Mind the Cap?
The Effects of Regulating Bankers' Pay

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Abstract

This paper examines the labor market consequences of regulating incentive pay, exploiting the introduction of the Dutch bonus cap as a sector-wide constraint on variable compensation in banking. Using registry employer–employee data with detailed information on fixed and variable pay and complete employment histories, we estimate a difference-in-differences design comparing banks to other financial institutions, complemented by within-bank variation in pre-reform exposure to the cap. We find that the reform led to a sharp decline in variable pay that was only partially offset by increases in fixed pay, resulting in lower total compensation for incumbent workers. The adjustment is consistent with a compensating differential arising from reduced pay risk and is substantially larger for high-talent employees, leading to a compression of within-bank earnings inequality. While the policy increased involuntary separations and early retirement, voluntary separations remained unchanged, suggesting a sector-wide adjustment of incumbents' outside options in response to the shock. In contrast, hiring fell markedly, particularly from abroad and from the upper end of the prior earnings distribution, indicating reduced ability of banks to attract top talent. Taken together, the results show that bonus caps reduce pay dispersion within firms but may impair talent acquisition, while suggesting that a broader policy coverage can attenuate these costs thanks to its diffused impact on outside options.

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1 Introduction

Variable pay has become a central component of compensation in modern labor markets, especially in high-skill and performance-oriented occupations. In the United States, performance-related pay — including bonuses, commissions, and piece rates — covers roughly 30 percent of employees and accounts for a disproportionate share of earnings growth and wage dispersion at the top of the distribution (Lemieux et al., 2009). Similar patterns are observed in Europe, where variable pay is particularly prevalent in finance, information technology, and professional services. In the Netherlands, the context of our study, 61 percent of employees in financial institutions receive some form of variable pay, the highest incidence among all sectors and roughly 10 percentage points higher than in ICT.

Due to its growing diffusion, variable pay has become a primary target of regulation. In contrast to traditional labor market interventions that regulate pay levels or employment conditions, variable pay and bonus caps directly constrain how workers are paid rather than how much they earn. Such policies are motivated by concerns about excessive risk-taking, rising pay inequality, and the social costs of high-powered incentives, particularly in the financial sector. By altering both the level and the riskiness of compensation, caps on variable pay have the potential to affect workers' earnings, within-firm pay inequality, and the selection of talent.

Despite their policy relevance, there is limited evidence on the labor market consequences of introducing or regulating incentive pay. Existing work on performance pay largely exploits firm-level or job-level variation in the availability of performance pay—often within a single firm—where workers' outside options are unaffected and adjustments operate primarily through internal pay-setting mechanisms (e.g., Lazear (2000)). By contrast, sector-wide restrictions simultaneously affect a large set of employers, reshaping workers' outside options and the competitive equilibrium for talent. As a result, the responses of compensation structures, worker mobility, and talent allocation may differ fundamentally from those documented in micro-level settings. This paper studies the first large-scale sector-wide reform of variable pay, exploiting the introduction of the Dutch bonus cap to analyze how regulating incentive compensation affects pay structures, inequality, and the allocation of talent in the labor market.

We use linked employer–employee administrative data from the Netherlands that cover the universe of employees in the financial sector and provide detailed information on fixed and variable pay components. We begin by documenting that variable pay in our setting behaves like pay for performance. We then exploit the introduction of the Dutch bonus cap, which limits the ratio of variable to fixed pay, as a natural experiment. The policy applies to banks but not to other subsectors within finance, which we use as a control group. Using a difference-in-differences design, we study

how the reform affected the pay composition and total compensation of incumbent workers. We then analyze how the policy influenced firms' ability to retain incumbent employees and attract new talent from the external labor market.

Our analysis relies on administrative data covering workers' contracts and earnings, with precise information on fixed pay, variable pay, and employment histories. Variable pay is particularly prevalent in the financial sector, making it an ideal context to study the effects of a cap on performance-based compensation. We provide three pieces of evidence showing that variable pay operates as pay for performance in our setting. First, variable pay fluctuates substantially within worker–firm spells and does not simply track the smooth growth of fixed pay with seniority. Second, variable pay is significantly more dispersed than fixed pay across workers within the same job titles and occupations. Third, following Lemieux et al. (2009), we show that jobs with variable pay exhibit higher returns to education and experience, consistent with incentive pay rewarding productivity.

We estimate difference-in-differences specifications comparing treated and control sectors before and after the announcement of the bonus cap. A potential threat to identification is the presence of bank-specific shocks contemporaneous with the policy. To address this concern, we implement a series of robustness checks, including a within-bank specification that compares incumbent workers who were exposed to the cap prior to the reform with those who were not. This strategy isolates variation in exposure to the policy while holding constant time-varying bank-level shocks. To address potential anticipation effects, we define treatment timing based on the policy announcement rather than its full implementation. We first focus on incumbent workers to study changes in pay structures while holding workforce composition constant. Then, we complement this analysis with firm-level evidence on hirings and separations, as well as individual-level analyses of worker mobility and the characteristics of new hires.

We begin our core empirical analysis by examining how the bonus cap affected the compensation of incumbent workers, defined as employees working in the financial sector up to and including 2011. As a first stage, we show that the ratio of variable to fixed pay declines sharply following the reform. By 2019, the probability that the variable-to-fixed pay ratio exceeds the 20 percent cap is 5.6 percentage points lower in banks relative to their baseline level, corresponding to a decline of nearly 60 percent compared with banks' pre-reform average of 9.6 percent. Consistent with this pattern, the continuous variable-to-fixed pay ratio falls by about 8 percentage points, implying that variable pay declines from roughly 11 percent to 3 percent of fixed pay.

We then show that the relative importance of variable pay in total compensation decreases substantially. By 2019, variable pay as a share of total pay falls by 6.6 percentage points, a reduction of roughly 75 percent relative to its baseline average. We investigate whether firms offset this reduction by increasing fixed pay. Fixed pay

increases significantly: by 2019, bankers' fixed pay rises by 12.8 percentage points relative to its 2011 level, corresponding to a 14 percent increase with respect to the baseline fixed-pay share. However, this adjustment is insufficient to fully compensate for the decline in variable pay. As a consequence, total compensation for bankers grows more slowly than that of control workers and is 6.3 percentage points lower by 2019 relative to its 2011 level. These patterns are consistent with the presence of compensating differentials: by reducing pay risk, the policy lowers the level of compensation firms must offer when workers are risk-averse.

We next study heterogeneous responses across workers with different levels of talent, measured using residual earnings after controlling for gender, age–education profiles, sector, and time fixed effects. We define high-talent workers as those in the top 10 percent of the residual earnings distribution.

For both high- and low-talent workers, the reform leads to a pronounced decline in variable pay and a corresponding increase in fixed pay. However, the effects differ sharply across talent groups. Among low-talent workers, the increase in fixed pay largely offsets the reduction in variable pay, leaving total compensation broadly unchanged. In contrast, for high-talent workers, the increase in fixed pay is insufficient to compensate for the loss of variable pay. As a result, by 2019 total compensation for high-talent workers is 13.1 percentage points lower relative to comparable control workers. These effects imply a compression of the earnings distribution within banks and a reduction in pay inequality. We obtain similar results when talent is defined relative to peers within the same firm.

Several arguments address the concern that our results may reflect contemporaneous shocks rather than the bonus cap. First, the adjustment in compensation is inconsistent with a general contraction in pay: the decline in variable pay among bankers is accompanied by a simultaneous increase in fixed pay relative to the control group, a pattern that is difficult to reconcile with financial distress or broader sectoral shocks. Second, a within-bank analysis comparing incumbents with pre-reform variable-to-fixed pay ratios above and below the cap threshold yields results that are very similar to our baseline estimates.

We then turn to the effects of the bonus cap on firms' ability to retain and attract workers. At the firm level, we document that both hiring and separations decline following the reform, measured cumulatively relative to employment in 2011. Hiring begins to diverge shortly after the policy announcement, with cumulative hires at banks 63.7 percentage points lower than in control firms by 2019. Separations also decline, but by a smaller magnitude: cumulative separations at banks are 52.8 percentage points lower by 2019. As a result, the contraction in hiring dominates, and total employment at banks declines relative to control firms, indicating that banks face increasing difficulty in attracting workers.

At the individual level, we show that incumbent employees are initially less likely to separate following the policy announcement, consistent with the aggregate decline in separations. However, in the medium to long run (2016–2019), incumbents in treated banks become about 2 percentage points more likely to separate than comparable workers in control sectors. This increase is driven entirely by involuntary separations and early retirement, while voluntary separations (excluding early retirement) remain unchanged. This pattern provides initial evidence that the policy does not significantly affect the voluntary retention of incumbent workers, while suggesting that firms actively adjust to the reform by shedding some employees. These effects are similar across talent groups.

Examining post-separation outcomes, we find that incumbents in treated banks are more likely to transition to unemployment or early retirement, but are less likely to move to non-capped financial firms within finance. Transitions to other industries increase modestly, but this effect is concentrated among lower-talent workers. We find no evidence that high-talent workers systematically move to other financial firms to escape the bonus cap. Overall, banks do not appear to face difficulties in retaining talented incumbent workers. Several mechanisms are consistent with this finding. First, increases in fixed pay partially compensate high-talent employees for the reduction in variable compensation. Second, skill specificity may limit outside opportunities, reducing incentives to separate. Finally, even after the reform, pay levels in banking remain relatively high compared to alternative employment options.

Finally, we study how the characteristics of new hires change following the reform. Using repeated cross-sections of hires, we show that banks increasingly hire workers who were previously unemployed, employed outside the financial sector, or working at non–bonus-capped firms within finance. At the same time, they hire significantly fewer workers from other capped banks and from abroad. The latter effect is particularly informative, as cross-border hires tend to be positively selected on skills, and therefore points to a reduction in the ability of banks to attract high-skilled workers.

We further analyze the prior earnings of new hires to assess changes in the quality of attracted workers. Banks increase hiring from the lower end of the earnings distribution, with a 10.7 percentage point rise in hires who previously earned between €50,000 and €100,000. In contrast, they reduce hiring of workers previously earning between €100,000 and €175,000, as well as those earning more than €175,000. Taken together, these results indicate that the bonus cap weakened banks' ability to attract top-paid—and likely high-skilled—workers from the external labor market.

Our paper relates to several strands of literature. A large literature in personnel economics shows that when compensation is linked to performance, it can increase productivity and wage dispersion through both incentive and selection effects. Canonical models predict that when workers differ in ability and pay is linked to performance,

stronger incentives can raise both average effort and earnings dispersion (Lazear, 1986; Holmström and Milgrom, 1991; Prendergast, 1999). While we do not observe individual output or performance measures directly, we exploit detailed administrative data on realized earnings to study how variation in variable pay eligibility affects pay outcomes across workers.

Our paper relates most directly to empirical studies estimating the effects of performance pay on productivity and sorting. Lazear (2000) shows that switching to piece rates significantly increased productivity at the Safelite Glass Corporation, with about half of the gain due to selection. Field experiments and firm-level studies (e.g., Bandiera et al. (2007); Delfgaauw et al. (2014, 2022)) similarly document incentive effects and changes in workforce composition. Unlike these studies, which are typically limited to single firms or small groups of firms, we study a sector-wide shift in variable pay regulation and leverage comprehensive employer-employee data covering the entire Dutch economy. This allows us to test for both incentive and sorting effects in a setting where employees' outside options are affected by the policy.

Related to our approach, Chen et al. (2024) study the introduction of pay-for-performance in the public sector and show that linking pay to performance can accelerate voluntary exits, highlighting the role of outside options in shaping turnover responses. In contrast, we analyze a private-sector regulation that limits performance pay in an environment where such compensation schemes were already prevalent, allowing us to assess how constraining incentive pay affects compensation, retention, and talent allocation.

