463	3463	3463
Adj R2	0.036	0.032	0.016	0.022
Mean C Group 2011	0.089	0.074	0.046	0.043
Mean T Group 2011	0.116	0.072	0.033	0.083
Job spell F.E.	No	No	No	No

[1] *Notes*. All specifications include job-spell fixed-effects.

Table 6: Sepid Full matrix

	(1) Invol. Unemploym.	(2) Retire (Norm)	(3) Retire Early	(4) Change Industry	(5) Fin - Same Status (T vs C)	(6) Fin - Same Status (Oth)	(7) Fin - Switch Status (T vs C)	(8) Fin - Switch Status (Oth)	(9) Fin Resid	(10) Self-empl.	(11) Abroad
A: All incumbents											
Post Ann. (2012-15)	0.017***	0.006***	0.008***	0.030***	0.001***	0.010***	0.009***	0.002***	0.000**	0.006***	0.004***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
Post Impl. (2016-19)	0.018***	0.009***	0.008***	0.028***	0.001***	0.011***	0.010***	0.001***	0.000*	0.007***	0.002***
	(0.002)	(0.001)	(0.001)	(0.002)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
T × Post Ann. (2012-15)	0.008***	-0.004***	0.007***	-0.012***	0.003***	-0.005***	-0.009***	-0.001	0.000	-0.000	-0.002***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
T × Post Impl. (2016-19)	0.006***	-0.003***	0.016***	0.013***	0.003***	-0.007***	-0.009***	0.001	0.001***	0.001	-0.000
	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
N	269765	269765	269765	269765	269765	269765	269765	269765	269765	269765	269765
N job spells	41619	41619	41619	41619	41619	41619	41619	41619	41619	41619	41619
Adj R2	0.004	0.002	0.004	0.009	0.001	0.001	0.006	0.000	0.000	0.001	0.000
Mean C Group 2011	0.027	0.002	0.008	0.042	0.000	0.006	0.011	0.004	0.000	0.008	0.005
Mean T Group 2011	0.021	0.000	0.009	0.014	0.002	0.006	0.000	0.001	0.001	0.004	0.003
Job spell F.E.	No	No	No	No	No	No	No	No	No	No	No
B: 50<Tot≤100											
Post Ann. (2012-15)	0.016***	0.007***	0.007***	0.030***	0.001**	0.009***	0.008***	0.001***	0.000	0.005***	0.003***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
Post Impl. (2016-19)	0.015***	0.010***	0.007***	0.028***	0.001**	0.012***	0.010***	0.001**	0.000	0.006***	0.002***
	(0.002)	(0.001)	(0.001)	(0.002)	(0.000)	(0.002)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
T × Post Ann. (2012-15)	0.010***	-0.006***	0.008***	-0.012***	0.003***	-0.005***	-0.008***	-0.000	0.000	-0.001	-0.001
	(0.002)	(0.001)	(0.001)	(0.002)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
T × Post Impl. (2016-19)	0.010***	-0.005***	0.015***	0.016***	0.004***	-0.007***	-0.010***	0.000	0.001***	0.001	0.000
	(0.002)	(0.001)	(0.001)	(0.003)	(0.001)	(0.002)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)
N	200505	200505	200505	200505	200505	200505	200505	200505	200505	200505	200505
N job spells	30641	30641	30641	30641	30641	30641	30641	30641	30641	30641	30641

Adj R2	0.004	0.002	0.004	0.009	0.001	0.001	0.006	0.000	0.000	0.001	0.000
Mean C Group 2011	0.012	0.001	0.009	0.028	0.000	0.004	0.009	0.002	0.000	0.005	0.004
Mean T Group 2011	0.019	0.000	0.009	0.013	0.002	0.006	0.000	0.001	0.000	0.004	0.002
Job spell F.E.	No	No	No	No	No	No	No	No	No	No	No
C: 100<Tot≤175											
Post Ann. (2012-15)	0.020***	0.003***	0.010***	0.032***	0.002**	0.012***	0.012***	0.003***	0.001	0.006***	0.006***
	(0.003)	(0.001)	(0.002)	(0.004)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
Post Impl. (2016-19)	0.020***	0.009***	0.012***	0.027***	0.002*	0.013***	0.007***	0.000	0.001	0.010***	0.005***
	(0.004)	(0.003)	(0.003)	(0.004)	(0.001)	(0.003)	(0.002)	(0.000)	(0.001)	(0.003)	(0.002)
T × Post Ann. (2012-15)	0.003	-0.001	0.006***	-0.015***	0.002**	-0.006***	-0.011***	-0.001	0.000	0.001	-0.003**
	(0.003)	(0.001)	(0.002)	(0.004)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
T × Post Impl. (2016-19)	0.002	-0.001	0.015***	0.006	0.003**	-0.007**	-0.007***	0.002***	0.001	0.002	-0.003
	(0.004)	(0.003)	(0.003)	(0.004)	(0.001)	(0.003)	(0.002)	(0.000)	(0.001)	(0.003)	(0.002)
N	56024	56024	56024	56024	56024	56024	56024	56024	56024	56024	56024
N job spells	8773	8773	8773	8773	8773	8773	8773	8773	8773	8773	8773
Adj R2	0.003	0.003	0.004	0.006	0.001	0.001	0.007	0.000	0.000	0.002	0.001
Mean C Group 2011	0.011	0.001	0.007	0.026	0.000	0.002	0.011	0.005	0.000	0.005	0.003
Mean T Group 2011	0.019	0.000	0.009	0.012	0.003	0.008	0.000	0.001	0.001	0.006	0.003
Job spell F.E.	No	No	No	No	No	No	No	No	No	No	No
D: Tot>175											
Post Ann. (2012-15)	0.021***	0.003	0.011***	0.030***	0.000	0.016***	0.011***	0.003	0.001	0.009**	0.011***
	(0.005)	(0.002)	(0.004)	(0.006)	(0.000)	(0.005)	(0.004)	(0.002)	(0.001)	(0.003)	(0.004)
Post Impl. (2016-19)	0.051***	0.002	0.010**	0.037***	0.005	0.002	0.015**	0.005	-0.000	0.007*	0.000
	(0.011)	(0.002)	(0.005)	(0.009)	(0.003)	(0.002)	(0.006)	(0.003)	(.)	(0.004)	(0.000)
T × Post Ann. (2012-15)	0.004	0.002	0.010**	-0.013*	0.002***	-0.008*	-0.010***	-0.000	-0.001	0.001	-0.008**
	(0.006)	(0.002)	(0.004)	(0.007)	(0.001)	(0.005)	(0.004)	(0.002)	(0.001)	(0.004)	(0.004)
T × Post Impl. (2016-19)	-0.020*	0.008***	0.028***	-0.002	-0.000	0.004	-0.013**	0.001	0.003***	0.006	0.001**
	(0.011)	(0.003)	(0.006)	(0.010)	(0.004)	(0.003)	(0.006)	(0.004)	(0.001)	(0.005)	(0.001)
N	13236	13236	13236	13236	13236	13236	13236	13236	13236	13236	13236
N job spells	2205	2205	2205	2205	2205	2205	2205	2205	2205	2205	2205
Adj R2	0.005	0.002	0.007	0.007	0.001	0.002	0.005	0.001	0.000	0.002	0.002
Mean C Group 2011	0.020	0.000	0.004	0.016	0.000	0.020	0.008	0.008	0.000	0.004	0.004
Mean T Group 2011	0.032	0.000	0.012	0.014	0.004	0.007	0.001	0.003	0.000	0.004	0.003

Job spell F.E.	No	No	No	No	No	No	No	No	No	No	No
E: Bottom 95pct FE											
Post Ann. (2012-15)	0.016***	0.004***	0.007***	0.030***	0.001***	0.010***	0.009***	0.002***	0.001**	0.006***	0.004***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
Post Impl. (2016-19)	0.016***	0.007***	0.006***	0.028***	0.002***	0.011***	0.010***	0.001***	0.001*	0.008***	0.002***
	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
T × Post Ann. (2012-15)	0.008***	-0.003***	0.006***	-0.012***	0.002***	-0.005***	-0.009***	-0.001	0.000	-0.001	-0.002***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
T × Post Impl. (2016-19)	0.007***	-0.002**	0.013***	0.014***	0.003***	-0.006***	-0.010***	0.001	0.001***	0.001	0.000
	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
N	250475	250475	250475	250475	250475	250475	250475	250475	250475	250475	250475
N job spells	38156	38156	38156	38156	38156	38156	38156	38156	38156	38156	38156
Adj R2	0.004	0.001	0.003	0.009	0.001	0.001	0.006	0.000	0.000	0.001	0.000
Mean C Group 2011	0.028	0.002	0.006	0.046	0.000	0.006	0.011	0.004	0.000	0.008	0.005
Mean T Group 2011	0.019	0.000	0.006	0.013	0.002	0.007	0.000	0.001	0.001	0.004	0.002
Job spell F.E.	No	No	No	No	No	No	No	No	No	No	No
F: Top 5pct FE											
Post Ann. (2012-15)	0.026***	0.020***	0.025***	0.035***	0.000***	0.009***	0.009***	0.001	-0.000	0.005**	0.005**
	(0.005)	(0.004)	(0.005)	(0.006)	(0.000)	(0.003)	(0.003)	(0.001)	(.)	(0.002)	(0.002)
Post Impl. (2016-19)	0.039***	0.037***	0.026***	0.029***	0.000***	0.013***	0.006*	0.000	-0.000***	0.006*	0.006*
	(0.008)	(0.008)	(0.007)	(0.007)	(0.000)	(0.005)	(0.003)	(0.000)	(0.000)	(0.003)	(0.003)
T × Post Ann. (2012-15)	0.008	-0.012***	0.030***	-0.020***	0.003***	-0.006**	-0.009***	0.000	0.000**	0.005**	-0.002
	(0.005)	(0.004)	(0.005)	(0.006)	(0.001)	(0.003)	(0.003)	(0.001)	(0.000)	(0.002)	(0.002)
T × Post Impl. (2016-19)	-0.010	-0.004	0.065***	-0.002	0.002***	-0.010**	-0.005*	0.001**	0.002***	0.001	-0.004
	(0.009)	(0.008)	(0.008)	(0.008)	(0.001)	(0.005)	(0.003)	(0.000)	(0.001)	(0.003)	(0.003)
N	19290	19290	19290	19290	19290	19290	19290	19290	19290	19290	19290
N job spells	3463	3463	3463	3463	3463	3463	3463	3463	3463	3463	3463
Adj R2	0.006	0.011	0.018	0.006	0.001	0.002	0.005	0.000	0.000	0.002	0.001
Mean C Group 2011	0.014	0.002	0.029	0.014	0.000	0.007	0.007	0.000	0.000	0.010	0.005
Mean T Group 2011	0.044	0.000	0.040	0.014	0.002	0.004	0.000	0.001	0.000	0.008	0.003
Job spell F.E.	No	No	No	No	No	No	No	No	No	No	No

[1] *Notes.* All specifications include job-spell fixed-effects.