A complementary strand of work emphasizes how performance pay contributes to earnings inequality. Lemieux et al. (2009) show that performance-pay jobs feature higher returns to skill and greater residual wage dispersion, and attribute up to one-quarter of the increase in wage inequality between the late 1970s and early 1990s to the rise of performance-related pay. More recent work by Tang et al. (2023) documents that performance pay is a major driver of the observed increase in within-job earnings inequality. Our contribution is to provide direct causal evidence on how the ability to use variable pay affects within-establishment wage levels and dispersion, using detailed information on individual earnings. Relatedly, our findings speak to broader work on wage inequality and pay-setting institutions. While some studies emphasize sorting across firms as a key driver of rising inequality (Barth et al., 2016), others also highlight growing dispersion within firms and jobs (Card et al., 2013). We show that even within the same firm, differences in access to performance-related bonuses can meaningfully shape the distribution of earnings across workers. In addition, we provide evidence that performance pay contributes to the sorting of talent across firms, amplifying between-firm differences in workforce composition and pay.

The remainder of the paper is organized as follows. Section 2 describes the Dutch

bonus cap policy. Section 3 presents the data and outlines the sample construction and treatment definitions. Section 4 examines whether variable pay operates as pay for performance. Section 5 discusses the empirical strategy and identification assumptions underlying the natural experiment. Section 6 presents results on the effects of the policy on incumbent employees' pay, while Section 7 analyzes firms' ability to retain and attract workers. Section 8 concludes.

2 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. 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.[1]

Although the primary objective of CRD IV was to transpose the Basel III prudential framework into EU law (notably capital and liquidity requirements), it also introduced important EU-specific reforms on remuneration, including a binding cap on bonuses. In particular, the CRD IV stipulates that variable remuneration cannot exceed 100% of the annual fixed remuneration for *identified staff* of banks and other financial institutions (e.g. investment firms), where the group of identified staff includes individuals whose professional activities affect the risk profile of the financial institution.[2] 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 CRD IV is binding for all member states, it allows countries to adopt more restrictive remuneration rules. The Netherlands used this flexibility to extend the EU

[1]Differently from the CRD IV, the Basel III regulatory framework is not a law but rather a voluntary set of internationally agreed standards developed by supervisors and central banks.

[2]CRD IV also strengthened requirements on the structure of variable pay. In particular, a substantial share of variable remuneration for identified staff must be deferred over several years (commonly 40–60% deferred for 3–5 years), alongside requirements regarding payment in instruments and ex post risk adjustment (malus/clawback).

[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.

framework in two ways. First, starting in 2014, it applied CRD IV–related remuneration provisions to a broader set of financial institutions than those directly covered by the EU rules, through the Regulation on Sound Remuneration Policies (Rbb Wft 2014). Second, it introduced an additional, tighter cap on variable pay for employees in a subset of financial institutions, most notably banks, through the Act on Remuneration Policies of Financial Undertakings (in Dutch *Wet beloningsbeleid financiële ondernemingen*; henceforth Wbfo).

In terms of timing, the Dutch coalition government announced its intention to introduce the stricter cap in October 2012. The Dutch Lower House adopted the proposal in October 2014, and the regulation entered into force in February 2015. The cap applied immediately to employees hired in 2015, while existing employees were granted a transition period, with compliance becoming binding for them in 2016.

Compared with most EU member states, the Dutch bonus cap is stricter along two dimensions. First, it sets a much lower ceiling on variable remuneration: the maximum variable-to-fixed pay ratio is 20%, which is five times below CRD IV's standard 100% cap.[4] Second, whereas CRD IV's remuneration rules primarily apply to *identified staff* (material risk takers), the Dutch cap applies to (nearly) all employees in covered institutions, allowing us to study effects across the broader workforce.[5] Importantly, the stricter 20% cap applies only to a subset of institutions operating in the financial sector, while other institutions remain regulated under CRD IV. This institutional split generates the cross-firm variation we exploit in our identification strategy (Section 5).

The Dutch financial industry.

Industry reaction. The introduction of EU-wide limits on variable remuneration was met with pushback from the financial sector, which argued that the cap would fail to "level the playing field" in the competition for talent with financial centres outside the EU. A recurring concern was that pay in finance is shaped by strong outside options and high mobility among top earners: banks operate in an environment where "star talent is apt to jump between different groups ... whenever a bigger pay-packet appears," and, as City grandee Sir David Arculus put it in the *Financial Times* after the onset of the Great Recession, "These bonuses are crazy – we all know that. But we don't know how to stop paying them without losing our best staff." (*Financial Times* (2009)).[6]

[4]Only three other European countries—Cyprus, Denmark, and Iceland—set bonus caps below CRD IV's threshold. However, all of them adopt 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 **??** for a list of employee categories exempt from the Wbfo.

[6]The debate remained active after the regulation was introduced, particularly in the UK. Following Brexit, UK regulators formally scrapped the EU-era bonus-cap rules that had limited bankers' bonuses to twice base salary, with the change taking effect from the end of 2023 (*The Guardian*, 2024).

These concerns were particularly salient in the Netherlands, where the Wbfo imposed a substantially tighter cap (20%) than elsewhere in Europe and applied it broadly to employees in covered institutions, rather than exclusively to *identified staff*. As a result, Dutch institutions most exposed to national and international labour-market competition—especially banks—warned that the stricter regime could hinder recruitment and retention relative to otherwise comparable institutions not covered by the stricter cap (pwc 2017, Bloomberg 2017). Beyond cross-border competition, the Dutch debate also emphasized that banks compete domestically for scarce specialist talent—especially high-level IT and data staff—who have attractive outside options in technology and other sectors.[7] Reflecting these concerns and complaints, in January 2026 the Dutch Lower House backed an amendment to relax the 20% bonus cap for most bank and fintech staff (while retaining it for material risk takers), although the proposal still requires further parliamentary approval.

3 Data

The administrative microdata from Statistics Netherlands covers the entire population of individuals living in the Netherlands. Our primary data source, the Job and Wages Register, is based on employees' monthly income statements and contains detailed information on earnings, hours worked, contract type, job start and end dates, and firm identifiers.[8] Using unique individual and firm identifiers, we link this register to additional datasets that provide information on individual and household characteristics (e.g., gender, age, country of birth, marital status, and presence of children) and firm characteristics (e.g., industry and size). Since performance payments are typically disbursed in April, May, or December rather than uniformly throughout the year, we aggregate the monthly information to the annual level and obtain a panel dataset tracking individuals and firms from 2008 to 2019.

3.1 Incumbent sample

Our individual-level analysis focuses on the incumbent sample, defined as individuals employed in the financial sector up to and including 2011. Although the bonus cap became effective only in 2015–2016, we use 2011 as the baseline year because the policy was announced in 2012, giving banks time to begin adjusting compensation schemes and workers scope to adjust their behavior in anticipation of its implementation from

[7] See, e.g., *NOS* (2026); and reporting linking the cap to competition for IT talent against Big Tech in *ING/ANP* (2025). See also discussion of the cap's impact on banks and fintechs' ability to attract specialist staff in *DutchNews.nl* (2026).

[8] The firm identifier refers to a single legal entity. In multi-entity groups, each entity is treated as a separate firm if it has legal control over its own activities, following the Statistics Netherlands' definition consistent with the Eurostat's Business Registers Manual (Eurostat, 2010).

2012 onward.[9] We restrict our attention to individuals aged 18 or older who work full time and hold a permanent contract.

While the bonus cap policy applies to all employees in treated firms, its primary objective was to influence employees whose decisions could affect the firm's overall risk profile rather than administrative or lower-skilled staff. Because the data lacks occupational codes, we approximate this target group of individuals by restricting the sample to employees earning at least 50,000 euro in fixed pay in 2011.[10] This threshold is intended to exclude most administrative and lower-ranked employees who are unlikely to have been directly affected by the policy.

Given the nature of the policy, we retain only firms with at least one employee whose variable-to-fixed pay ratio exceeded 20% during the pre-policy period (2008–2011). We also exclude firm-year observations corresponding to the years in which a firm was founded or exited the market, as these could otherwise confound our analysis of pay, hiring, and separations. Finally, to enhance comparability between the treatment and control groups, we restrict the sample to firms with at least ten employees in every pre-policy year. This ensures a more balanced size distribution, since—as discussed in the next subsection—control firms tend to be considerably smaller than banks.

3.2 Treated and control sectors

Our analysis focuses on firms operating in the financial sector, defined according to the Dutch Firm Industry Classification (Standaard Bedrijfsindeling; SBI) as all 5-digit industries with codes from 64000 to 66300.[11] Within these financial industries, we distinguish between sectors that are subject to the 20% bonus cap (treated sectors) and sectors that are not covered by the cap (control sectors). This classification follows the report by Weitzel et al. (2018) – prepared in collaboration with the Dutch Ministry of Finance – to identify affected and unaffected industries at the 5- digit industry level. Under their definition, the Wbfo cap applies to banking, insurance, and a small heterogeneous group of other financial firms, whereas pension funds and the majority of investment firms are not covered by the Wbfo. These excluded sectors are instead subject to the EU-wide remuneration framework introduced under CRD IV.[12] Appendix Table A2 lists all 5-digit industries and their treatment status.

[9] Both anecdotal and data evidence suggest that remuneration practices began to change well before formal enforcement. For example, ING Group began discussing adjustments to remuneration practices as early as 2014, as documented in its Annual Report for that year (ING, 2014).

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

[11] The Dutch Industry Classification follows the EU NACE Rév. 2 up to the 4-digit level.

[12] In the Netherlands, the remuneration provisions introduced by the CRD-IV were applied to a broad set of financial institutions through the Regulation on Sound Remuneration Policies (Rbb Wft 2014). As a result, both treated and control industries are subject to the general CRD IV remuneration reforms. The key difference is that, in treated industries, the Wbfo imposes an additional and stricter constraint on variable pay by replacing CRD IV's 100% cap (200% with shareholder approval) with a binding 20% cap.

Building on the classification by Weitzel et al. (2018), we refine the treatment and control definitions by excluding financial industries subject to other remuneration regulations, ensuring that the *only* regulatory difference between treated and control sectors is the Wbfo. Specifically, from the control group we remove pension funds, which are governed by the Pensions Act introducing its own remuneration rules in 2014 and revised in 2018, potentially confounding the identification of the bonus cap's effects.[13] From the treated group, we exclude the insurance sector, which is regulated under the European Solvency II regulation imposing separate remuneration requirements, as well as the small residual category of "other treated sectors" (see Appendix Table A2). After these refinements, the treated group refers unambiguously to banks, facilitating the interpretation of the estimated effects. These exclusions do not affect our results, since, as shown in Table A2, these industries are significantly smaller than the banking sector: in 2011, the omitted industries employed about 4,000 workers, compared with over 38,500 employees in banking.

Table 2 summarizes the composition of the treated and control sectors. By construction, the treated group consists exclusively of banks.[14] The control group is more diverse: roughly 46 percent are investment firms, 29 percent operate in risk and damage evaluation, and 12 percent are participation companies such as private equity and venture capital firms. The remaining firms provide other financial services or act as financial holdings. Treated firms are also substantially larger. While the treated group includes 32 entities, it has an average of 985 employees and a median of 51 employees, reflecting the presence of a small number of very large institutions. By contrast, the 54 control financial institutions are much smaller on average (mean 41; median 18).

3.3 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.[15]

Variable pay in our dataset is measured as the sum of two components: variable

[13] We also exclude the Central Banking and Trust Offices sectors, as they are not covered by the Dutch Sound Remuneration Policies (Regeling beheerst beloningsbeleid (Rbb) and Wet op het financieel toezicht (Wft)) implemented in 2011.

[14] Employment in the treated group is roughly evenly split between cooperative banks and commercial retail banks. The large share of employment in cooperative banks is driven by Rabobank, which is one of the largest banks operating in the Netherlands and is organized as a cooperative.