Table 7: Joinid Full matrix

	(1) Unempl.	(2) Change Industry	(3) Fin - Same Status (T vs C)	(4) Fin - Same Status (Oth)	(5) Fin - Switch Status (T vs C)	(6) Fin - Switch Status (Oth)	(7) Fin Resid	(8) Self-empl.	(9) Abroad
A: All employees									
Post Ann. (2012-15)	0.000	-0.058**	-0.003	-0.022*	-0.039**	-0.041***	-0.003	0.001	0.165***
	(0.017)	(0.023)	(0.004)	(0.013)	(0.015)	(0.011)	(0.005)	(0.002)	(0.018)
Post Impl. (2016-19)	-0.024	-0.048**	-0.004	-0.018	-0.059***	-0.037***	-0.005	-0.000	0.195***
	(0.017)	(0.023)	(0.004)	(0.013)	(0.015)	(0.011)	(0.004)	(0.002)	(0.018)
T × Post Ann. (2012-15)	-0.015	0.054**	-0.017*	0.049***	0.043***	0.042***	0.004	-0.002	-0.156***
	(0.020)	(0.027)	(0.009)	(0.018)	(0.016)	(0.011)	(0.006)	(0.003)	(0.019)
T × Post Impl. (2016-19)	0.034*	0.130***	-0.031***	-0.067***	0.063***	0.045***	0.011*	-0.002	-0.183***
	(0.019)	(0.027)	(0.009)	(0.017)	(0.015)	(0.011)	(0.006)	(0.002)	(0.019)
N	10286	10286	10286	10286	10286	10286	10286	10286	10286
N job spells	10286	10286	10286	10286	10286	10286	10286	10286	10286
Adj R2	0.000	0.005	0.025	0.049	0.063	0.009	0.002	-0.000	0.086
Mean C Group 2011	0.148	0.432	0.008	0.064	0.096	0.044	0.000	0.004	0.204
Mean T Group 2011	0.189	0.367	0.059	0.273	0.004	0.027	0.020	0.002	0.060
Job spell F.E.	No	No	No	No	No	No	No	No	No
B: 50 < Tot ≤ 100									
Post Ann. (2012-15)	-0.021	0.060*	0.003	0.001	-0.017	-0.030*	0.004	0.000	0.000
	(0.019)	(0.037)	(0.003)	(0.023)	(0.028)	(0.016)	(0.009)	(.)	(.)
Post Impl. (2016-19)	-0.031*	0.110***	0.003	0.013	-0.061**	-0.029*	-0.005	0.000	0.000
	(0.018)	(0.036)	(0.003)	(0.024)	(0.026)	(0.016)	(0.008)	(.)	(.)
T × Post Ann. (2012-15)	0.014	-0.090**	-0.020*	0.050*	0.021	0.031*	-0.004	0.000	0.000
	(0.021)	(0.041)	(0.012)	(0.029)	(0.028)	(0.017)	(0.011)	(.)	(.)
T × Post Impl. (2016-19)	-0.006	-0.003	-0.040***	-0.060**	0.067**	0.037**	0.005	0.000	0.000
	(0.020)	(0.041)	(0.011)	(0.029)	(0.026)	(0.017)	(0.009)	(.)	(.)
N	5043	5043	5043	5043	5043	5043	5043	5043	5043
N job spells	5043	5043	5043	5043	5043	5043	5043	5043	5043
Adj R2	0.003	0.013	0.022	0.023	0.103	0.009	-0.001	.	.

Mean C Group 2011	0.062	0.660	0.000	0.124	0.124	0.031	0.000	0.000	0.000
Mean T Group 2011	0.081	0.553	0.072	0.248	0.005	0.028	0.014	0.000	0.000
Job spell F.E.	No	No	No	No	No	No	No	No	No
C: 100 < Tot ≤ 175									
Post Ann. (2012-15)	0.009	0.045	-0.012	0.037	0.000	-0.068**	-0.012	0.000	-0.000***
	(0.031)	(0.054)	(0.012)	(0.036)	(0.043)	(0.033)	(0.012)	(.)	(0.000)
Post Impl. (2016-19)	-0.052**	0.144***	0.001	0.025	-0.083**	-0.030	-0.005	0.000	-0.000***
	(0.025)	(0.055)	(0.015)	(0.036)	(0.040)	(0.036)	(0.014)	(.)	(0.000)
T × Post Ann. (2012-15)	-0.049	-0.010	-0.029	-0.007	0.008	0.067**	0.021	0.000	-0.000
	(0.034)	(0.061)	(0.023)	(0.047)	(0.044)	(0.034)	(0.015)	(.)	(0.000)
T × Post Impl. (2016-19)	0.041	-0.039	-0.049*	-0.126***	0.086**	0.042	0.041**	0.000	0.003
	(0.031)	(0.063)	(0.026)	(0.047)	(0.040)	(0.038)	(0.019)	(.)	(0.003)
N	2002	2002	2002	2002	2002	2002	2002	2002	2002
N job spells	2002	2002	2002	2002	2002	2002	2002	2002	2002
Adj R2	0.004	0.030	0.029	0.071	0.103	0.028	0.007	.	-0.000
Mean C Group 2011	0.085	0.553	0.021	0.043	0.170	0.128	0.000	0.000	0.000
Mean T Group 2011	0.138	0.329	0.072	0.383	0.000	0.042	0.036	0.000	0.000
Job spell F.E.	No	No	No	No	No	No	No	No	No
D: Tot > 175									
Post Ann. (2012-15)	0.056	0.107	-0.031	-0.063	-0.050	-0.034	0.014	0.000	0.000
	(0.045)	(0.085)	(0.030)	(0.045)	(0.057)	(0.053)	(0.014)	(.)	(.)
Post Impl. (2016-19)	0.038	-0.003	-0.045*	0.005	0.043	-0.052	0.014	0.000	0.000
	(0.042)	(0.085)	(0.026)	(0.053)	(0.064)	(0.050)	(0.014)	(.)	(.)
T × Post Ann. (2012-15)	-0.107**	-0.092	-0.008	0.131**	0.058	0.026	-0.008	0.000	0.000
	(0.051)	(0.093)	(0.044)	(0.066)	(0.059)	(0.054)	(0.021)	(.)	(.)
T × Post Impl. (2016-19)	-0.077	0.148	-0.023	-0.111	-0.032	0.075	0.020	0.000	0.000
	(0.050)	(0.097)	(0.042)	(0.072)	(0.066)	(0.054)	(0.025)	(.)	(.)
N	834	834	834	834	834	834	834	834	834
N job spells	834	834	834	834	834	834	834	834	834
Adj R2	0.001	0.094	0.029	0.115	0.078	0.017	0.003	.	.
Mean C Group 2011	0.091	0.636	0.045	0.000	0.136	0.091	0.000	0.000	0.000
Mean T Group 2011	0.169	0.215	0.108	0.415	0.015	0.062	0.015	0.000	0.000

Job spell F.E.	No	No	No	No	No	No	No	No	No
E: Below 5pct FE									
Post Ann. (2012-15)	0.002	0.111***	-0.001	0.012	0.007	-0.025**	0.004	-0.001	-0.108***
	(0.020)	(0.028)	(0.004)	(0.016)	(0.020)	(0.013)	(0.006)	(0.001)	(0.012)
Post Impl. (2016-19)	-0.002	0.120***	0.001	0.026	-0.015	-0.016	-0.005	-0.001	-0.108***
	(0.020)	(0.028)	(0.004)	(0.017)	(0.019)	(0.013)	(0.005)	(0.001)	(0.012)
T × Post Ann. (2012-15)	-0.039*	-0.091***	-0.006	0.053**	-0.001	0.028**	0.000	-0.003	0.058***
	(0.022)	(0.031)	(0.010)	(0.021)	(0.020)	(0.013)	(0.007)	(0.002)	(0.012)
T × Post Impl. (2016-19)	0.008	-0.018	-0.024**	-0.085***	0.021	0.028**	0.015**	-0.003	0.058***
	(0.023)	(0.032)	(0.010)	(0.021)	(0.019)	(0.014)	(0.007)	(0.002)	(0.012)
N	8266	8266	8266	8266	8266	8266	8266	8266	8266
N job spells	8266	8266	8266	8266	8266	8266	8266	8266	8266
Adj R2	0.002	0.012	0.022	0.038	0.089	0.011	0.002	0.002	0.043
Mean C Group 2011	0.148	0.445	0.004	0.066	0.096	0.044	0.000	0.004	0.192
Mean T Group 2011	0.174	0.381	0.058	0.276	0.003	0.027	0.021	0.003	0.058
Job spell F.E.	No	No	No	No	No	No	No	No	No
F: Top 5pct FE									
Post Ann. (2012-15)	-0.008	0.110	-0.005	0.024	0.024	-0.027	-0.024	-0.000***	-0.094***
	(0.071)	(0.086)	(0.025)	(0.057)	(0.057)	(0.043)	(0.017)	(0.000)	(0.032)
Post Impl. (2016-19)	0.079	0.034	-0.024	0.023	0.055	-0.050	-0.024	-0.000***	-0.094***
	(0.093)	(0.102)	(0.017)	(0.069)	(0.074)	(0.044)	(0.017)	(0.000)	(0.032)
T × Post Ann. (2012-15)	-0.049	0.044	-0.065*	0.038	-0.037	0.033	0.012	-0.003	0.027
	(0.084)	(0.100)	(0.034)	(0.078)	(0.057)	(0.047)	(0.020)	(0.003)	(0.035)
T × Post Impl. (2016-19)	-0.016	0.132	-0.048	-0.081	-0.069	0.030	0.029	-0.003	0.027
	(0.110)	(0.118)	(0.030)	(0.090)	(0.075)	(0.044)	(0.025)	(0.003)	(0.035)
N	664	664	664	664	664	664	664	664	664
N job spells	664	664	664	664	664	664	664	664	664
Adj R2	0.000	0.034	0.019	0.044	0.067	0.012	-0.004	-0.006	0.026
Mean C Group 2011	0.158	0.316	0.053	0.053	0.105	0.053	0.000	0.000	0.263
Mean T Group 2011	0.324	0.225	0.070	0.254	0.014	0.028	0.014	0.000	0.070
Job spell F.E.	No	No	No	No	No	No	No	No	No

[1] *Notes.* All specifications include job-spell fixed-effects.

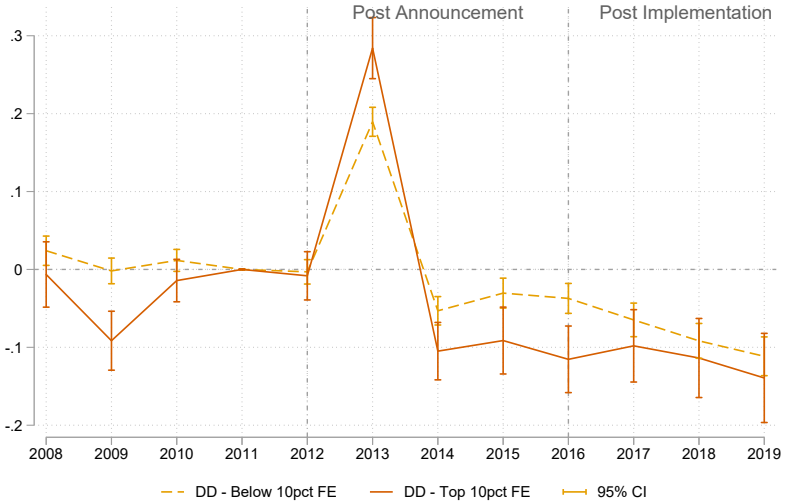
Table 8: Joinid absolute income matrix

	(1) Prev. Job 50<Tot≤100	(2) Prev. Job 100<Tot≤175	(3) Prev. Job Tot>175	(4) Prev. Job missing
A: All employees				
Post Ann. (2012-15)	0.005	0.019	0.004	-0.027**
	(0.029)	(0.026)	(0.018)	(0.012)
Post Impl. (2016-19)	-0.013	0.008	0.009	-0.004
	(0.029)	(0.026)	(0.018)	(0.013)
T × Post Ann. (2012-15)	0.020	-0.016	-0.010	0.006
	(0.033)	(0.030)	(0.021)	(0.013)
T × Post Impl. (2016-19)	0.107***	-0.058**	-0.035*	-0.014
	(0.033)	(0.029)	(0.021)	(0.014)
N	7587	7587	7587	7587
N job spells	7587	7587	7587	7587
Adj R2	0.007	0.003	0.001	0.005
Mean C Group 2011	0.565	0.267	0.124	0.043
Mean T Group 2011	0.621	0.225	0.085	0.069
Job spell F.E.	No	No	No	No

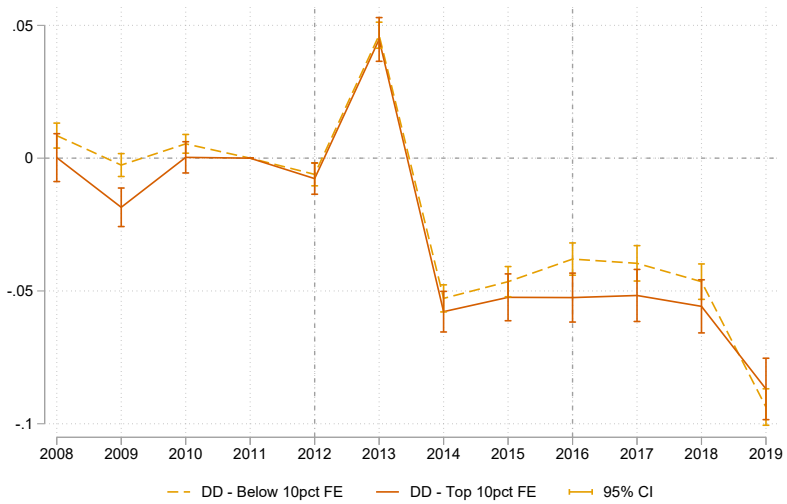
[1] *Notes*. All specifications include job-spell fixed-effects.

A Other Tables and Figures

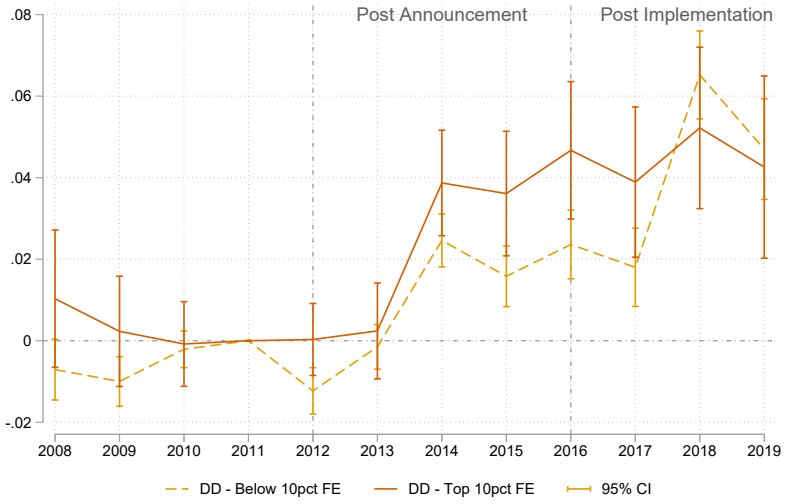
Figure A1: Effects on pay components - Heterogeneity analysis for top 10pct worker FE



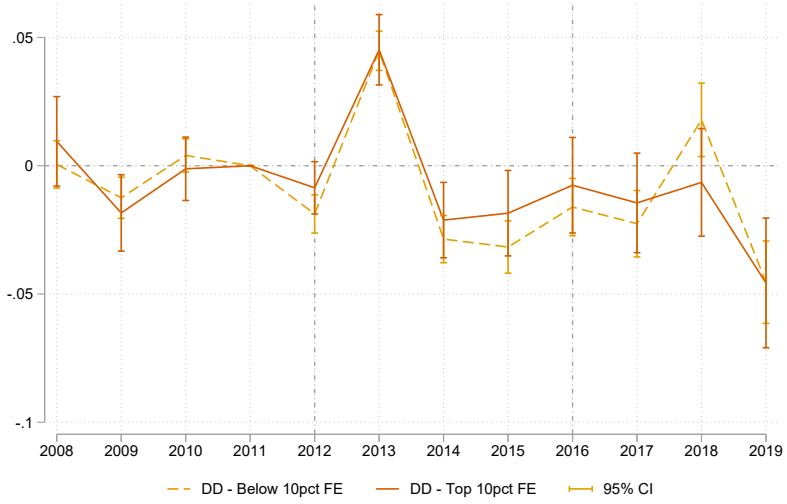
(a) Variable to fix pay ratio above 20%



(b) Variable pay over baseline total pay



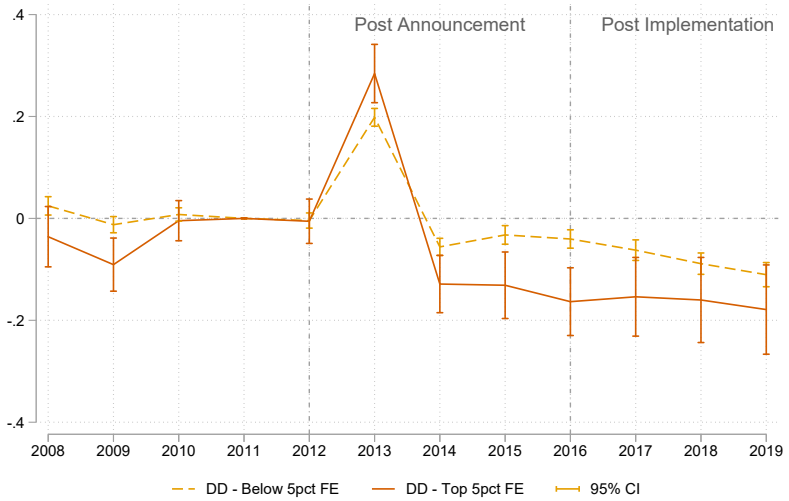
(c) Fixed pay over baseline total pay



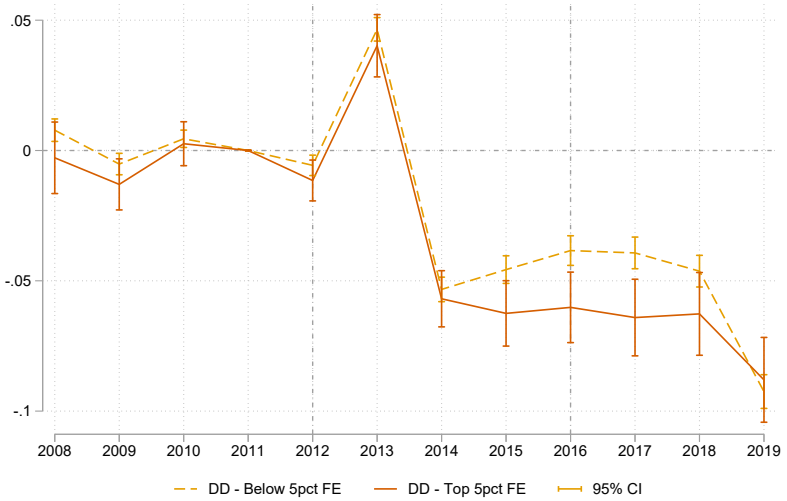
(d) Total pay over baseline total pay

Notes.

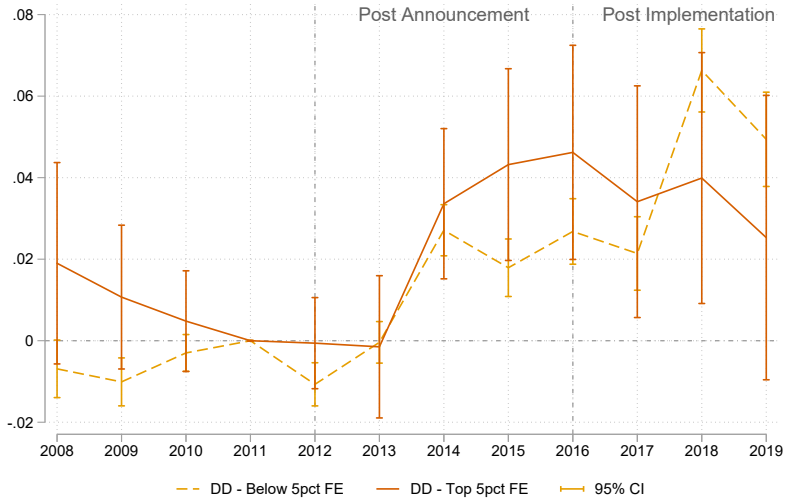
Figure A2: Effects on pay components - Heterogeneity analysis for top 5pct worker FE



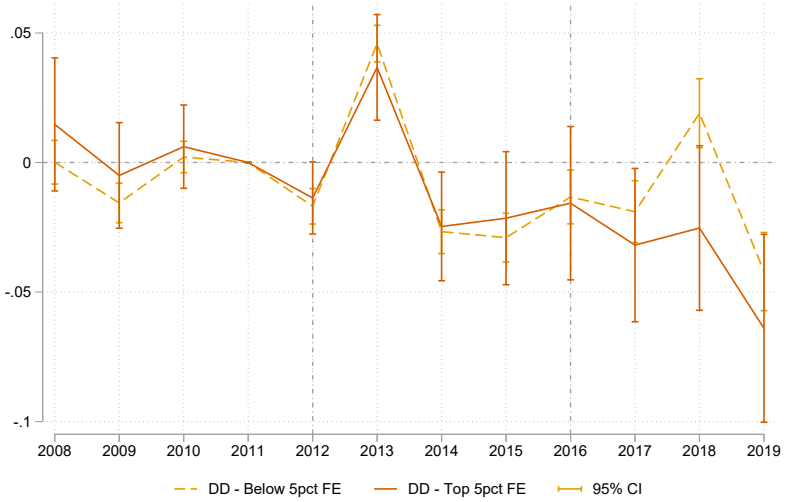
(a) Variable to fix pay ratio above 20%



(b) Variable pay over baseline total pay



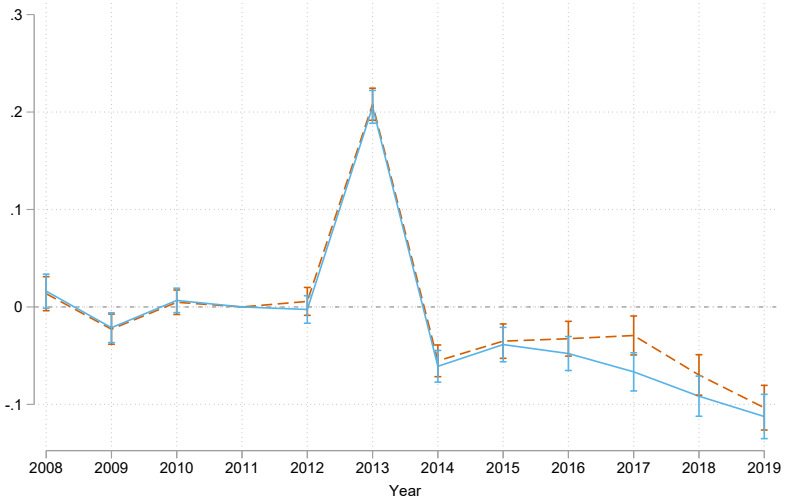
(c) Fixed pay over baseline total pay



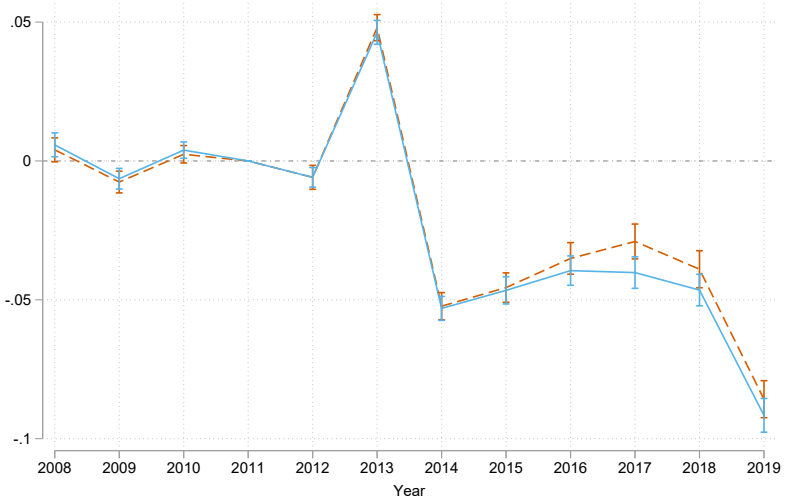
(d) Total pay over baseline total pay

Notes.