[15] We deflate all pay measures and express them in real terms using the 2015 Dutch consumer price index. After selecting employees in the finance industry, we apply winsorisation to all continuous outcome variables, capping extreme values at the 1st and 99th percentiles of the respective variable to mitigate the influence of outliers.

cash payments and variable in-kind payments. Variable cash payments consist of bonuses paid in cash, while variable in-kind payments include bonuses awarded in financial instruments such as stocks. Stock-based bonuses trigger an income tax event at the time of award, and thus they are recorded with no timing lag in our data. By contrast, stock options are not taxed when granted but only when they are exercised at the employee's discretion, regardless of whether the resulting shares are immediately tradable. As a result, compensation paid in the form of stock options may be recorded with a lag in our dataset, potentially introducing some imprecision in the timing of measured variable pay. This limitation is, however, not empirically relevant in our setting. Stock options were very rarely used by Dutch banks during our period of analysis. In particular, annual reports from the three major Dutch banks indicate that ABN AMRO and Rabobank did not use stock options at all during the sample period, while at ING the compensation scheme that included stock options (the "Long-Term Incentive Plan") was discontinued after 2010. Since no new stock options were issued during our sample period, any timing issues in the recording of variable pay due to stock options are minor and affect only a very small number of ING employees.[16]

Another potential source of timing mismeasurement in observed variable pay arises from the deferral provisions introduced under CRD III (from 2011) and subsequently tightened under CRD IV (from 2014). These rules require that at least 40% of variable remuneration be deferred for three to five years for *identified staff*. From a regulatory perspective, deferred bonuses are counted toward the bonus cap in the year of award, so the policy binds immediately at the time compensation is promised. From a tax perspective, however, deferred pay is typically recorded only at vesting, which may generate a lag between the year of award and the year in which variable pay is observed in our data. Three considerations substantially limit the empirical relevance of this concern in our setting. First, CRD III/IV deferral rules apply to both treated and control industries, so any lag in measured variable pay is a common feature of both groups and does not differentially bias our treatment-control comparison. Second, the deferral requirement applies only to identified staff, who account for a very small share of employment—typically around 1% of bank employees, according to information reported in banks' annual reports. Given the small size of this group and our focus on the entire workforce rather than exclusively on identified staff, any resulting timing mismeasurement is unlikely to materially affect our results. Third, if anything, deferral should work against finding an immediate decline in observed variable pay following the reform announcement. Because a portion of bonuses awarded prior to the introduction of the bonus cap continues to vest in subsequent years—with the first vesting occurring only from 2014 due to the three year deferral requirement—deferral

[16]The predominance of cash-based variable pay in our data is confirmed by the descriptive statistics reported in Table 3.5 and discussed in Section 3.5. More generally, the European Banking Authority reports that it is common practice for banks to pay bonuses below 50,000 euros entirely in cash.

mechanically smooths the transition to the post–bonus-cap regime, attenuating short-run declines in measured variable pay.

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. In our analysis, this includes for example the 'standard' end-of-year bonus and holiday 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-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.

3.4 Measuring 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 use the incumbent sample and classify individuals in both the treated and control group as either "talented" or "non-talented" based on their estimated residual salaries, obtained from 2008-2011 data using the following model:

$$log(totalsalary)_{ist} = \alpha + \beta SEX_i + \lambda AGE_i * EDUCATION_i + \eta_s + \phi_t + \epsilon_{ist} \quad (1)$$

where i represents the individual, s represents the economic sector and t represents the calendar year. 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)[17] to capture any age-education heterogeneity in total wages. In addition, we include 5-digit sector fixed effects η and year fixed effects ϕ.

An individual with a high positive residual is earning more than what the model would predict considering the individual's sex, age-education group, industry of employment, and year. We define talent as a binary indicator equal to one for individuals who are in the top 10% of the residual distribution in 2011. We construct this measure in two ways. First, we rank workers in the pooled sample of treated and control firms in our analysis and classify as *absolute talent* those in the top decile of the residual distribution. This measure captures top performers relative to the overall population

[17]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).

of workers observed in our sample of analysis. Second, we rank workers within each firm and classify as *relative talent* those in the top decile of their firm's residual distribution. This measure captures firm-specific performance by identifying workers who are top earners relative to their coworkers, allowing the level of talent to differ across firms. Together, these two measures allow us to distinguish whether the effects of the bonus cap on pay schemes differ for workers who are top performers relative to their firm or top performers in the finance sector overall.

3.5 Descriptive statistics

Table 1 reports descriptive statistics on pay composition and workforce characteristics of the incumbent sample, measured in 2011. Panel A shows that, in the year prior to the policy announcement, bankers in our sample earned an average total compensation of 96,000 euros, while finance workers in the control firms earned 122,000 euros on average. Variable pay accounted for 11,400 euros for bankers and 29,900 euros for workers in the control group. On average, the ratio of variable to fixed pay was 10.7% for bankers and 13.7% for control-group employees. Consistent with these distributions, nearly 10% of bankers had a variable-to-fixed pay ratio above the 20% threshold in the year before the policy announcement, compared with nearly 20% of workers in the control industries. Variable pay is almost entirely paid in cash for workers in both the treated and control groups.[18]

Focusing on workers identified as top talent according to the absolute talent measure, we observe substantially higher levels of compensation. Average total compensation in this group amounts to nearly 225,000 euros for bankers and more than 434,000 euros for control financial workers. Of these amounts, over 52,000 euros and nearly 211,000 euros, respectively, correspond to variable pay. Consequently, variable pay as a share of fixed pay is considerably higher among top-talent workers, reaching nearly 26% in the treated group and 37% in the control group. As a result, the fraction of workers whose variable-to-fixed pay ratio exceeds the bonus cap threshold in the year prior to the policy announcement is 46.8% among bankers and 65.2% in the control group.

Finally, Panel B shows that both the treated and control groups are male-dominated. Women account for only 22.5% of bankers and 15% of workers in the control group. These shares decline further when focusing on the top-talent subsample, to 14.3% and 8.4%, respectively. Workers in both groups are on average around 45 years old, and more than 90% are Dutch natives. Finally, approximately 40% of workers in both the treated and control groups hold a tertiary degree.

[18]Specifically, less than 1% of variable pay is paid in financial instruments in the treated group, making any potential timing issues related to stock-based compensation empirically negligible in our setting.

3.6 Full sample

To assess the policy's impact on the bank's ability to attract workers, we analyse 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 Netherlands during our sample period.[19] 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.

4 Variable pay as pay for performance

Sectoral incidence of variable pay: We begin by documenting the incidence of variable pay across sectors in the Netherlands. For 2011, the last full year before the announcement of the variable pay cap, we calculate for each economic sector the share of employees receiving at least 1,000 euros of variable pay. Figure 1 shows that among sectors that account for at least 1% of overall employment, financial institutions stand out. In this sector, 61% of employees are paid some form of variable pay. This is by far the highest incidence of variable pay across the economy. The only sector with a comparable level is ICT, but even there the share is about 10 percentage points lower. In all other sectors, the incidence of variable pay is at most around half of that observed in financial institutions. The fact that variable pay is most prevalent in finance provides suggestive evidence that it is used as pay for performance, consistent with the extensive use of performance-based compensation schemes in this sector.

We now conduct three exercises to assess whether variable pay indeed rewards performance. We focus on the finance sector, though results are similar when extending the analysis to the full economy.[20]

Variable pay variability within job spells: We then turn to the question of whether variable pay in practice behaves like an incentive instrument, or whether it simply reflects a predictable quasi-fixed component of overall pay. To do so, we examine how variable pay fluctuates within job spells in the entire financial sector. If variable pay is genuinely tied to performance, we would expect to observe substantially greater within-spell variation in variable pay than in fixed pay, even after netting out job-spell-specific age trends. Table 3 reports the residual standard deviation of variable and fixed pay for the pre-policy period 2008–2011. We measure both components in levels to accommodate spells with zero variable pay. In column 1, where we include spell

[19] 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 in fixed pay, and was aged 18 or older.

[20] See Appendix Table A3 and A4.

fixed effects to allow each job spell to have its own intercept, we find that the within-spell standard deviation of variable pay is roughly four times larger than that of fixed pay. Importantly, this result does not disappear when we add controls for life-cycle variation and business cycle fluctuations: including nonparametric age-bin trends does not change the magnitude of the difference (column 2), nor does the inclusion of year fixed effects (column 3). Extending the sample in column 4 to cover the years after the introduction of the cap (i.e., the period 2012–2019), we observe a decline in the relative volatility of variable pay, which falls to 2.9, consistent with the cap reducing the scope for variable pay variation. Finally, in column 5 when we go back to the pre-policy period and include differential age trends across job spells allowing each job spell to have both its own intercept and its own age profile, the ratio remains equal to four, strengthening the conclusion that variable pay fluctuates substantially more than fixed pay within job spells. These results support the view that variable pay operates as a flexible, performance-linked component of compensation, rather than as a stable extension of the base salary.

Variable pay dispersion across workers in a given job: We then test a further implication of incentive pay models claiming that, if variable pay is used to reward worker performance, the cross-sectional dispersion in variable pay across individuals within a job should exceed the dispersion in fixed pay (Lazear, 1999; Tang et al., 2023). To assess this prediction, we focus on the pre-policy period 2008–2011 and compare the dispersion of residual variable pay with that of residual fixed pay across individuals employed in the same job. To proxy for a job, our empirical strategy includes fixed effects defined at the combination of firm, sex, age group, native status, education level, and the year the worker started the job-spell, thereby ensuring to compare employees in very similar positions. The results show that residual dispersion in variable pay is 1.53 times greater than in fixed pay (Table 3, column 6). Restricting the analysis to workers who earn at least one euro in variable pay in column 7, the ratio rises to 1.62. These findings align closely with the prediction: variable pay is more dispersed across workers within the same job, consistent with its role in rewarding higher performance.

Higher returns to observables within variable pay jobs: Finally, we implement a test proposed by Lemieux et al. (2009), which provides an additional way to assess whether variable pay rewards performance.[21] The intuition is that if variable pay is used to compensate workers according to their productivity, then the returns to observable productive characteristics, such as education and experience, should be higher in jobs with variable pay than in jobs without it. To investigate this, we divide

[21] As in our case, Lemieux et al. (2009) do not observe pay for performance directly. Instead, they classify as potential performance-pay jobs those spells that include variable pay, bonuses, commissions, or piece rates in at least one year.

jobs into two categories—those in which variable pay is at least 1,000 euro and those in which it is less—and run separate Mincerian wage regressions for the two groups. Following Lemieux et al. (2009), the dependent variable is log hourly wages, and the explanatory variables include years of education, a cubic polynomial in experience, a squared polynomial in tenure, a married dummy, and an indicator for being born in the Netherlands. We also control for year, sector and year-municipality fixed effects throughout and cluster standard errors at the job spell level. Results are reported in Table 4.

The estimates show clear differences between the two groups of jobs. In OLS regressions on the two separate samples, the returns to both education and experience are higher in jobs that include variable pay (column 1 and 2). Specifically, an additional year of education increases log hourly wages by 0.094 in variable-pay jobs, compared with 0.061 in fixed-pay jobs. These magnitudes are consistent with the estimates of Lemieux et al. (2009) using U.S. data. Similarly, when we evaluate the experience polynomial at the sample-specific mean, we find a coefficient of 1.026 for variable-pay jobs and 0.899 for fixed-pay jobs. These differences are statistically significant when we pool the two groups together in a single regression and include interactions with a variable-pay dummy (column 3). Turning to tenure, the results point in the opposite direction: returns to tenure are lower in jobs with variable pay. This finding, which mirrors the conclusions of Lemieux et al. (2009), suggests that tenure is best interpreted as a characteristic of the job rather than of the worker. Leveraging the richness of our data, we extend the analysis of Lemieux et al. (2009) and show that the pattern of higher returns to education and experience in jobs with variable pay remains robust when firm fixed effects are added to the pooled regression (column 4). We further estimate a specification including individual fixed effects (column 5). Experience still shows larger returns in variable-pay jobs and no significant effects in the other group of jobs. In this specification, tenure behaves more similarly to experience, showing higher returns in variable-pay jobs. For education, the coefficient on years of schooling cannot be identified, as education does not vary within individuals. However, the interaction between education and the variable-pay dummy remains positive and significant. Because this effect is identified from movers between fixed- and variable-pay jobs, it indicates that workers who switch into variable-pay jobs experience higher returns to education. This evidence let us conclude that in our setting variable pay is likely to function as pay for performance.