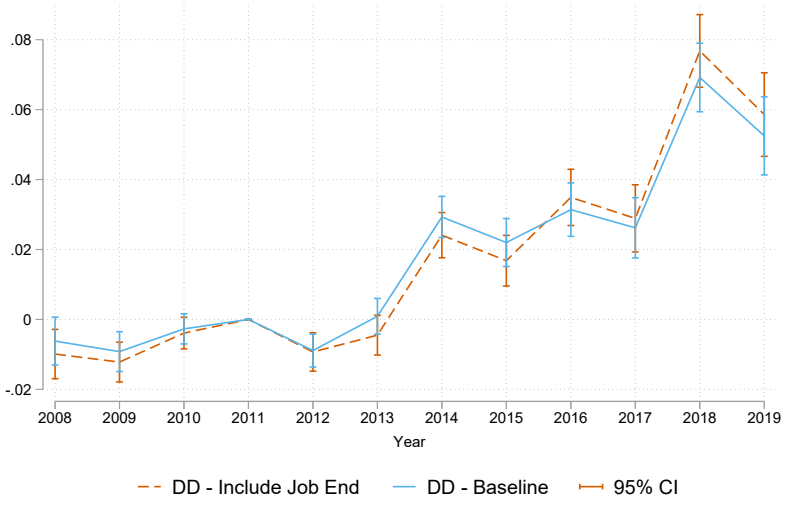
Figure A3: Robustness checks - Including the year of job termination



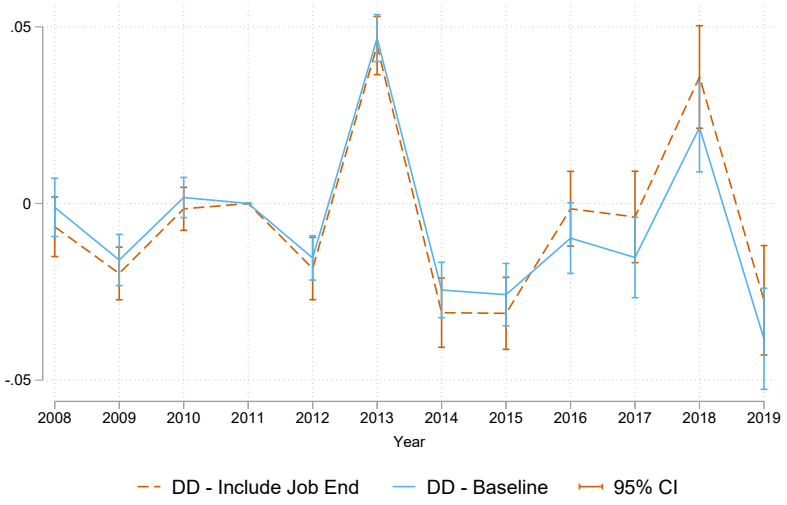
(a) Variable to fix pay ratio above 20%



(b) Variable pay over baseline total pay



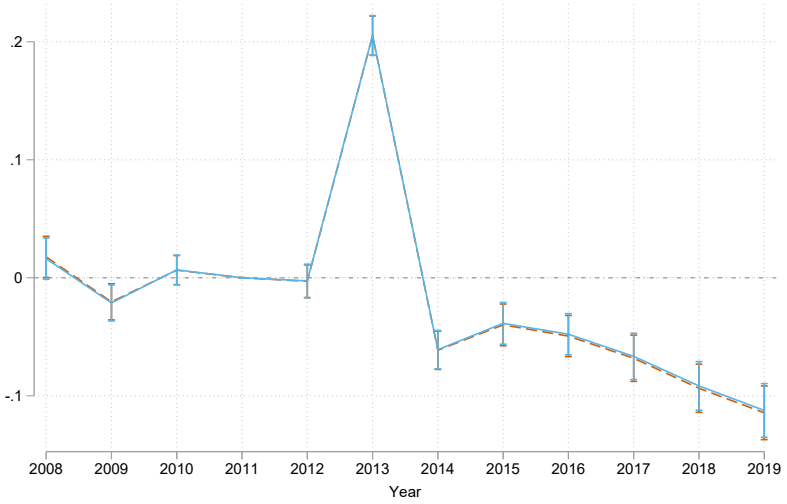
(c) Fixed pay over baseline total pay



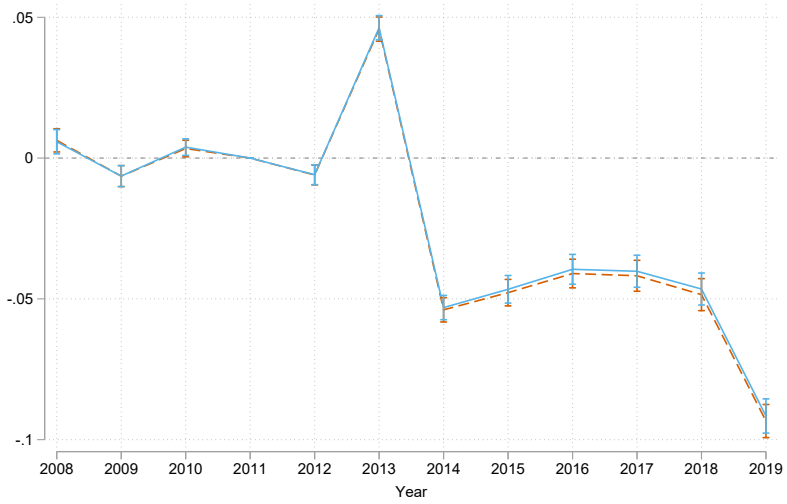
(d) Total pay over baseline total pay

Notes.

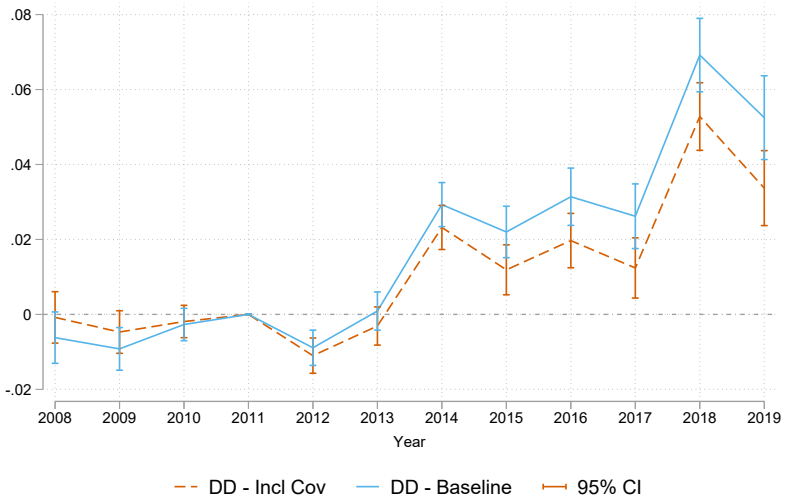
Figure A4: Robustness checks - Adding covariates



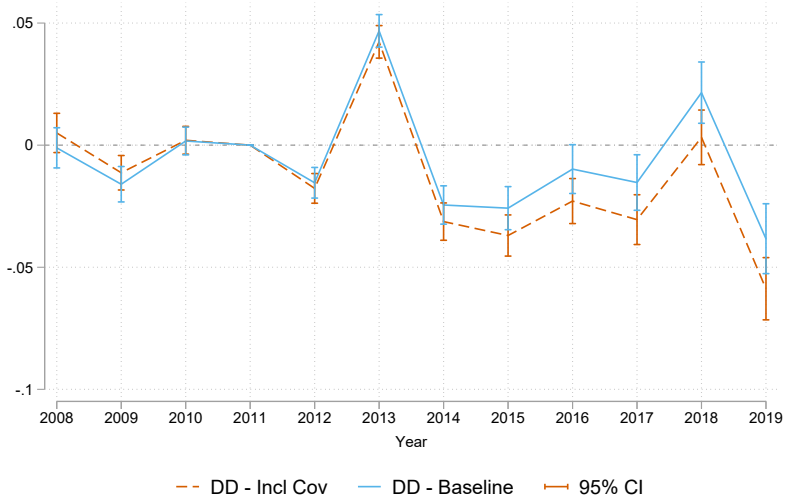
(a) Variable to fix pay ratio above 20%



(b) Variable pay over baseline total pay



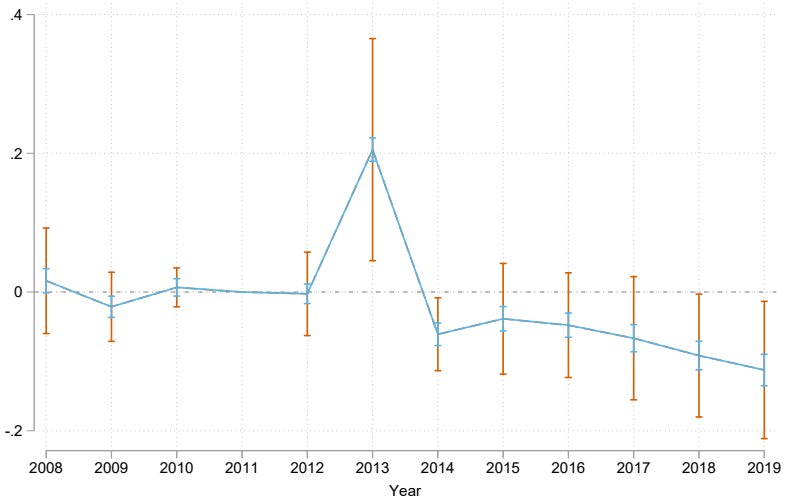
(c) Fixed pay over baseline total pay



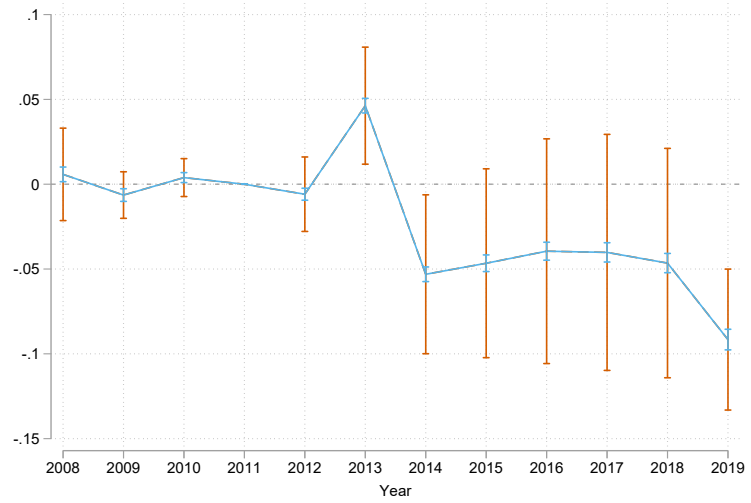
(d) Total pay over baseline total pay

Notes.

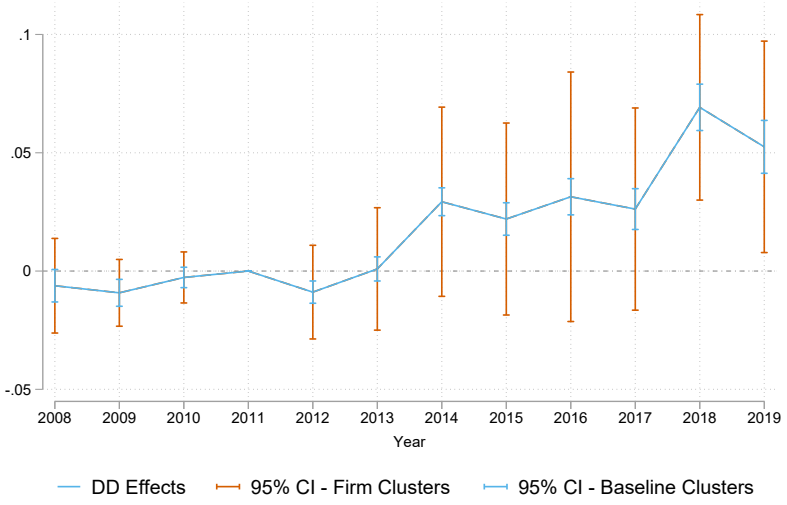
Figure A5: Robustness checks - se's clustered at the firm level



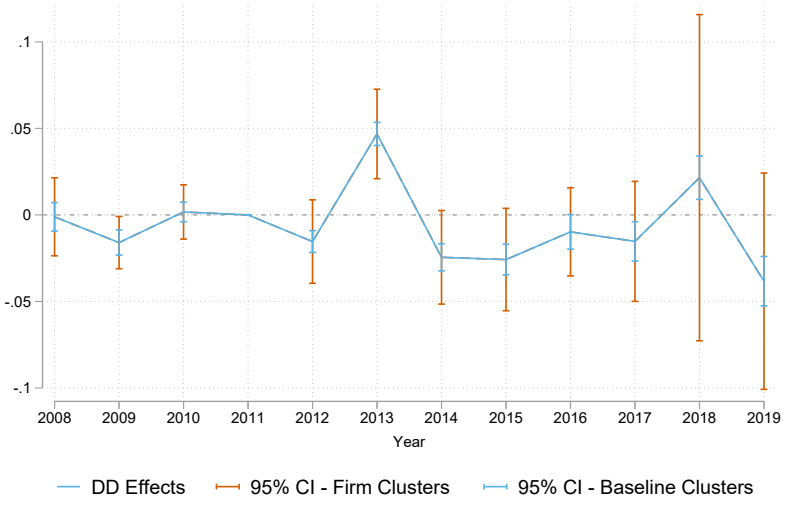
(a) Variable to fix pay ratio above 20%



(b) Variable pay over baseline total pay



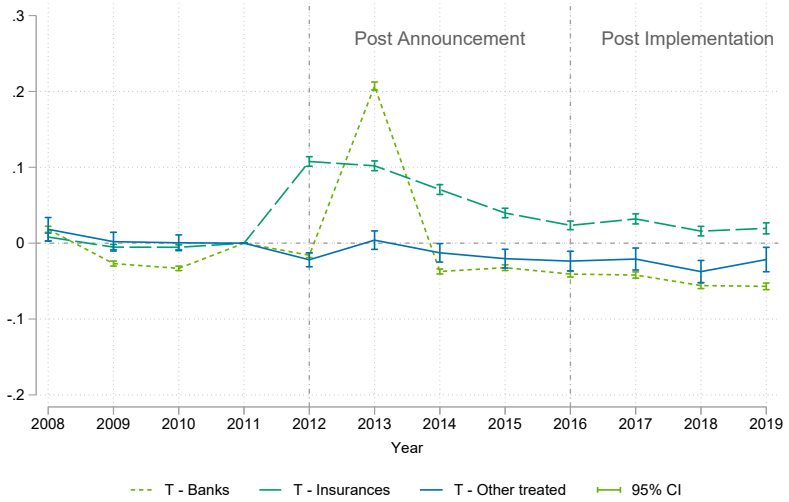
(c) Fixed pay over baseline total pay



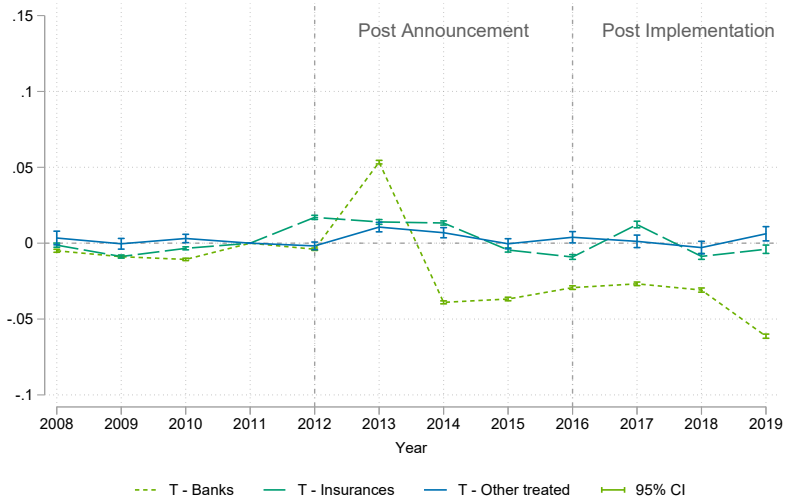
(d) Total pay over baseline total pay

Notes.

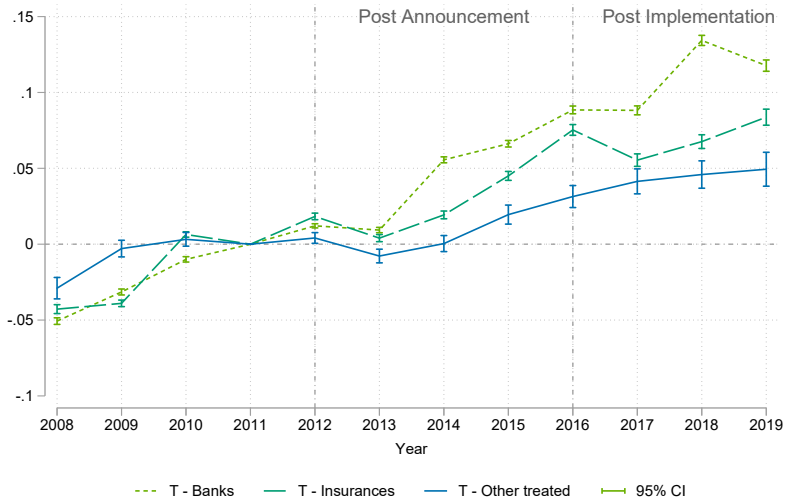
Figure A6: Robustness checks - annual pattern by treated subindustry



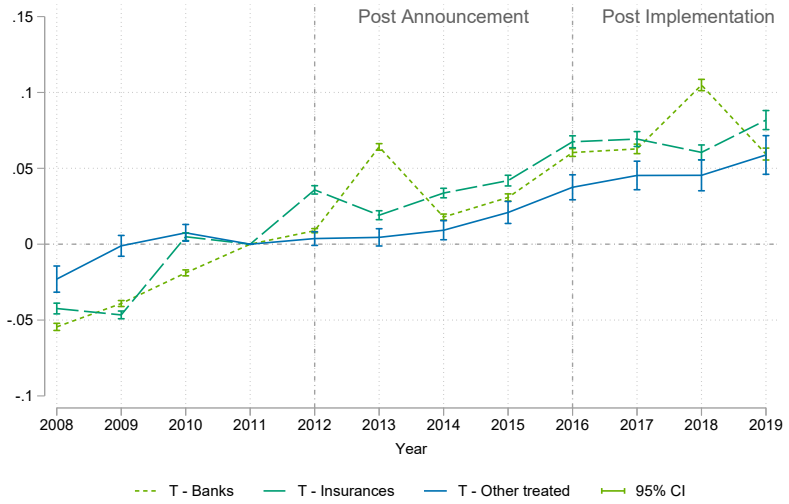
(a) Variable to fix pay ratio above 20%



(b) Variable pay over baseline total pay



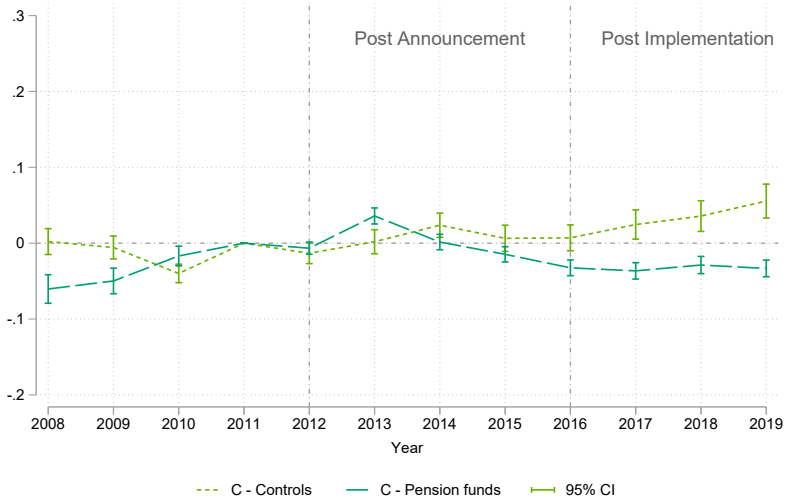
(c) Fixed pay over baseline total pay



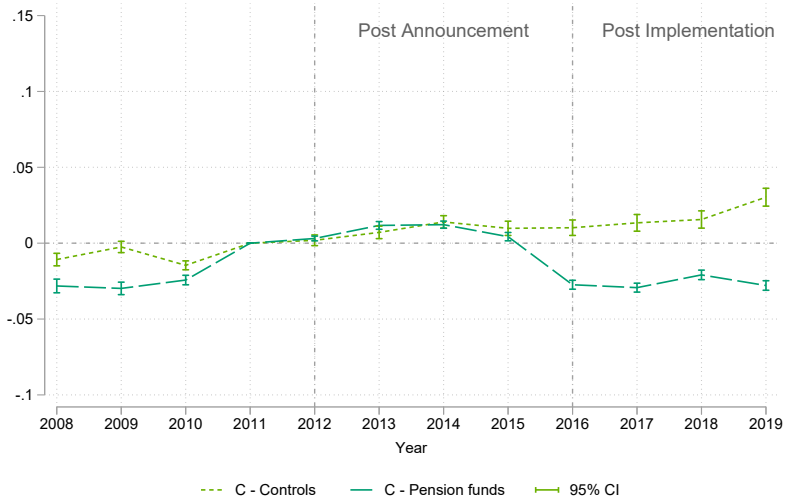
(d) Total pay over baseline total pay

Notes. Year effects by treatment subindustry (not the DD effect).

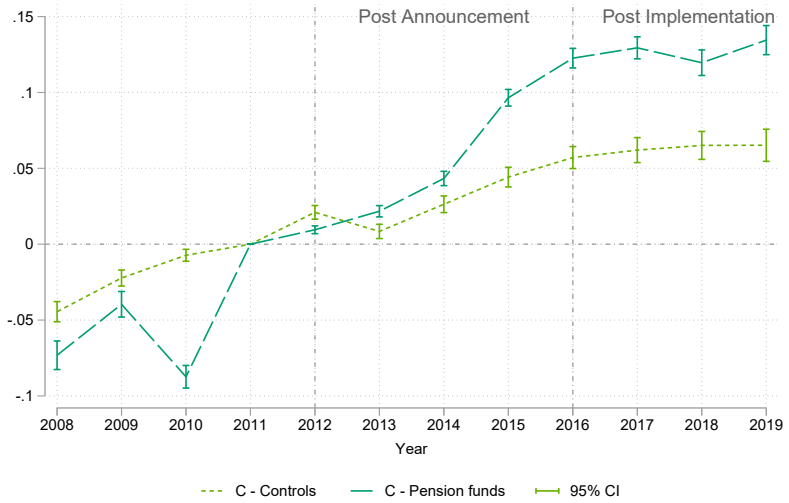
Figure A7: Robustness checks - annual pattern by control subindustry



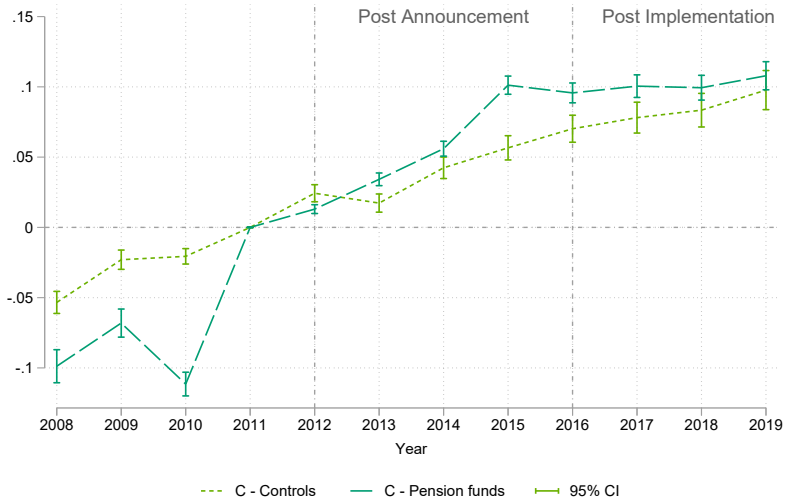
(a) Variable to fix pay ratio above 20%



(b) Variable pay over baseline total pay



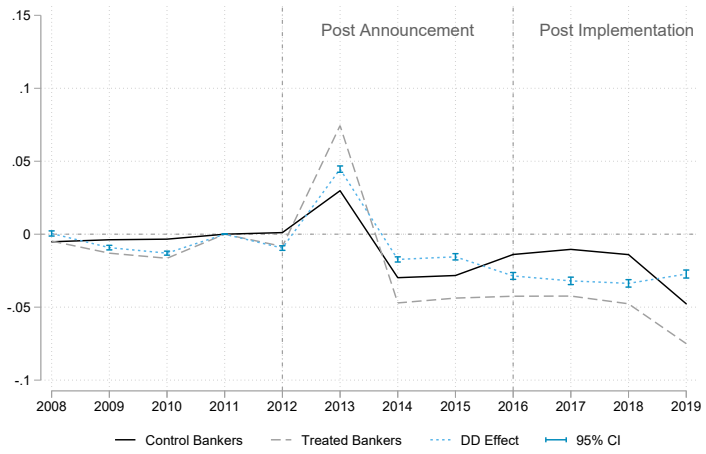
(c) Fixed pay over baseline total pay



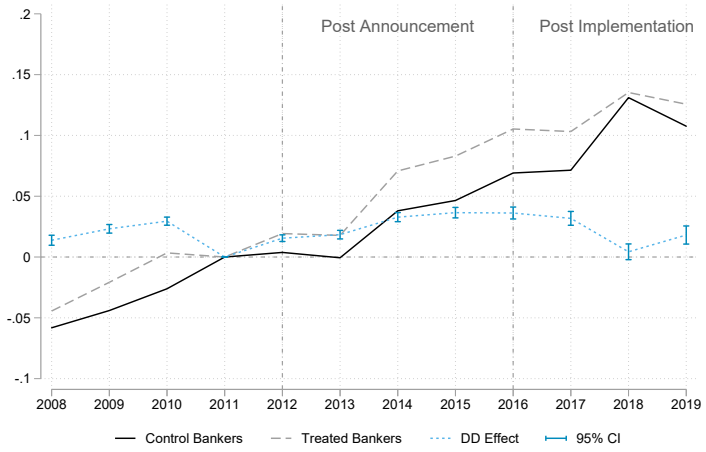
(d) Total pay over baseline total pay

Notes. Year effects by control subindustry (not the DD effect).