5 Empirical strategy

5.1 Identification challenges and strategy

We implement a difference-in-differences (DiD) strategy to study the effects of the introduction of the bonus cap, comparing treated and control sectors before and after the policy change. The key identifying assumption is that, absent the policy, outcomes in the two groups would have followed parallel trends. While this assumption is not directly testable, our dynamic DiD specification allows us to assess its plausibility by inspecting multiple years of pre-reform data.

A potential concern within our design is the presence of bank-specific shocks in the post-reform period that may be unrelated to the bonus cap but differentially affect treated firms. This issue is particularly relevant because our control group consists of financial institutions that are not banks.[22] We address this concern in several ways, including by complementing our baseline analysis with an alternative identification strategy that exploits only within-bank variation, thereby isolating the effect of the policy from sector-level confounding shocks (see Section 6.1.1).

Beyond this concern, we discuss a few additional issues that may arise in our empirical setting. A first challenge is the potential presence of anticipation effects. Although the bonus cap came into force between 2015 and 2016, it was announced in 2012. Firms and workers may therefore have adjusted their behavior prior to the implementation. To account for this, we adopt two strategies. First, as previously explained, we define the sample of incumbents as workers employed in banks in 2011 or earlier, i.e., before the policy was announced. Second, we employ a dynamic DiD covering the period 2008–2019, which allows us to separately examine outcome trajectories around the announcement (2012–2015) and the implementation (2016–2019) phases of the reform.

A second challenge is that changes in firm-level pay structures may reflect not only adjustments to incumbents' pay, but also changes in workforce composition. For example, following the bonus cap, firms may hire different types of workers or experience selective separations, which can affect the level and composition of pay independently of changes in pay-setting induced directly by the policy. To isolate changes in pay structures driven by the bonus cap from those driven by changes in hiring, our baseline analysis of pay composition focuses on incumbents and is then complemented by an analysis of hiring and retention, disaggregated by ability group.

[22] Examples of such bank-specific shocks include the implementation of Basel III capital and liquidity requirements, prolonged compression of net interest margins in the low-interest-rate environment, and heightened government oversight of banks following the crisis (including partial state ownership). These shocks could have independently reduced banks' profitability and, in turn, affected bankers' compensation and separation decisions, thereby confounding the estimated effects of the bonus cap.

A third challenge concerns spillovers arising from job-to-job transitions between treated and control firms. Because the bonus cap applies only to treated firms, employees may respond by moving to firms not subject to the cap, potentially "contaminating" the control group. We assess the importance of these spillovers in our separation analysis, where we directly test whether the introduction of the bonus cap affects transitions from treated to control firms. In addition, we examine job-to-job transitions in the pre-policy period (2008–2011), to assess the degree of labor market integration between treated and control sectors. As shown in Table A5 the two groups were not strongly interconnected through labor flows prior to the reform. Finally, in our dynamic DiD analysis, we present outcome dynamics separately for treated and control groups alongside the DiD estimates to verify that the estimated effects are driven by changes in the treated group and that outcomes for the control group evolve smoothly around the reform.

In the next subsection, we describe the main empirical specifications used in the different parts of the analysis.

5.2 Analysis of Incumbents

5.2.1 Empirical models on pay

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

$$Y_{ift} = \alpha + \sum_{t=2008}^{2019} \delta_t \left(Bank_{if} * Year_t \right) + \gamma_{if} + \phi_t + \epsilon_{ift}, \tag{2}$$

where Y_{ift} represents one of our pay outcomes. We absorb all time-invariant characteristics of the worker, the firm, and their match by including job-spell fixed effects, γ_{if}. As a result, identification relies exclusively on within–job-spell variation, capturing changes in compensation that occur while an employee remains with the same employer. The coefficients δ_t capture the dynamic treatment effects and measure whether pay-setting at firms subject to the bonus cap evolved differently over time relative to control firms.[23] Standard errors are clustered at the job-spell level.[24]

To assess the outcome dynamics separately for treated and controls, we also estimate the following specification for each group:

[23] In a robustness check, we augment the specification with a vector of time-varying individual and job characteristics X_{ift}, including indicators for age (eight five-year bins), marital status (married or in a de facto partnership), parenthood, and firm size (1–19; 20–199; 200–499; 500 or more employees).

[24] A more conservative approach would cluster standard errors at the treatment-group level (i.e., two clusters) or at the five-digit SBI or firm level. However, the number of 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. As shown in our analysis, employees with a ratio below 20 percent are unlikely to be affected by the bonus cap.

$$Y_{ift} = \alpha + \sum_{t=2008}^{2019} \beta_t \, Year_t + \gamma_{if} + \epsilon_{ift}. \tag{3}$$

The coefficients β_t capture the evolution of the outcome variable over the period 2008–2019, relative to the reference year 2011. We consistently present results based on Equation (3) separately for treated and control employees to transparently assess whether the estimated DiD effects are driven by changes in the treated group rather than by shifts in the control group. This exercise also provides an additional diagnostic for potential spillovers between treated and control firms discussed above.

5.3 Analysis on attraction and retention

To investigate the effect of the bonus cap on banks' ability to attract and retain workers, we examine firm-level hiring and separations and then analyze which categories of employees are most affected by these adjustments.

5.3.1 Attraction and retention at the firm level

We use a panel of firm-year observations in which hiring and separation outcomes are computed using the full workforce, including employees who join after or leave before the policy announcement. We estimate the following specification:

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

where f and t index firms and years. The coefficients of interest, δ_t, capture the dynamic differential effect of the bonus cap on the ability of treated firms, relative to control firms, to attract and retain workers. The terms γ_f and ϕ_t denote firm and year fixed effects, respectively. We use analytical weights based on firm employment in 2011 so that larger firms receive greater weight, and cluster standard errors at the firm level.

5.3.2 Attraction and retention of employees

To analyze the retention of incumbent workers and the attraction of new hires, we estimate specifications analogous to those in (3) and (2), but without job-spell fixed effects. The exclusion of spell fixed effects reflects the nature of the outcome variables in these analyses.

To study worker retention, we focus on incumbent workers and define the outcome as a binary indicator equal to one in the year a job spell ends and zero in all preceding years. Because this outcome exhibits no within–job-spell variation except in the

terminal year, job-spell fixed effects are not included. The observation window spans 2011–2019, since incumbents—by construction—cannot exit before 2012. Individuals remain in the sample until their job spell ends.

To analyze the hiring, we study changes over time in workers' probability of joining treated versus control firms. Using data from the full period 2008–2019, we retain only the observation corresponding to the year in which a worker starts a job. As each worker contributes a single observation, job-spell fixed effects are again not included. This approach amounts to estimating a difference-in-differences specification on repeated cross-sections of hires.

6 Earnings of Incumbent Workers

6.1 Changes in the Pay Structure of Incumbent Workers

Figure 2, panel (a), shows that by 2019 the probability that the variable-to-fixed pay ratio exceeded 20% was 5.6 percentage points lower in banks relative to their baseline level. This corresponds to a decline of nearly 60% compared with the banks' baseline average of 9.6.[25] At baseline, the control group already exhibited a 9.6 percentage-point higher probability of having a variable-to-fixed pay ratio above 20%. Following the policy, this gap widened by an additional 15 percentage points, more than doubling the initial difference between banks and the control group.

This evidence is complemented by panel (b) of the same figure, which plots the trends in the continuous variable-to-fixed pay ratio. By 2019, this ratio was about 8 percentage points lower in banks relative to their baseline average of 10.7%, meaning that variable pay declined from roughly 11% to 3% of fixed pay. When we consider the difference between treated and control employees, the estimates indicate that this gap widened by about 13 percentage points, representing a more than fivefold increase relative to the baseline difference of 3 percentage points in favor of the control group.

A decline in the variable-to-fixed pay ratio among bankers can occur through a reduction in variable pay, an increase in fixed pay, or a combination of the two. In

[25] A less than perfect compliance is expected in our setting for several reasons. First, the Dutch framework contains explicit carve-outs and scope rules under which a higher cap than 20% can apply. In particular, employees who work predominantly abroad (more than 50% of hours) can fall under the European remuneration regime (CRD IV) or, for work outside the EEA, face an even higher ceiling (up to 200%). Second, employees in the Netherlands whose employment conditions are not fully covered by a collective labor agreement may face a higher domestic cap (up to 100%). Third, staff employed at Dutch-based parent/holding entities of international groups can be subject to the European regime when most group employees work outside the Netherlands for a sustained period. Fourth, branches in the Netherlands of EU-headquartered credit institutions or investment firms are generally governed by the European framework rather than the Dutch 20% cap. Finally, compliance is not perfect: for instance, De Nederlandsche Bank imposed a €15 million fine on ABN AMRO for breaching statutory bonus restrictions.

what follows, we separate these components and examine their implications for overall compensation. Figure 3, panel (a), analyzes trends in variable pay normalized by total pay. We observe a substantial decline in the relative importance of variable pay in bankers' pay structure: by 2019, this share had fallen by 6.6 percentage points, corresponding to a reduction of roughly 75% relative to its baseline average of 8.9%. In comparison to the control group, this implies that the relative importance of variable pay for bankers declined by nearly 10 percentage points, relative to a baseline difference between treated and control employees of 1.7 percentage points percentage points.

Mechanically, a decline in the variable-pay share implies a corresponding increase in the share of fixed pay. To assess whether this compositional shift reflects an increase in fixed pay in levels, Figure 3, panel (b), plots fixed pay normalized by baseline total compensation measured in 2011. By holding the denominator constant over time, this normalization isolates changes in fixed pay itself rather than mechanical movements induced by changes in variable pay. The results show that bankers' fixed pay grew faster than that of workers in the control group. By 2019, bankers' fixed pay had increased by 12.6 percentage points relative to its 2011 level, corresponding to a 14% increase with respect to the baseline fixed-pay share of 91%. In difference-in-differences terms, the estimated increase in fixed pay amounts to 8 percentage points by 2019. This effect is roughly five times larger than the baseline treated–control difference in fixed-pay shares observed in 2011 (91.1% in the treated group versus 89.4% in the control group).

Taken together, the evidence from panels (a) and (b) of Figure 3 indicates that banks responded to the bonus cap by increasing fixed pay as a way to compensate for the reduction in variable compensation. This raises the question of whether this adjustment was sufficient to fully offset the decline in variable pay—a question we address in panel (c), which tracks the evolution of total pay for treated and control employees, normalized by its baseline value. The results show that total pay for treated employees experiences a downward level adjustment around 2014 and subsequently evolves in parallel with that of control workers, remaining consistently lower throughout the post-reform period. In particular, while total pay increased relative to 2011 in both groups, by 2019 this increase was 5.3 percentage points smaller for bankers than for control employees. These findings indicate that the adjustment in fixed pay only partially compensated for the reduction in variable pay, leaving bankers' overall earnings persistently lower relative to control workers in the post-policy period.

Next, we investigate whether the aggregate results described so far conceal important heterogeneity across workers. In particular, we examine whether changes in pay composition differ by workers' ability. As emphasized by Lazear (2000), performance-based pay serves not only to raise average productivity through stronger incentives, but also to attract and retain more able workers, who can translate their higher ability

into greater earnings under such schemes. Understanding how the pay of high-ability workers adjusts following the reform is therefore crucial before turning to the analysis of separation and hiring patterns in Section 7.