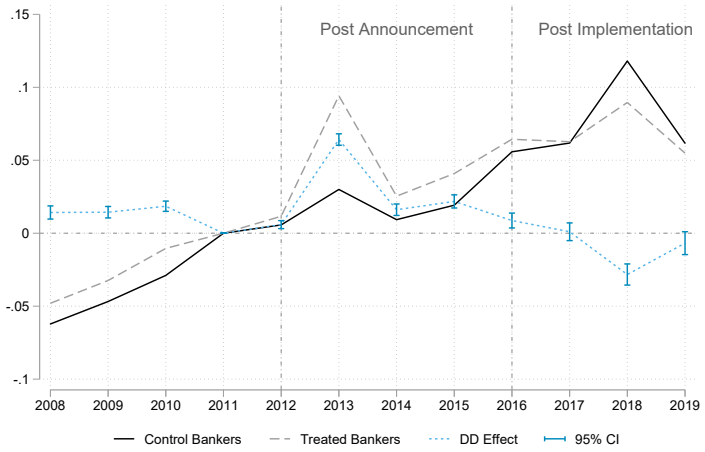
Figure A8: Robustness checks - within-bank specification



(a) Variable pay over total baseline pay



(b) Fix pay over total baseline pay



(c) Total pay over total baseline pay

Notes. ROB1b - variation is within banks and treatment status is defined at the individual level.

Table A1: All treated and control sectors

Groups	Sectors affected by the Wbfo	Sectors not affected by the Wbfo
Banks (N=38,603)	Cooperative banks (64191); Stock credit companies (64192); Saving banks (64193); General banks (64194)	
Insurances (N=25,415)	Life insurance (65111); Insurance in-kind (65112); Non-life insurance (65120); Reinsurance (65200)	
Other treated sectors (N=4,216)	Financial leasing (64910); Bill-brokers and other credit granting n.e.c. (64924); Stockbrokers, investment consultants etc. (66120); Insurance agents (66220); Fund management (66300)	
Pension funds (N=3,350)		Industry funds (65301); Corporate funds (65302); Pension funding for professional groups (65303); Other pension funding (65309)
Other control sectors (N=3,339)		Financial holdings (64200); Investment funds in financial assets (64301); Investment funds in real estate (64302); Investment funds with restricted entry (64303); Participation companies (64923); Market makers (66192); Risk and damage evaluation (66210); Consultancy; Management of pension funds (66292); Guarantee funding (66293); Other services, insurance and pension funding (66299)

Notes: We excluded the following sectors from the control group as they are the only micro-industries within the financial sector that do not fall under the Wft regulations: Central banking (6411) and Trust offices (66191). In addition, the residual category for which there is no information to evaluate whether they are treated or not contains: Mortgage banks and building funds (64921), Municipal credit banks and commercial finance companies (64922), Administration of financial markets (66110) and Mortgage, credit and currency brokers, bank and savings bank agencies etc. (66193). The number of individual employees (N) are provided by sector for the year 2011.

Table A2: Main industries of joiners and leavers in the period 2008-2011

Industry (4-digit code)	Treated		Control	
	Join from	Leave to	Join from	Leave to
Banks (6419)*	33.69%	18.48%	8.58%	5.23%
Temporary employment (7820)	8.41%	11.17%	4.92%	7.09%
Non-life insurance (6512)*	6.70%	5.50%	9.00%	10.35%
Computing programming (6201)	6.49%	2.03%	2.53%	2.21%
Business consultancy activities (7022)	4.91%	6.44%	6.61%	7.44%
Accounting and (tax) administration (6920)	4.22%	3.33%	3.80%	
Stockbrokers and investment consultants (6612)*	3.67%		2.25%	
Computer consultancy activities (6202)	3.40%	1.10%		
Insurance agents (6622)*		2.56%	5.34%	3.95%
General public administration (8411)	1.24%	2.43%		
Holding companies (7010)	1.92%	2.25%	4.08%	3.26%
Risk and damage evaluation (6621)**			4.78%	4.30%
Number of individuals	2,912	6,005	711	860

Notes: This table is based on our sample of analysis and shows the industry of individuals who join or leave the treated and control industries between 2008 and 2011. The table reports 4-digit industries for which we observe shares of at least 1.0% and at least 10 workers join from or leave to. The last row of the table displays the total number of individuals who leave or join the treatment and control group during this period. 4-digit sectors codes followed by an asterisk (*) are part of the treated and (**) are part of the controls. For example, of all employees starting employment in the treated sector between 2008 and 2011, 33.7% join from other banks and 6.7% come from the non-life insurance sector. Similarly, of all employees leaving the treated sector between 2008 and 2011, 18.5% go to other banks and 5.5% go to the non-life insurance sector.

Table A3: Rob 2

	(1) Var_t / Fix_t > 20%	(2) Var_t / Tot_{2011}	(3) Fix_t / Tot_{2011}	(4) Tot_t / Tot_{2011}
A: All incumbents				
Post Ann. (2012-15)	-0.004	0.006***	-0.000	0.006**
	(0.003)	(0.001)	(0.002)	(0.002)
Post Impl. (2016-19)	-0.007*	0.031***	0.042***	0.073***
	(0.004)	(0.001)	(0.004)	(0.005)
T × Post Ann. (2012-15)	0.054***	-0.003***	0.056***	0.053***
	(0.004)	(0.001)	(0.002)	(0.002)
T × Post Impl. (2016-19)	-0.030***	-0.063***	0.086***	0.023***
	(0.004)	(0.001)	(0.005)	(0.005)
N	323084	323084	323084	323084
N job spells	38032	38032	38032	38032
Adj R2	0.286	0.266	0.480	0.481
Mean C Group 2011	0.071	0.038	0.962	1.000
Mean T Group 2011	0.108	0.096	0.904	1.000
Job spell F.E.	Yes	Yes	Yes	Yes
B: 50<Tot≤100				
Post Ann. (2012-15)	0.010***	0.009***	-0.001	0.009***
	(0.003)	(0.001)	(0.002)	(0.003)
Post Impl. (2016-19)	0.015***	0.038***	0.042***	0.081***
	(0.003)	(0.001)	(0.005)	(0.005)
T × Post Ann. (2012-15)	0.050***	-0.002**	0.061***	0.058***
	(0.003)	(0.001)	(0.002)	(0.003)
T × Post Impl. (2016-19)	-0.023***	-0.064***	0.097***	0.033***
	(0.003)	(0.001)	(0.005)	(0.006)
N	241074	241074	241074	241074
N job spells	28144	28144	28144	28144
Adj R2	0.119	0.191	0.474	0.497
Mean C Group 2011	0.020	0.022	0.978	1.000
Mean T Group 2011	0.049	0.084	0.915	1.000
Job spell F.E.	Yes	Yes	Yes	Yes
C: 100<Tot≤175				
Post Ann. (2012-15)	-0.028**	-0.004	0.002	-0.001
	(0.012)	(0.003)	(0.005)	(0.006)
Post Impl. (2016-19)	-0.069***	0.001	0.052***	0.053***
	(0.017)	(0.005)	(0.012)	(0.013)
T × Post Ann. (2012-15)	0.064***	-0.004	0.051***	0.046***
	(0.012)	(0.003)	(0.005)	(0.006)
T × Post Impl. (2016-19)	-0.024	-0.049***	0.062***	0.013
	(0.017)	(0.005)	(0.012)	(0.013)
N	66502	66502	66502	66502
N job spells	7924	7924	7924	7924
Adj R2	0.249	0.313	0.456	0.434
Mean C Group 2011	0.191	0.085	0.915	1.000
Mean T Group 2011	0.183	0.116	0.884	1.000
Job spell F.E.	Yes	Yes	Yes	Yes
D: Tot>175				
Post Ann. (2012-15)	-0.234***	-0.037***	-0.002	-0.040***
	(0.044)	(0.008)	(0.008)	(0.008)
Post Impl. (2016-19)	-0.316***	-0.038***	-0.006	-0.044***

	(0.059)	(0.009)	(0.012)	(0.011)
T × Post Ann. (2012-15)	0.166***	0.014*	0.010	0.026***
	(0.045)	(0.008)	(0.008)	(0.009)
T × Post Impl. (2016-19)	0.066	-0.026***	0.004	-0.019
	(0.060)	(0.009)	(0.013)	(0.012)
N	15508	15508	15508	15508
N job spells	1964	1964	1964	1964
Adj R2	0.390	0.413	0.463	0.495
Mean C Group 2011	0.575	0.145	0.855	1.000
Mean T Group 2011	0.624	0.169	0.831	1.000
Job spell F.E.	Yes	Yes	Yes	Yes
E: Bottom 95pct FE				
Post Ann. (2012-15)	0.001	0.007***	0.001	0.009***
	(0.003)	(0.001)	(0.002)	(0.002)
Post Impl. (2016-19)	-0.001	0.033***	0.046***	0.080***
	(0.004)	(0.001)	(0.005)	(0.005)
T × Post Ann. (2012-15)	0.050***	-0.003***	0.059***	0.055***
	(0.003)	(0.001)	(0.002)	(0.003)
T × Post Impl. (2016-19)	-0.029***	-0.063***	0.089***	0.025***
	(0.004)	(0.001)	(0.005)	(0.005)
N	300214	300214	300214	300214
N job spells	34955	34955	34955	34955
Adj R2	0.259	0.256	0.487	0.492
Mean C Group 2011	0.056	0.034	0.966	1.000
Mean T Group 2011	0.095	0.094	0.906	1.000
Job spell F.E.	Yes	Yes	Yes	Yes
F: Top 5pct FE				
Post Ann. (2012-15)	-0.072***	-0.015***	-0.017***	-0.032***
	(0.020)	(0.004)	(0.005)	(0.006)
Post Impl. (2016-19)	-0.098***	-0.012**	-0.027**	-0.037***
	(0.025)	(0.006)	(0.013)	(0.012)
T × Post Ann. (2012-15)	0.097***	-0.001	0.021***	0.020***
	(0.021)	(0.004)	(0.006)	(0.006)
T × Post Impl. (2016-19)	-0.055**	-0.056***	0.034**	-0.021
	(0.027)	(0.006)	(0.014)	(0.013)
N	22870	22870	22870	22870
N job spells	3077	3077	3077	3077
Adj R2	0.382	0.368	0.370	0.372
Mean C Group 2011	0.243	0.087	0.913	1.000
Mean T Group 2011	0.253	0.118	0.881	1.000
Job spell F.E.	Yes	Yes	Yes	Yes

[1] *Notes*. All specifications include job-spell fixed-effects.

Table A4: Joinid Income relative matrix

	Bottom 95pct FE	Top 5pct FE	Bottom 90pct FE	Top 10pct FE	FE missing
A: All employees					
Post Ann. (2012-15)	0.011	-0.023	0.021	-0.034	0.013**
	(0.017)	(0.017)	(0.023)	(0.023)	(0.005)
Post Impl. (2016-19)	-0.035*	-0.062***	0.034	-0.130***	0.096***
	(0.019)	(0.015)	(0.023)	(0.020)	(0.013)
T × Post Ann. (2012-15)	0.004	0.002	0.011	-0.005	-0.006
	(0.019)	(0.018)	(0.026)	(0.026)	(0.006)
T × Post Impl. (2016-19)	0.000	0.017	-0.010	0.027	-0.017
	(0.021)	(0.016)	(0.026)	(0.022)	(0.014)
N	7587	7587	7587	7587	7587
N job spells	7587	7587	7587	7587	7587
Adj R2	0.005	0.007	0.002	0.018	0.048
Mean C Group 2011	0.932	0.068	0.863	0.137	0.000
Mean T Group 2011	0.933	0.067	0.850	0.150	0.000
Job spell F.E.	No	No	No	No	No

[1] *Notes.* All specifications include job-spell fixed-effects.