Figure 4 compares changes in pay composition between treated and control workers by talent, measured using our absolute ability measure defined in Section 3.4. All previously documented changes in pay composition are markedly more pronounced among high-ability workers. Panel (a) shows that, by 2019, the probability that the variable-to-fixed pay ratio exceeded 20% was 34.5 percentage points lower for treated high-ability workers relative to their control counterparts, whereas the corresponding decline among the remaining (lower-ability) workers was only 13.1 percentage points. This difference-in-differences estimate for high-ability workers reflects a 33.3 percentage point reduction in the probability for treated employees (not shown in the graph), corresponding to a 71.1% decline relative to their baseline average of 46.8% (compared with a baseline average of 65.2% in the control group).[26]

Consistent with the evidence in panel (a), panels (b) and (c) show similarly pronounced adjustments in the variable and fixed pay of high-ability workers. In particular, panel (b), which examines changes in the relative importance of variable pay between treated and control workers, shows that by 2019 variable pay normalized by total pay was 15.8 percentage points lower for high-ability treated workers relative to control workers. This difference is driven primarily by changes in the pay structure of treated employees, who experience by a 12.3-percentage-point reduction in the relative importance of variable pay (not shown in the figure), corresponding to a 66.5% decline relative to their baseline average of 18.5% (compared with a baseline average of 24.8% in the control group).[27] Panel (c) presents the corresponding evidence for fixed pay normalized by baseline total pay. By 2019, fixed pay was 17.2 percentage points higher for high-ability treated workers than for their control counterparts when measured relative to 2011 total compensation. This difference is again driven primarily by changes in the pay structure of treated workers, whose fixed pay increased by 18.9 percentage points relative to baseline total pay, corresponding to a 23.2% increase relative to the baseline average of 81.5%.

Finally, panel (d) shows the implications for total pay normalized by its baseline value. For high-ability treated workers, total pay in 2019 was 9.6 percentage points lower than that of high-ability control workers. This difference primarily reflects the slower growth in total pay among treated workers, whose total compensation increased by only 8.5 percentage points between 2011 and 2019, compared to a 18.1 percentage

[26]For low-ability treated bankers, the baseline average was 5.4%, and the decline observed in 2019 was 3 percentage points, corresponding to 55.5% decline relative to the baseline average.

[27]The corresponding difference-in-difference estimate for low-ability workers is 9 percentage points, with a decline of 6.1 percentage points for the treated group, corresponding to a 77.2% decline relative to the baseline average of 7.9% (compared with a baseline average of 9% in the control group).

point increase for their control counterparts. Among workers outside the high-ability group, total pay grew at a mostly similar rate across treated and control firms. Taken together, this evidence indicates that although increases in fixed pay largely offset reductions in variable pay for most workers, this was not the case for bankers in the top 10% of the talent distribution in the finance industry. These high-ability workers experienced a substantial slowdown in earnings growth relative to top talent in control firms. The results are both quantitatively and qualitatively similar when workers are classified into high- and low-ability groups based on the within-firm talent distribution, as shown in Figure A2.

6.1.1 Robustness

A potential concern with our baseline specification is that banks might have followed a different trend from the control group for reasons unrelated to the bonus cap. In particular, banks were simultaneously subject to other regulatory changes, such as the implementation of CRD IV, which could have affected bank performance and, in turn, compensation and retention policies. A decline in performance associated with these reforms could, in principle, also be consistent with a reduction in bankers' variable pay.

Our baseline results, however, already point against a simple bank-shock interpretation. The decline in variable pay among bankers following the introduction of the bonus cap is accompanied by a simultaneous and relatively stronger increase in fixed pay compared to the control group. This pattern is inconsistent with a broad contraction in pay due to financial distress or sectoral shocks, and instead suggests a rebalancing in the composition of compensation—away from variable and toward fixed pay.

To further address this concern, we evaluate three implications of the bank-shock alternative. If bank-specific shocks were driving the results, we would expect: (i) a broad contraction in pay within banks; (ii) declines in variable pay that are not systematically related to exposure to the 20% threshold; and (iii) no particular reason for fixed pay to rise more for workers most exposed to variable remuneration. We test these predictions using a *within-bank* identification strategy. We restrict the sample to banking-sector employees and compare individuals who, prior to the policy announcement, had earned a variable-to-fixed pay ratio above 20% at least once (the treated group) with those whose ratio had always remained below this threshold (the control group). Because this comparison is made within the same bank and includes job-spell fixed effects, it absorbs bank-level shocks that affect all employees (including changes in performance or regulatory pressures such as CRD IV). The remaining variation captures differential exposure to the bonus cap within banks.

Results are reported in Table 5 and provide little support for the bank-shock ex-

planation. First, we do not observe a broad contraction in pay: total pay rises for all bankers after the announcement and implementation of the reform, and treated bankers experience faster growth than control bankers. Second, changes in variable pay are tightly linked to exposure to the cap: while variable pay declines for all bankers, the reduction is substantially larger for treated bankers. In the post-implementation period (2016-2019), their variable pay as a share of total pay declines by an additional 2.6 percentage points, over and above the 0.8 percentage point decline experienced by control bankers. Third, fixed pay increases disproportionately for the most exposed workers: treated bankers' fixed pay rises 3.6 percentage points faster than that of control bankers in the post-implementation period.

Taken together, these within-bank patterns are difficult to reconcile with bank-specific negative shocks. It is unclear why such shocks would lead to faster growth in fixed and total pay precisely for bankers with greater pre-reform exposure to variable remuneration. Instead, the opposite movements of variable and fixed pay within banks are consistent with banks substituting toward fixed remuneration when the bonus cap binds, thereby preserving overall earnings for the workers most affected by the policy.

7 Effects on retention and hiring

Following the introduction of the bonus cap, banks raised concerns that the regulation would limit their ability to attract and retain qualified workers. To assess the policy's impact on worker flows, we first examine its aggregate effects on firm-level hiring and separations and then analyze individual-level worker transitions to identify which groups were most affected by banks' workforce adjustment decisions.

7.1 Firm-level analysis

In this section, we present firm-level results from estimating (4). To study worker flows, we define the outcomes as the cumulative number of hires and separations relative to baseline employment, measured by total firm employment in 2011. Specifically, these variables capture the total number of hires or separations since 2011 as a percentage of a firm's 2011 employment level. To assess overall net changes in workforce size, we also examine the ratio of total annual employment to baseline employment. Panels (a), (b), and (c) of Figure 5 present these outcomes, respectively.

Figure 5 shows that, relative to control firms, banks experienced a marked slowdown in both hiring and separations following the introduction of the policy. The cumulative number of hires at banks begins to diverge from that of control firms in 2013, with a gap of 12.5 percentage points, which widens to 63.7 percentage points by 2019. A similar pattern emerges for separations: starting in 2014, cumulative separations at

banks are 10.5 percentage points lower than in control firms, with the gap reaching 52.8 percentage points by 2019. While the decline in separations shows an increase in staff retention at banks, the contraction in hiring is substantially larger. As a result, total employment relative to baseline levels declines more in banks than in control firms, as shown in Panel (c).

7.2 Individual-level analysis on retention: Incumbents at banks

We analyze employee retention and attraction at the individual level to assess whether the bonus cap disproportionately affected talented incumbent bank employees and whether it created an uneven playing field for talent between treated and control firms.

Table 6 reports estimates of whether the policy differentially affected the probability that incumbent workers leave their employer in treated versus control industries. The analysis distinguishes between voluntary separations (with and without early retirement) and involuntary separations, defined as cases in which the worker remains non-employed for the entire 365-day period following job-spell dissolution. Reported 2011 means provide context on the incidence of exit states and are computed using the full sample rather than the estimation sample. Columns (3) and (4) are mutually exclusive and together sum to Column (1).

Panel A shows that in the years following the announcement (2012–2015), incumbent employees were less likely to leave banks than control firms, consistent with the decline in separation rates documented in Section 7.1. This decline is primarily driven by a reduction in voluntary separations, while involuntary separations increase. The rise in involuntary exits suggests that banks actively adjusted to the policy by reducing employment, although employees' reduced propensity to quit more than offset this effect. This firm-side adjustment is consistent with the observed contraction in bank employment.

In the longer run (2016–2019), incumbent bank employees are about 2 percentage points more likely to separate than their counterparts in control firms. This increase is driven mainly by involuntary separations and early retirement. By contrast, when early retirement is excluded from the definition of voluntary separations, voluntary exit rates are similar between bank and control employees (see columns (2)–(4) of Panel A).

These patterns are broadly consistent across ability groups (Panels B and C). Taken together, while banks experience higher incumbent separation rates in the long run, these are primarily driven by involuntary exits—namely separations followed by prolonged non-employment or early retirement. We find no evidence of an increase in voluntary separations (excluding early retirement) among incumbent bank employees relative to controls following the policy.

Table 7 provides a more granular analysis of the effects of the bonus cap by distinguishing exit states observed within 365 days of job-spell dissolution. Columns (1) and (3) capture involuntary separations, while Columns (2) and (4)–(11) correspond to voluntary exits. Panel A shows that, among incumbents, bank employees are more likely than workers 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 separation. By contrast, bankers are less likely to switch treatment status within the financial sector—that is, to move from a bank to a non-capped financial firm (–0.9 percentage points; Column (7)).

Panels B and C reveal insightful patterns across ability groups of incumbents. First, early retirement is consistently more prevalent among bankers than among control incumbent employees across all groups, with particularly large effects for high-ability workers: those in the top 5 percent of the worker fixed-effect distribution are 6.5 percentage points more likely to retire early than comparable controls per effect of the policy (Panel C). Second, transitions to other industries (Column (4)) are more common among bankers, but this effect is concentrated among lower-ability workers, defined as those outside the top 5 percent (Panel B). Third, we find no evidence that bankers respond to the bonus cap by engaging in strategic job-to-job moves within finance to escape the regulation—for example, by switching to non-capped financial subsectors. Consistent with this interpretation, Columns (7) and (8), which capture within-finance transitions that alter the bonus-cap exposure, show no positive effects. Finally, exits into self-employment or migration abroad are small in magnitude and statistically insignificant.

Taken together, these results do not suggest that banks faced greater difficulties in retaining talent as a consequence of the policy, including among the most talented employees. This pattern is consistent with two complementary interpretations. First, increases in fixed pay have partly offset reductions in variable compensation, limiting voluntary departures even among high-ability workers. Second, mobility across financial subsectors may be constrained by skill specificity, making it difficult for bank employees to transition to other financial firms with more bonus-oriented pay structures.

7.3 Individual-level analysis on hiring

We analyze whether the bonus cap affected banks' ability to attract workers, with a particular focus on talented employees.

Table 8 reports estimates of how hiring by banks evolved relative to control firms over time. The analysis uses data from the full period 2008–2019 but retains only one observation per worker, corresponding to the year of hire. Hence, we implement a difference-in-differences strategy in repeated cross-sections of hires as detailed in

Section 5.3.2.

Panel A shows that, in the post-implementation period (2016–2019) relative to the pre-period (2008–2011), banks hired disproportionately more workers who were previously unemployed (Column (1)), came from outside the financial sector (Column (2)), or were employed at non–bonus-capped firms within finance (Columns (5) and (6)). In contrast, banks were less likely than control firms to hire workers from other bonus-capped firms within finance (–3.1 percentage points; Column (3)) and from abroad (–18.3 percentage points; Column (9)). The descriptive means provide context for these magnitudes: the decline in hiring from abroad represents more than a doubling relative to the 14.4 percentage point difference between banks and control firms in 2011. Since cross-border hires are typically more positively selected on skills and experience, this pattern is suggestive of a deterioration in the average quality of new hires at banks.

Since the number of high-talent hires is too small to precisely estimate effects at this level of disaggregation, we instead classify talent using income categories from the previous job. Table 9 reports results using four mutually exclusive income categories. Banks hired 10.7 percentage points more workers who previously earned between €50,000 and €100,000 than control firms (Column (1)). Relative to the 2011 difference—6.2 percentage points, given shares of 56.5 percent for banks and 62.1 percent for controls—this indicates that banks disproportionately attracted workers from the lower end of the earnings distribution. Consistent with this interpretation, Columns (2) and (3) show reductions in hiring workers who previously earned between €100,000 and €175,000 or more than €175,000. Finally, Column (4) shows no differential hiring of workers with missing prior income, suggesting no difference in hiring from entry states such as self-employment, abroad, or recent graduation. Since these hires cannot be classified into the four income categories, this finding is reassuring and indicates that selection into the past-income sample is unlikely to drive the results.

These results indicate that while banks experienced a reduction in hiring following the introduction of the bonus cap, the composition of new hires shifted in a way consistent with a deterioration in hire quality. Banks increasingly hired workers from unemployment, from outside finance, and from the lower end of the prior earnings distribution, while hiring fewer workers from abroad and fewer high-earning employees. Thus, the bonus cap appears to have weakened banks' ability to attract top talent.

8 Conclusions

This paper studies the labor market consequences of regulating incentive pay, exploiting the introduction of the Dutch bonus cap as a sector-wide reform that constrained variable compensation in banking. Using comprehensive administrative data, we pro-

vide causal evidence on how firms and workers adjust when the structure of pay—rather than its level—is directly regulated.

We show that the bonus cap substantially reshaped compensation. Variable pay declined sharply and was only partially offset by higher fixed pay, leading to persistently lower total compensation relative to comparable workers in control firms. These effects are significantly stronger for high-talent employees, for whom fixed pay increases did not compensate for the loss of bonuses. As a result, the policy compressed the earnings distribution within banks and reduced within-firm pay inequality.

The reform had asymmetric effects on on retention and hiring. Banks did not experience increased voluntary separations among incumbent workers, including high-talent employees, likely due to the sector-wide nature of the shock and the simultaneous deterioration of outside options within finance. By contrast, banks faced a large and persistent decline in hiring, particularly of high-skilled workers from abroad, from other capped banks, and from the upper end of the pre-hire earnings distribution.

Overall, our findings highlight an important trade-off of regulating incentive pay. While bonus caps reduce the use of high-powered incentives and compress within-firm pay inequality, they also weaken firms' ability to compete for talent in the external labor market. In our analysis, we show that incumbent workers do not respond to the bonus cap by leaving their employers, likely because the policy applies broadly within the banking sector and thus affects many relevant outside options simultaneously. This pattern suggests that a wider application of such rules could reduce talent reallocation and therefore mitigate the costs of the policy. More broadly, our results show that policies targeting the structure of pay can have substantial equilibrium effects on compensation and talent allocation, even in the absence of explicit limits on total pay.

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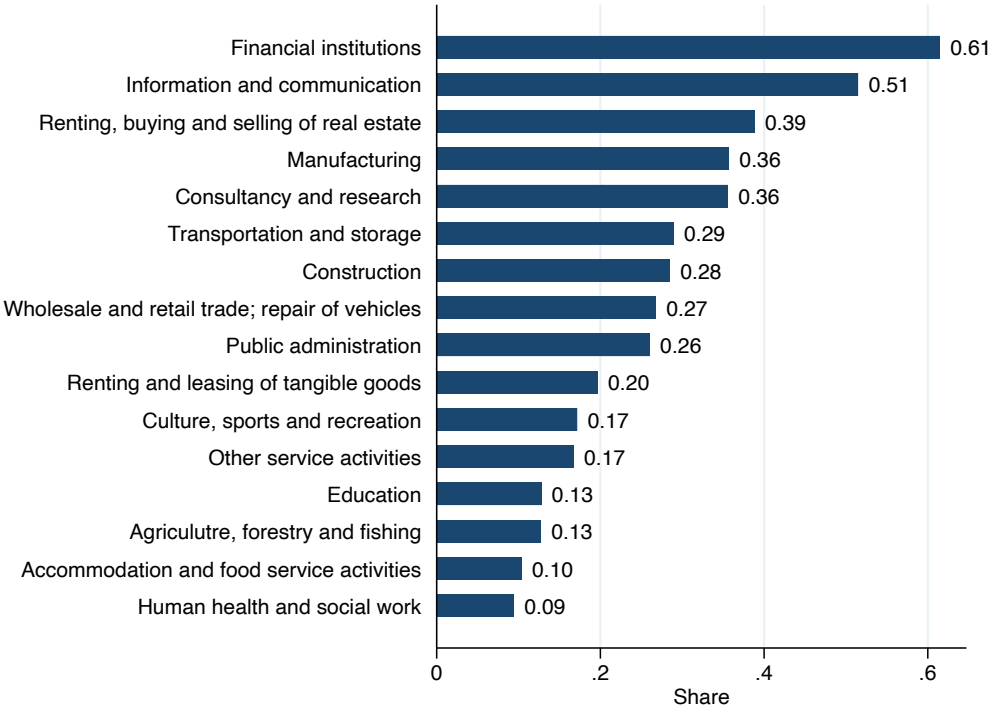
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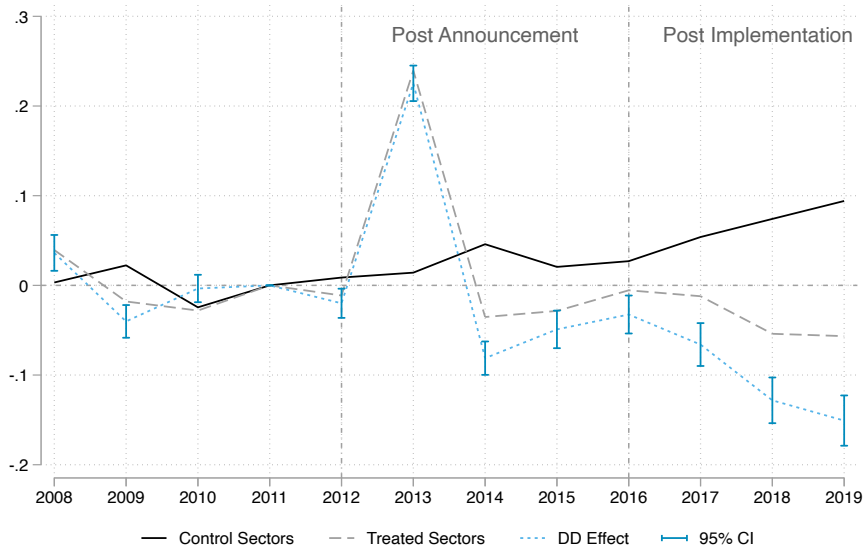
Figures

Figure 1: Share of employees paid variable pay

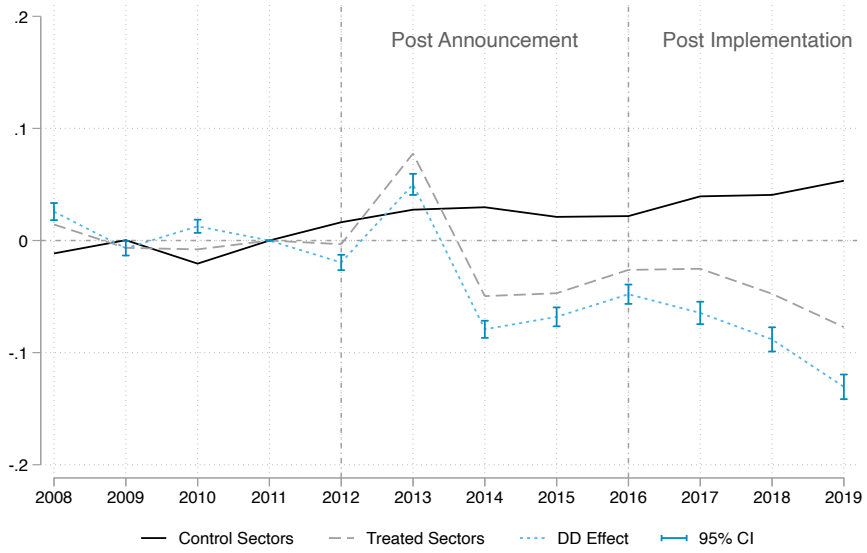


Notes. This figure plots the share of employees paid at least 1,000 euros of variable pay across sectors. Sectors with less than 1% of total employment are excluded.

Figure 2: Variable to Fixed Pay Ratio



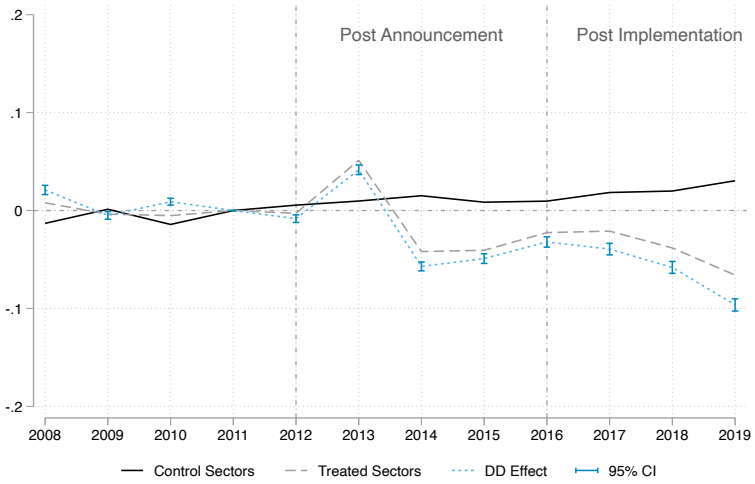
(a) Dummy for Variable to Fixed Pay Ratio > 20%



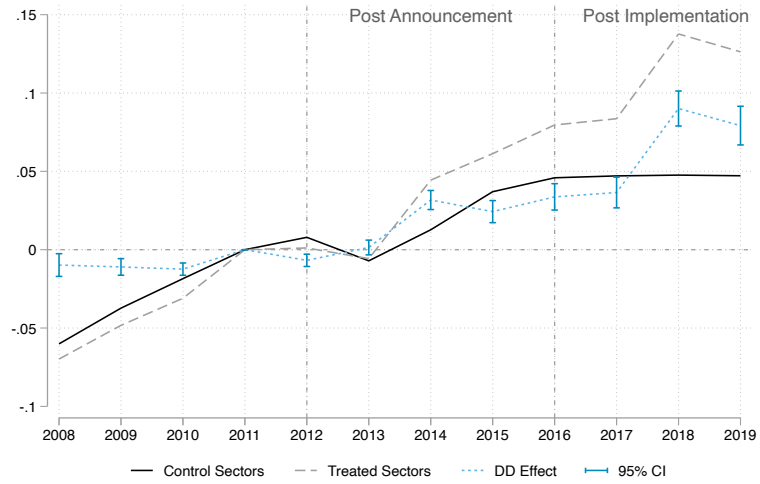
(b) Variable to Fixed Pay Ratio

Notes. This figure plots the regression coefficients and 95% confidence intervals from estimating equations (2) and (3) for the sample of incumbent workers. Incumbent workers are defined as individuals employed in the financial sector up to and including 2011. The dependent variables are an indicator for whether the variable-to-fixed pay ratio exceeds 20% in panel (a), and the continuous variable-to-fixed pay ratio in panel (b). The black line represent the evolution of outcomes for control sector employees relative to 2011 (equation 3). The dashed line represent the evolution of outcomes for treated sector (bank) employees relative to 2011 (equation 3). The blue dashed line represent the difference-in-differences estimates comparing treated and control sectors in each year relative to 2011 (equation 2). All regressions include job-spell fixed effects. Standard errors are clustered at the job-spell level.