Table A5: Sepid Rob 1b

	(1) All Separations	(2) Voluntary	(3) Voluntary excl. early retirement	(4) Involuntary
A: All incumbents				
Post Ann. (2012-15)	0.074***	0.049***	0.034***	0.040***
	(0.001)	(0.001)	(0.001)	(0.001)
Post Impl. (2016-19)	0.090***	0.071***	0.053***	0.037***
	(0.001)	(0.001)	(0.001)	(0.001)
T × Post Ann. (2012-15)	0.009***	0.007***	0.007***	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
T × Post Impl. (2016-19)	0.055***	0.045***	0.035***	0.020***
	(0.002)	(0.002)	(0.002)	(0.002)
N	249427	249427	249427	249427
N job spells	38337	38337	38337	38337
Adj R2	0.023	0.020	0.016	0.008
Mean C Group 2011	0.102	0.071	0.055	0.048
Mean T Group 2011	0.031	0.019	0.017	0.015
Job spell F.E.	No	No	No	No
B: 50<Tot≤100				
Post Ann. (2012-15)	0.071***	0.047***	0.032***	0.039***
	(0.001)	(0.001)	(0.001)	(0.001)
Post Impl. (2016-19)	0.091***	0.071***	0.053***	0.037***
	(0.002)	(0.001)	(0.001)	(0.001)
T × Post Ann. (2012-15)	0.014***	0.009***	0.009***	0.005***
	(0.002)	(0.001)	(0.001)	(0.001)
T × Post Impl. (2016-19)	0.059***	0.048***	0.039***	0.021***
	(0.003)	(0.002)	(0.002)	(0.002)
N	186252	186252	186252	186252
N job spells	28376	28376	28376	28376
Adj R2	0.024	0.021	0.017	0.008
Mean C Group 2011	0.087	0.060	0.045	0.042
Mean T Group 2011	0.020	0.012	0.011	0.009
Job spell F.E.	No	No	No	No
C: 100<Tot≤175				
Post Ann. (2012-15)	0.089***	0.062***	0.042***	0.047***
	(0.003)	(0.003)	(0.002)	(0.002)
Post Impl. (2016-19)	0.085***	0.069***	0.047***	0.039***
	(0.004)	(0.004)	(0.003)	(0.003)
T × Post Ann. (2012-15)	-0.014***	-0.006**	-0.002	-0.012***
	(0.004)	(0.003)	(0.003)	(0.003)
T × Post Impl. (2016-19)	0.045***	0.038***	0.031***	0.014***
	(0.005)	(0.005)	(0.004)	(0.004)
N	51280	51280	51280	51280
N job spells	7985	7985	7985	7985
Adj R2	0.022	0.019	0.014	0.008
Mean C Group 2011	0.109	0.083	0.061	0.049
Mean T Group 2011	0.042	0.027	0.024	0.018
Job spell F.E.	No	No	No	No
D: Tot>175				
Post Ann. (2012-15)	0.178***	0.124***	0.086***	0.092***
	(0.019)	(0.016)	(0.015)	(0.015)
Post Impl. (2016-19)	0.094***	0.072***	0.065***	0.029**

	(0.025)	(0.022)	(0.022)	(0.014)
T × Post Ann. (2012-15)	-0.088***	-0.058***	-0.040***	-0.048***
	(0.020)	(0.017)	(0.015)	(0.015)
T × Post Impl. (2016-19)	0.059**	0.049**	0.017	0.042***
	(0.026)	(0.023)	(0.022)	(0.015)
N	11895	11895	11895	11895
N job spells	1976	1976	1976	1976
Adj R2	0.030	0.024	0.015	0.013
Mean C Group 2011	0.205	0.163	0.108	0.096
Mean T Group 2011	0.072	0.042	0.034	0.038
Job spell F.E.	No	No	No	No
E: Bottom 95pct FE				
Post Ann. (2012-15)	0.071***	0.047***	0.033***	0.037***
	(0.001)	(0.001)	(0.001)	(0.001)
Post Impl. (2016-19)	0.088***	0.069***	0.052***	0.036***
	(0.001)	(0.001)	(0.001)	(0.001)
T × Post Ann. (2012-15)	0.007***	0.005***	0.008***	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
T × Post Impl. (2016-19)	0.052***	0.042***	0.036***	0.016***
	(0.002)	(0.002)	(0.002)	(0.001)
N	232118	232118	232118	232118
N job spells	35234	35234	35234	35234
Adj R2	0.022	0.019	0.016	0.007
Mean C Group 2011	0.095	0.066	0.055	0.040
Mean T Group 2011	0.027	0.017	0.016	0.011
Job spell F.E.	No	No	No	No
F: Top 5pct FE				
Post Ann. (2012-15)	0.157***	0.111***	0.049***	0.108***
	(0.007)	(0.006)	(0.004)	(0.006)
Post Impl. (2016-19)	0.172***	0.149***	0.070***	0.103***
	(0.013)	(0.012)	(0.008)	(0.010)
T × Post Ann. (2012-15)	-0.033***	-0.017**	-0.006	-0.026***
	(0.008)	(0.007)	(0.005)	(0.007)
T × Post Impl. (2016-19)	0.028*	0.021	0.008	0.020*
	(0.015)	(0.014)	(0.010)	(0.012)
N	17309	17309	17309	17309
N job spells	3103	3103	3103	3103
Adj R2	0.038	0.033	0.014	0.022
Mean C Group 2011	0.227	0.156	0.056	0.171
Mean T Group 2011	0.065	0.037	0.025	0.040
Job spell F.E.	No	No	No	No

[1] *Notes*. All specifications include job-spell fixed-effects.

Table A6: Sepid Full matrix Rob 1b

	(1) Invol. excl. retire (Norm)	(2) Retire (Norm)	(3) Retire Early	(4) Change Industry	(5) Fin - Same Status (T vs C)	(6) Fin - Same Status (Oth)	(7) Fin - Switch Status (T vs C)	(8) Fin - Switch Status (Oth)	(9) Fin Resid	(10) Self-empl.	(11) Abro
A: All incumbents											
Post Ann. (2012-15)	0.025***	0.002***	0.016***	0.017***	0.003***	0.005***	0.000***	0.001***	0.001***	0.004***	0.002
	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.00
Post Impl. (2016-19)	0.019***	0.005***	0.018***	0.031***	0.004***	0.003***	0.000***	0.001***	0.002***	0.006***	0.001
	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.00
T × Post Ann. (2012-15)	0.001	-0.000	0.000	0.003***	0.000	-0.000	0.000	0.000**	0.000*	0.002***	0.002
	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.00
T × Post Impl. (2016-19)	0.010***	0.002***	0.010***	0.020***	0.002***	0.003***	0.000***	0.001***	0.000	0.005***	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.00
N	249427	249427	249427	249427	249427	249427	249427	249427	249427	249427	2494
N job spells	38337	38337	38337	38337	38337	38337	38337	38337	38337	38337	3833
Adj R2	0.004	0.002	0.004	0.010	0.001	0.001	0.000	0.000	0.000	0.002	0.00
Mean C Group 2011	0.031	0.000	0.016	0.026	0.003	0.009	0.002	0.002	0.001	0.008	0.00
Mean T Group 2011	0.012	0.000	0.002	0.006	0.001	0.004	0.001	0.001	0.000	0.002	0.00
Job spell F.E.	No	No	No	No	No	No	No	No	No	No	No
B: 50<Tot≤100											
Post Ann. (2012-15)	0.024***	0.001***	0.015***	0.016***	0.003***	0.005***	0.000***	0.001***	0.000***	0.004***	0.002
	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.00
Post Impl. (2016-19)	0.019***	0.004***	0.018***	0.032***	0.004***	0.003***	0.000**	0.001***	0.002***	0.006***	0.001
	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.00
T × Post Ann. (2012-15)	0.005***	-0.000	0.000	0.005***	0.001	-0.001	-0.000	0.000	0.000	0.002***	0.002
	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.00
T × Post Impl. (2016-19)	0.011***	0.002***	0.010***	0.025***	0.001***	0.004***	0.000**	0.000	0.000	0.003***	0.002
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.00
N	186252	186252	186252	186252	186252	186252	186252	186252	186252	186252	1862
N job spells	28376	28376	28376	28376	28376	28376	28376	28376	28376	28376	283
Adj R2	0.005	0.002	0.004	0.012	0.001	0.001	0.000	0.000	0.000	0.001	0.00

Mean C Group 2011	0.027	0.000	0.015	0.021	0.003	0.008	0.002	0.002	0.001	0.006	0.00
Mean T Group 2011	0.008	0.000	0.001	0.005	0.001	0.002	0.000	0.000	0.000	0.001	0.00
Job spell F.E.	No	No	No	No	No	No	No	No	No	No	No
C: 100<Tot≤175											
Post Ann. (2012-15)	0.028***	0.003***	0.020***	0.017***	0.004***	0.007***	-0.000	0.001***	0.001***	0.008***	0.001
	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.00
Post Impl. (2016-19)	0.017***	0.009***	0.022***	0.021***	0.003***	0.004***	0.000	0.001*	0.001**	0.007***	0.00
	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.00
T × Post Ann. (2012-15)	-0.008***	-0.001**	-0.005***	-0.001	-0.001	-0.001	0.000**	0.001*	0.000	-0.001	0.002
	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.00
T × Post Impl. (2016-19)	0.007***	-0.002	0.006**	0.018***	0.002**	0.003**	0.000	0.002***	0.000	0.007***	0.002
	(0.002)	(0.002)	(0.003)	(0.003)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.002)	(0.00
N	51280	51280	51280	51280	51280	51280	51280	51280	51280	51280	5128
N job spells	7985	7985	7985	7985	7985	7985	7985	7985	7985	7985	798
Adj R2	0.004	0.003	0.005	0.008	0.001	0.001	0.000	0.000	0.000	0.002	0.00
Mean C Group 2011	0.026	0.000	0.022	0.026	0.004	0.010	0.002	0.004	0.001	0.011	0.00
Mean T Group 2011	0.015	0.000	0.003	0.008	0.002	0.006	0.001	0.001	0.000	0.003	0.00
Job spell F.E.	No	No	No	No	No	No	No	No	No	No	No
D: Tot>175											
Post Ann. (2012-15)	0.054***	0.011**	0.038***	0.027***	0.003	0.014**	-0.000***	0.005	0.000***	0.024***	0.00
	(0.012)	(0.005)	(0.010)	(0.009)	(0.003)	(0.006)	(0.000)	(0.004)	(0.000)	(0.008)	(0.00
Post Impl. (2016-19)	0.022*	0.029**	0.007	0.014	-0.000	-0.000	-0.000***	0.000	0.000	0.022*	-0.0
	(0.012)	(0.014)	(0.007)	(0.010)	(.)	(0.000)	(0.000)	(.)	(.)	(0.012)	(.)
T × Post Ann. (2012-15)	-0.030**	-0.006	-0.018*	-0.011	-0.000	-0.006	0.001**	-0.003	0.001**	-0.016*	0.00
	(0.012)	(0.005)	(0.010)	(0.009)	(0.003)	(0.006)	(0.000)	(0.004)	(0.000)	(0.008)	(0.00
T × Post Impl. (2016-19)	0.010	-0.019	0.032***	0.021*	0.005***	0.007***	0.002**	0.006***	0.003***	-0.009	0.002
	(0.013)	(0.014)	(0.008)	(0.011)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.013)	(0.00
N	11895	11895	11895	11895	11895	11895	11895	11895	11895	11895	1189
N job spells	1976	1976	1976	1976	1976	1976	1976	1976	1976	1976	197
Adj R2	0.006	0.003	0.008	0.007	0.001	0.001	-0.000	0.001	0.000	0.003	0.00
Mean C Group 2011	0.042	0.000	0.054	0.054	0.000	0.012	0.006	0.006	0.000	0.018	0.01
Mean T Group 2011	0.030	0.000	0.008	0.011	0.004	0.008	0.002	0.004	0.000	0.003	0.00

Job spell F.E.	No	No	No	No	No	No	No	No	No	No	No
E: Bottom 95pct FE											
Post Ann. (2012-15)	0.024***	0.001***	0.014***	0.017***	0.003***	0.005***	0.000***	0.001***	0.001***	0.004***	0.002
	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.00
Post Impl. (2016-19)	0.019***	0.004***	0.017***	0.031***	0.004***	0.003***	0.000***	0.001***	0.002***	0.006***	0.001
	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.00
T × Post Ann. (2012-15)	0.002*	-0.000	-0.003***	0.003***	0.000	0.000	0.000	0.000***	0.000**	0.002***	0.002
	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.00
T × Post Impl. (2016-19)	0.010***	0.001*	0.006***	0.021***	0.002***	0.004***	0.000***	0.001***	0.000	0.005***	0.002
	(0.001)	(0.000)	(0.001)	(0.001)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.00
N	232118	232118	232118	232118	232118	232118	232118	232118	232118	232118	2321
N job spells	35234	35234	35234	35234	35234	35234	35234	35234	35234	35234	352
Adj R2	0.004	0.001	0.003	0.011	0.001	0.001	0.000	0.000	0.000	0.002	0.00
Mean C Group 2011	0.029	0.000	0.012	0.026	0.003	0.009	0.002	0.002	0.001	0.007	0.00
Mean T Group 2011	0.010	0.000	0.001	0.006	0.001	0.004	0.001	0.001	0.000	0.001	0.00
Job spell F.E.	No	No	No	No	No	No	No	No	No	No	No
F: Top 5pct FE											
Post Ann. (2012-15)	0.046***	0.011***	0.063***	0.013***	0.005***	0.005***	0.000	0.001*	0.001	0.010***	0.002
	(0.004)	(0.002)	(0.005)	(0.002)	(0.001)	(0.002)	(.)	(0.001)	(0.001)	(0.002)	(0.00
Post Impl. (2016-19)	0.023***	0.046***	0.080***	0.014***	0.000	0.001	0.000***	-0.000	0.001	0.006**	0.00
	(0.005)	(0.007)	(0.009)	(0.004)	(.)	(0.001)	(0.000)	(0.000)	(0.001)	(0.002)	(0.00
T × Post Ann. (2012-15)	-0.016***	-0.004*	-0.011*	0.002	-0.002	-0.002	0.001**	0.000	-0.001	-0.001	0.00
	(0.005)	(0.002)	(0.006)	(0.003)	(0.002)	(0.002)	(0.000)	(0.001)	(0.001)	(0.002)	(0.00
T × Post Impl. (2016-19)	0.006	-0.017**	0.013	0.017***	0.003***	0.002	0.000	0.001**	0.001	0.001	0.00
	(0.006)	(0.007)	(0.010)	(0.005)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.003)	(0.00
N	17309	17309	17309	17309	17309	17309	17309	17309	17309	17309	173
N job spells	3103	3103	3103	3103	3103	3103	3103	3103	3103	3103	310
Adj R2	0.007	0.012	0.017	0.006	0.001	0.001	-0.000	0.000	0.000	0.001	0.00
Mean C Group 2011	0.071	0.002	0.099	0.022	0.002	0.005	0.003	0.003	0.000	0.014	0.00
Mean T Group 2011	0.028	0.000	0.012	0.010	0.002	0.004	0.000	0.001	0.000	0.005	0.00
Job spell F.E.	No	No	No	No	No	No	No	No	No	No	No

[1] *Notes.* All specifications include job-spell fixed-effects.

B Institutional details

B.1 Summary of regulations on remuneration in the financial sector before the Dutch bonus cap was introduced

2009 EC Recommendations: General principles on remunerations, not legally binding, affecting identified staff. In April 2009, the European Commission published two recommendations concerning remuneration policies in the financial services sector: Recommendation 2009/384/EC and Recommendation 2009/385/EC. The Recommendation sets out general principles regarding remuneration policies that apply to all financial undertakings having their registered office in a European Union (EU) Member State. Financial undertakings are defined as performing any of the following business activities on a professional basis: (a) accepting deposits and other repayable funds; (b) providing investment services; (c) providing insurance services; and (d) activities similar to those set out in point (a), (b) or (c). A financial undertaking therefore includes credit institutions, investment firms, insurance undertakings and pension funds. The general goal of the two recommendations is to establish, implement and maintain a remuneration policy which is consistent with long-term interests of the financial undertaking, promotes effective risk management and does not induce excessive risk taking. The principles of the remuneration policies focus on remuneration structure, performance measures and governance. The EC Recommendation 384 and 385 are recommendations without any legal base. The principles contained in the 384 Recommendation apply to the remuneration of those category of staff whose professional activities have a material impact on the risk profile of the financial undertaking, whereas Recommendation 385 is specific to directors' remuneration only.

2011 EC Capital Requirements Directive III (CRD III): Only a limited amount of variable pay is payable upfront and in cash, affecting identified staff. The Directive 2010/76/EU on capital requirements and the supervisory review of remuneration policies came into effect on January 1, 2011, complementing the EC recommendations on remuneration policies of April 2009. The CRD III regulation includes rules on remuneration policies concerning the governance, risk alignment and transparency in credit institutions and investment firms. The main implication of CRD III for variable remuneration is that this type of remuneration should affect incentives such that it serves the long-term interests of the firm. The four main implications are: 1) Guaranteed variable remuneration should not be allowed; 2) A substantial portion of variable pay, at least 40 to 60%, should be deferred over a period of at least three to five years; 3) A substantial portion of variable pay, at least 50%, should be paid out as non-cash financial instruments, i.e. in-kind pay. This pay-out in financial instruments includes shares or share-linked instruments of the credit institution or investment firm; 4) Through malus or clawback arrangements the variable pay can be reduced upon negative performances. The CRD III affects employees who play a key role in the firm's risk profile, referred to as identified staff, including executives and senior staff responsible for management or independent control functions.

2011 NL Regulation on Sound Remuneration Policies (Rbb Wft 2011): Expands CRD III to additional sectors, affecting identified staff. The Regulation on Sound Remuneration Policies (in Dutch: Regeling beheerst beloningsbeleid (Rbb)) falls under the Financial Supervision Act 2011 (in Dutch: Wet nancieel toezicht (Wft)) and was introduced by DNB. It builds on the CRD III and came into effect on January 1, 2011. The main component of Rbb Wft 2011 is having a wider scope than CRD III, affecting, besides credit institutions and investment firms, also other firms in the financial economic sector, including clearing institutions, special purpose reinsurance vehicles, premium-based pension providers and insurers. Note that pension funds do not fall under the Rbb Wft 2011. Rbb Wft 2011 affects identified staff only.

2012 NL Bonus Prohibition Act: Blocks bonus payment for state-supported firms, affecting board members. The Bonus Prohibition Act (in Dutch: Wet bonusverbod staatsgesteunde ondernemingen) was announced and came into effect in June 2012. The Bonus Prohibition Act of 2012 for state-supported financial undertakings stipulates that financial undertakings that received help from the central government in the aftermath of the Great Financial Crisis - from 2012 onwards - were not

allowed to award variable remuneration to board members. This bonus ban holds for directors of banks that receive government support since the aftermath of the Great Financial Crisis (Ministerie van Financiën, 2018). Interestingly, this ban resulted in a ban on bonuses for members of the executive board but not for other senior management, which led to the situation that executive board members received less variable pay than the senior management. As we analyse the impact of the bonus cap on all employees of banks, our results are hardly affected by this ban. The Bonus Prohibition Act affects only Board Members.

2014 EC Capital Requirements Directive IV (CRD IV): 100% Bonus Cap, affecting identified staff. While the proposal for this directive was published already in 2011.The European Commission published Directive 2013/36/EU, CRD IV, in the Offcial Journal of the European Union on the 30th of September 2013, replacing CRD III and coming into force on January 1, 2014. The overarching goal of CRD IV is to strengthen the resilience of the EU banking sector, addressing the implementation of the Basel III capital rules. Therefore, while the main role of CRD IV was to implement in the EU the Basel III reforms, which include amendments to the definition of capital and counterparty credit risk and the introduction of a leverage ratio and liquidity requirements, CRD IV was also used to introduce non-Basel III reforms, such as the cap on bankers' bonuses. CRD IV stipulates that the maximum ration of variable pay to fix pay is 100%. This can be increased to 200% with the approval of shareholders11. Member states are allowed to set a lower maximum ratio of variable pay to fixed pay. The entities impacted by this Directive are, broadly speaking, credit institutions and investment firms within the Markets in Financial Instruments Directive (MiFID) - this includes all retail and investment banks, but also other forms of investment firms such as hedge funds. The bonus cap rules only applies to the group of identified staff, that is employees whose professional activities have a material impact on the risk profile of the firm.

2014 NL Regulation on Sound Remuneration Policies (Rbb Wft 2014): Expands CRD IV to additional sectors, affecting identified staff. The Regulation on Sound Remuneration Policies under the Financial Supervision Act, Rbb Wft 2014, amends the Rbb Wft 2011 and came into effect on August 1, 2014. Similar to the Rbb Wft 2011 and CRD III, the Rbb Wft 2014 is based on the CRD IV and extend its rules not only to credit institutions and investment firms but also to clearing institutions, special purpose reinsurance vehicles, premium-based pension providers and insurers. Rbb Wft 2014 affects identified staff only.

B.2 Exceptions to the Dutch bonus cap

There are a couple of exceptions to the Dutch bonus cap. First, employees working more than 50% of total hours abroad. For those working in EEA CRD IV holds (100% cap). For those working outside EEA the cap is at 200%. About 6.9% of bankers in 2016 make use of this exception. Importantly, however, in our data analysis are employees only included if they are formally registered and living in the Netherlands. Hence, it is likely that many of these expat bankers are not in our sample of analysis.

Second, employees in the NL who are not fully covered by a collective labour agreement. For them the bonus is capped at 100%. 0.6% of bankers in 2016. We examined employees that do not fall under a collective labour agreement in our data and the numbers are too low to analyse and report on them as per Statistics Netherlands privacy guidelines.

Third, employees working at an international holding company with at least 75% of its employees working outside the NL for at least 50% of their time for at least 3 years (over a period of 5 years). For them CRD IV holds.

Fourth, employees working in branch offices of credit institutions or investment firms that have their headquarters in another EU member state than the Netherlands. For them CRD IV holds.

B.3 Employee flows across treated and control sectors

In our analysis treated and control sectors are defined within the financial industry using 5-digit SBI codes. In particular, we follow the classification in Weitzel et al. (2018), who, working together with

the Dutch Ministry of Finance, operated a distinction between bonus-cap industries and non-bonus cap industries. Table A1 builds on Weitzel et al. (2018) and displays a conceptual categorization of sectors depending both on whether they were affected by the Wbfo bonus-cap policy and on the type of industry.[24] Note that, as explained in section 2.2, our main analysis includes in the treatment and control group only the category of sectors defined as *Banks* and *Other control sectors*. This sample selection is motivated by the fact that they are the only two groups of sectors that, with the exception of the Wbfo, fall under exactly the same regulations. This ensures that our analysis estimates the effect of the bonus cap. The table also shows the total number of incumbents belonging to each category of sectors in 2011.

To assess to what extent treated and control firms are interconnected, we compute the share of workers who join or leave the treated and control industries before the Wbfo announcement between 2008 and 2011. Table A2 shows that the two most important sectors for both joiners and leavers in the treatment as well as control sectors cover the banking sector and the temp work agency (TWA) sector.[25]

The insurance sector also represents another important industry for joiners and leavers for the treatment group as well as for the control group. However, both in entry and in exit, competition for workers is definitely not limited to the financial sector as at least 70% of the workers who joining and leave the treatment and control group are not employed in that industry. Taken together, the pre-policy patterns on labour mobility show that there is an interconnection between treated units (i.e. between different banks) but not much connectivity across treatment and control groups, which reassures us about the validity of our empirical approach. In addition, as explained in section 2.2 our analysis excludes observations on firms in the insurance sector where, given its role for labour mobility both for the treatment and control group, spillovers are more plausible.

[24]The categorization by Weitzel et al. (2018) excludes certain financial industries because it is unclear whether they are covered or not by the Wbfo. The excluded sectors are: Mortgage banks and building funds (64921), Municipal credit banks and commercial finance companies (64922), Administration of financial markets (66110), Mortgage, credit and currency brokers, bank and savings bank agencies (66193). Thus we also exclude these sectors from the analysis. Moreover, we also exclude Central banking (SBI code 64110) and Trust offices (SBI code 66191) because of their very different regulatory nature. Also, all sectors in Weitzel et al. (2018) for which we do not have any observations are automatically excluded.

[25]Employees who work at a TWA or who get a job placement through an intermediary organization will be part of the TWA sector group in the Dutch registry data. This is because the registry data does not allow us to observe the sector code of the client firm and therefore the TWA is considered the formal employer of this group of workers. The relatively high number of transitions between TWAs and the financial sector suggests that financial institutions make frequent use of the services of temporary work intermediary companies. In line with this, note that in the Netherlands TWAs are often commercial companies, and getting a job through a TWA is not considered a negative signal nor something typical of unskilled workers only.