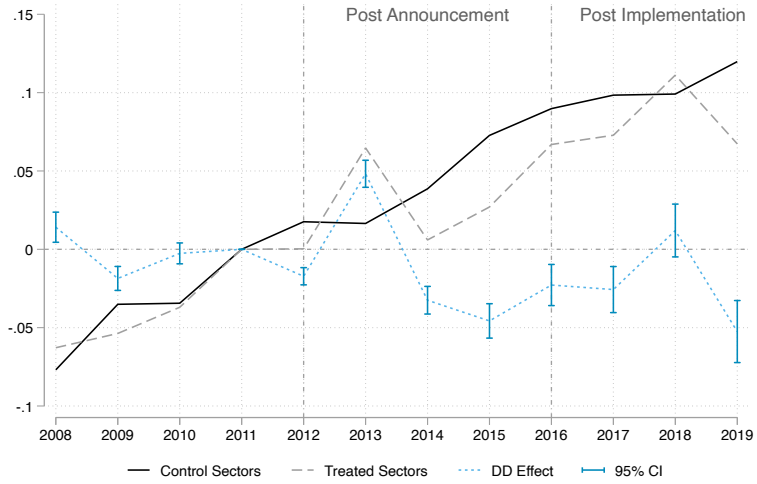
Figure 3: Variable, Fixed, and Total Pay



(a) Variable Pay over Total Pay



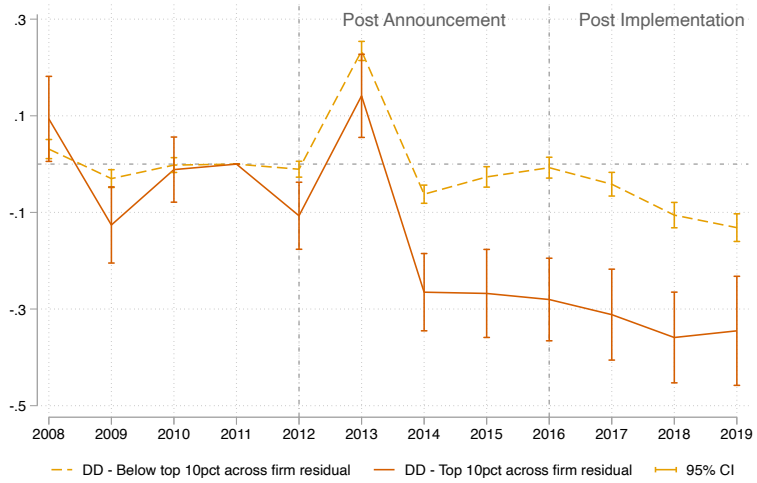
(b) Fixed Pay over Total Baseline Pay



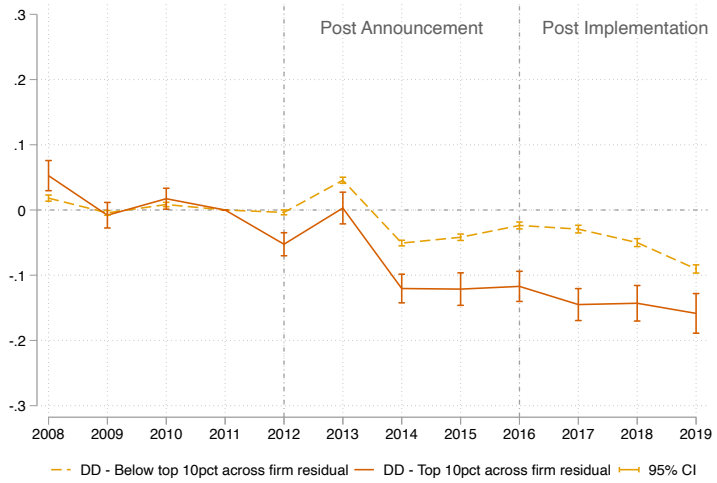
(c) Total Pay over Baseline Total Pay

Notes. This figure plots the regression coefficients as well as 95% confidence intervals from estimating equations (2) and equations (3) for the sample of incumbent workers. The dependent variables of interest are the variable pay over total pay in panel (a), fixed pay over total pay in panel (b), and total pay over baseline total pay (as measured in 2011) in panel (c). All regressions include the same fixed-effects and controls as in Figure 2. Standard errors are clustered at the job-spell level. The vertical dashed lines indicate the policy announcement period (October 2012) and implementation period (February 2015 for new hires, 2016 for existing employees).

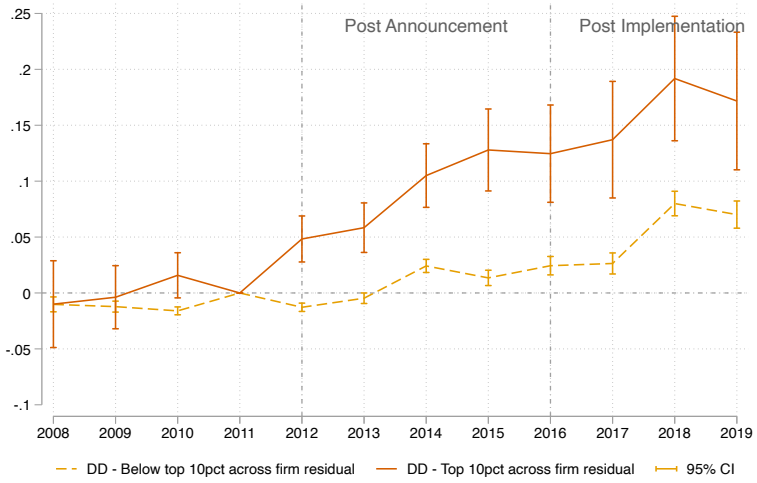
Figure 4: Effects on Pay Structure by Workers' Absolute Talent



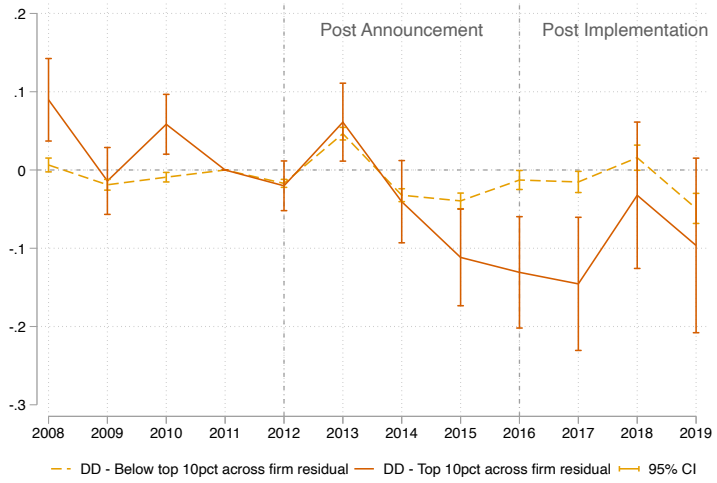
(a) Variable to fix pay ratio above 20%



(b) Variable pay over total pay



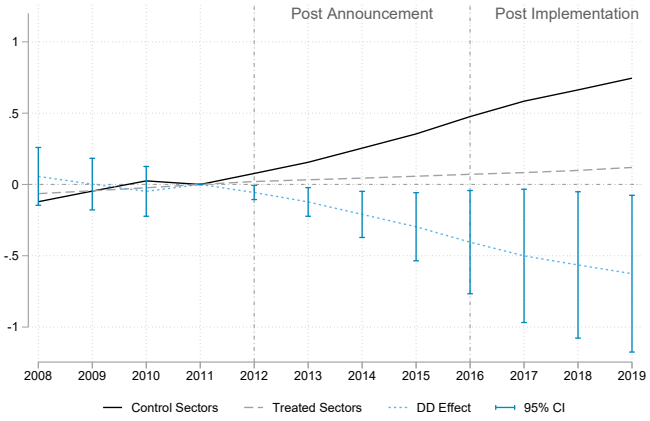
(c) Fixed pay over baseline total pay



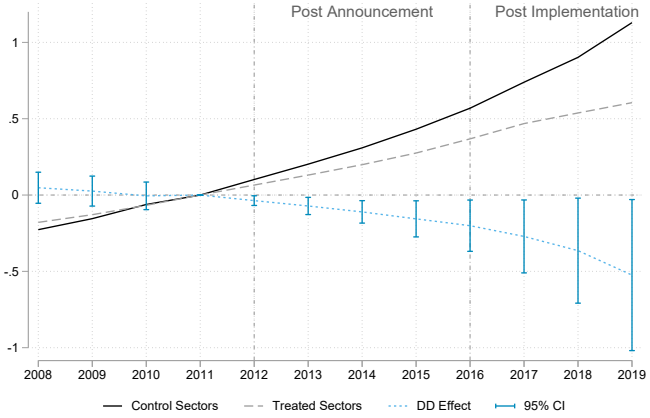
(d) Total pay over baseline total pay

Notes. This figure plots the regression coefficients as well as 95% confidence intervals from estimating equation (2) for the sample of incumbent workers, separately by workers' absolute talent level. Absolute talent is defined as a binary indicator equal to one for individuals in the top 10% of the residual earnings distribution in 2011, where residuals are obtained from equation (1). The dependent variables of interest are the variable pay over total pay in panel (a), fixed pay over total pay in panel (b), and total pay over baseline total pay (as measured in 2011) in panel (c). All regressions include the same fixed-effects and controls as in Figure 2. Standard errors are clustered at the job-spell level.

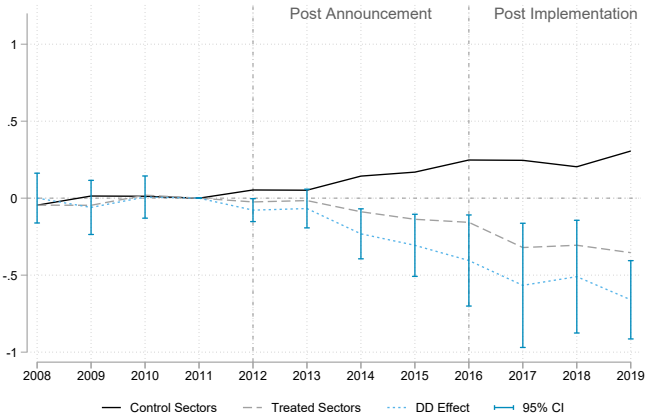
Figure 5: Worker flows at the firm level - full sample



(a) Cumulative hiring over baseline staff



(b) Cumulative separations over baseline staff



(c) Total staff over baseline staff

Notes. This figure plots the regression coefficients as well as 95% confidence intervals from estimating equation (4) for all the sample of workers. The dependent variables are the cumulative hiring since 2011 over baseline employment (measured in 2011) in panel (a), the cumulative separations since 2011 over baseline employment (measured in 2011) in panel (b), and the total employment over baseline employment (measured in 2011) in panel (c). All regressions include firm fixed effects and year fixed effects. Regressions are weighted by firm employment in 2011. Standard errors are clustered at the firm level.

Tables

Table 1: Descriptive statistics on individual characteristics

	Treated All incumbents	Controls All incumbents	Treated Abs. Talent YES	Treated Abs. Talent NO	Controls Abs. Talent YES	Controls Abs. Talent NO
	(1)	(2)	(3)	(4)	(5)	(6)
A: Pay composition						
Variable pay (th. €)	11.403	29.932	52.325	6.860	211.650	9.502
	(30.512)	(175.530)	(84.113)	(6.517)	(516.734)	(15.522)
Fixed pay (th. €)	84.541	92.182	172.850	74.736	223.203	77.451
	(42.444)	(76.659)	(76.238)	(19.857)	(185.434)	(23.207)
Total pay (th. €)	95.944	122.114	225.175	81.596	434.854	86.952
	(63.905)	(228.768)	(132.663)	(22.883)	(633.944)	(31.220)
Variable over fixed pay (%)	0.107	0.137	0.258	0.090	0.371	0.110
	(0.110)	(0.164)	(0.215)	(0.075)	(0.257)	(0.126)
Variable over fixed pay > 20%	0.096	0.192	0.468	0.054	0.652	0.140
	(0.294)	(0.394)	(0.499)	(0.226)	(0.477)	(0.347)
Variable over total pay (%)	0.089	0.106	0.185	0.079	0.245	0.090
	(0.073)	(0.102)	(0.122)	(0.056)	(0.137)	(0.084)
Variable pay in cash (th. €)	11.307	27.968	51.455	6.849	194.071	9.293
	(29.722)	(171.789)	(81.669)	(6.502)	(510.104)	(15.415)
Variable pay in stocks (th. €)	0.096	1.964	0.870	0.010	17.579	0.209
	(3.550)	(23.786)	(11.108)	(0.486)	(72.859)	(2.094)
B: Workforce characteristics						
Female	0.225	0.150	0.143	0.235	0.084	0.158
	(0.418)	(0.358)	(0.350)	(0.424)	(0.278)	(0.365)
Age (#)	45.002	46.287	47.417	44.733	47.715	46.127
	(8.558)	(9.254)	(7.646)	(8.611)	(7.948)	(9.378)
Missing education	0.418	0.513	0.470	0.413	0.546	0.510
	(0.493)	(0.500)	(0.499)	(0.492)	(0.499)	(0.500)
Non-tertiary education	0.112	0.125	0.091	0.115	0.084	0.129
	(0.316)	(0.330)	(0.288)	(0.319)	(0.278)	(0.336)
Tertiary education	0.469	0.362	0.439	0.473	0.370	0.361
	(0.499)	(0.481)	(0.496)	(0.499)	(0.484)	(0.480)
Native	0.935	0.915	0.917	0.937	0.907	0.916
	(0.246)	(0.278)	(0.276)	(0.243)	(0.290)	(0.277)
N of job spells in 2011	31511	2246	3149	28362	227	2019

Notes: This table reports descriptive statistics for the incumbent sample, measured in 2011 (the year before the policy announcement). Incumbent workers are defined as individuals employed in the financial sector up to and including 2011, aged 18 or older, working full-time with a permanent contract, and earning at least 50,000 in fixed pay in 2011 (expressed in 2015 euros). The sample is further restricted to firms with at least one employee whose variable-to-fixed pay ratio exceeded 20% during the pre-policy period (2008–2011) and to firms with at least ten employees in every pre-policy year. Treated sectors refer to banks. Control sectors refer to financial institutions not subject to the 20% bonus cap. "Abs. Talent YES" identifies workers in the top 10% of the residual earnings distribution in 2011, where residuals are obtained from equation (1). Panel A reports pay composition variables. All pay amounts are in thousands of euros, deflated to 2015 euros. Variable-to-fixed pay ratios are expressed as percentages. Panel B reports workforce characteristics. Standard deviations are reported in parentheses.

Table 2: Descriptive statistics on firm characteristics

	Treated - All incumbents	Controls - All incumbents
Cooperative banks	0.506	0.000
	(0.500)	(0.000)
Stock credit companies	0.004	0.000
	(0.064)	(0.000)
Savings banks	0.001	0.000
	(0.037)	(0.000)
General banks	0.489	0.000
	(0.500)	(0.000)
Financial holdings	0.000	0.007
	(0.000)	(0.084)
Investment funds in financial assets	0.000	0.107
	(0.000)	(0.309)
Investment funds in real estate	0.000	0.328
	(0.000)	(0.470)
Investment funds with restricted entry	0.000	0.030
	(0.000)	(0.171)
Participation companies	0.000	0.122
	(0.000)	(0.327)
Market makers	0.000	0.011
	(0.000)	(0.105)
Risk and damage evaluation	0.000	0.293
	(0.000)	(0.455)
Consultancy; management of pension funds	0.000	0.081
	(0.000)	(0.274)
Guarantee funding	0.000	0.016
	(0.000)	(0.124)
Other services, insurance and pension funding	0.000	0.004
	(0.000)	(0.067)
Firm size (#)	11628.856	260.598
	(5408.731)	(298.837)
10-49 employees	0.013	0.366
	(0.112)	(0.482)
50-99 employees	0.008	0.187
	(0.090)	(0.390)
100+ employees	0.979	0.447
	(0.143)	(0.497)
Average Firm size	984.719	41.593
	(3289.315)	(96.337)
Median Firm Size	50.500	17.500
	(0.000)	(0.000)
N of firms in 2011	32	54
N of job spells in 2011	31511	2246

Notes: This table reports descriptive statistics on firm characteristics for the incumbent sample in 2011. The sample includes firms in the financial sector with at least one employee whose variable-to-fixed pay ratio exceeded 20% during the pre-policy period (2008–2011) and with at least ten employees in every pre-policy year. Treated firms are banks. Control firms are financial institutions not subject to the 20% bonus cap under the Wbfo. Standard deviations are reported in parentheses.

Table 3: Variable pay variability

	Within job spells					Across workers within job	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
SD variable pay	33,367	33,367	33,364	35,228	26,031	40,781	44,386
SD fixed pay	10,490	10,436	10,391	12,421	6,431	26,592	27,334
Number of observations	297,109	297,109	297,109	1,087,842	297,109	169,650	141,266
Job Spell FE	Yes	Yes	Yes	Yes	Yes		
Age FE		Yes	Yes	Yes	Yes		
Year FE			Yes			Yes	Yes
Age × Job Spell FE					Yes		
Firm FE interacted						Yes	Yes
Sample: 2008–2011	X	X	X		X	X	X
Sample: 2008–2019				X			
Sample: Variable pay >0							X

Notes: This table reports the residual standard deviation of variable pay and fixed pay within job spells in the financial sector. For each specification, we residualize pay using the set of controls and fixed effects indicated in the table and then compute the standard deviation of the resulting residuals; the number of observations is reported below each column. Columns (1)–(3) use the pre-policy period 2008-2011 and sequentially add controls for life-cycle and business-cycle variation (spell fixed effects only; then age-bin controls; then year fixed effects). Column (4) extends the sample to 2008-2019 (including the post-cap period 2012-2019). Column (5) returns to 2008-2011 and additionally allows for differential age trends across job spells (age × spell fixed effects).

Table 4: Returns to observables within variable pay jobs in the finance sector

			All jobs		
	VP jobs	Non-VP jobs	Baseline	Firm FE	Worker FE
	(1)	(2)	(3)	(4)	(5)
Education	.094***	.061***	.059***	.063***	
	(.001)	(.001)	(.001)	(.001)	
Experience	1.026***	.899***	.89***	.869***	-.121
	(.018)	(.016)	(.016)	(.015)	(.88)
Tenure	.128***	.138***	.156***	.1***	.04***
	(.007)	(.006)	(.006)	(.006)	(.008)
Variable-pay dummy			-.509***	-.329***	-.48***
			(.039)	(.039)	(.128)
Education × performance pay job			.037***	.021***	.015**
			(.002)	(.002)	(.007)
Experience × performance pay job			.14***	.145***	.333***
			(.024)	(.022)	(.039)
Tenure × performance pay job			-.038***	.021**	.037***
			(.009)	(.008)	(.009)
Number of observations	131,826	107,990	239,820	232,979	215,540

Notes: This table implements the test in Lemieux et al. (2009) for jobs in the finance sector. "VP jobs" are job spells with at least €1,000 in variable pay; "Non-VP jobs" are job spells with variable pay below €1,000. Columns (1) and (2) report separate Mincerian regressions estimated on these two groups. Columns (3)–(5) report pooled specifications on all jobs that include a VP-job indicator and its interactions with education, experience, and tenure. The table reports the implied marginal effects of experience and tenure evaluated at the sample means. The dependent variable is log hourly wages. Controls include years of education, a cubic polynomial in experience, a squared polynomial in tenure, a married dummy, and an indicator for being born in the Netherlands, as well as year fixed effects, sector fixed effects, and year-by-municipality fixed effects. Standard errors (in parentheses) are clustered at the job-spell level. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Within-bank specification

	$\text{Var}_t/\text{Fix}_t > 20\%$	$\text{Var}_t/\text{Tot}_{2011}$	$\text{Fix}_t/\text{Tot}_{2011}$	$\text{Tot}_t/\text{Tot}_{2011}$
	(1)	(2)	(3)	(4)
Panel A: All incumbents				
Post Ann. (2012-15)	0.000	-0.004***	0.045***	0.041***
	(.)	(0.000)	(0.001)	(0.001)
Post Impl. (2016-19)	0.000	-0.008***	0.115***	0.107***
	(.)	(0.001)	(0.002)	(0.002)
T × Post Ann. (2012-15)	0.082	0.009***	0.018***	0.026***
	(.)	(0.001)	(0.001)	(0.002)
T × Post Impl. (2016-19)	-0.044	-0.026***	0.036***	0.010***
	(.)	(0.001)	(0.003)	(0.004)
N	260222	260229	260229	260229
N job spells	31628	31628	31628	31628
Adj R2	0.264	0.233	0.471	0.300
Mean C Group 2011	0.000	0.061	0.939	1.000
Mean T Group 2011	0.170	0.113	0.887	1.000
Job spell F.E.	Yes	Yes	Yes	Yes
Panel B: Abs. Talent YES				
Post Ann. (2012-15)	0.000	-0.020***	0.042***	0.022**
	(.)	(0.003)	(0.009)	(0.010)
Post Impl. (2016-19)	-0.000	-0.028***	0.132***	0.104***
	(.)	(0.005)	(0.017)	(0.019)
T × Post Ann. (2012-15)	-0.050	-0.011**	0.052***	0.041***
	(.)	(0.006)	(0.010)	(0.012)
T × Post Impl. (2016-19)	-0.259	-0.057***	0.118***	0.061***
	(.)	(0.008)	(0.019)	(0.022)
N	14388	14388	14388	14388
N job spells	1832	1832	1832	1832
Adj R2	0.382	0.319	0.629	0.319
Mean C Group 2011	0.000	0.075	0.925	1.000
Mean T Group 2011	0.560	0.220	0.780	1.000
Job spell F.E.	Yes	Yes	Yes	Yes
Panel C: Abs. Talent NO				
Post Ann. (2012-15)	0.000	-0.003***	0.071***	0.068***
	(.)	(0.000)	(0.001)	(0.001)
Post Impl. (2016-19)	-0.000	-0.010***	0.167***	0.157***
	(.)	(0.001)	(0.003)	(0.003)
T × Post Ann. (2012-15)	0.109	0.014***	0.014***	0.027***

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	$Var_t/Fix_t > 20\%$	Var_t/Tot_{2011}	Fix_t/Tot_{2011}	Tot_t/Tot_{2011}
	(1)	(2)	(3)	(4)
	(.)	(0.001)	(0.002)	(0.002)
T × Post Impl. (2016-19)	-0.001	-0.013***	0.025***	0.012**
	(.)	(0.002)	(0.004)	(0.005)
N	139238	139241	139241	139241
N job spells	16559	16559	16559	16559
Adj R2	0.147	0.158	0.477	0.402
Mean C Group 2011	0.000	0.063	0.937	1.000
Mean T Group 2011	0.115	0.099	0.901	1.000
Job spell F.E.	Yes	Yes	Yes	Yes
Panel D: Rel. Talent YES				
Post Ann. (2012-15)	0.000	-0.018***	0.042***	0.024***
	(.)	(0.002)	(0.009)	(0.009)
Post Impl. (2016-19)	0.000	-0.026***	0.116***	0.090***
	(.)	(0.005)	(0.017)	(0.018)
T × Post Ann. (2012-15)	-0.039	-0.014***	0.054***	0.040***
	(.)	(0.005)	(0.009)	(0.011)
T × Post Impl. (2016-19)	-0.279	-0.071***	0.146***	0.075***
	(.)	(0.008)	(0.020)	(0.022)
N	14494	14494	14494	14494
N job spells	1846	1846	1846	1846
Adj R2	0.403	0.337	0.644	0.320
Mean C Group 2011	0.000	0.065	0.935	1.000
Mean T Group 2011	0.554	0.222	0.778	1.000
Job spell F.E.	Yes	Yes	Yes	Yes
Panel E: Rel. Talent NO				
Post Ann. (2012-15)	-0.000	-0.003***	0.071***	0.068***
	(.)	(0.000)	(0.001)	(0.001)
Post Impl. (2016-19)	-0.000	-0.010***	0.167***	0.157***
	(.)	(0.001)	(0.003)	(0.003)
T × Post Ann. (2012-15)	0.107	0.014***	0.013***	0.027***
	(.)	(0.001)	(0.002)	(0.002)
T × Post Impl. (2016-19)	0.001	-0.011***	0.023***	0.012**
	(.)	(0.002)	(0.004)	(0.005)
N	139132	139135	139135	139135
N job spells	16545	16545	16545	16545
Adj R2	0.157	0.167	0.477	0.402
Mean C Group 2011	0.000	0.063	0.937	1.000
Mean T Group 2011	0.117	0.099	0.901	1.000

Continued on next page

	$\text{Var}_t/\text{Fix}_t > 20\%$	$\text{Var}_t/\text{Tot}_{2011}$	$\text{Fix}_t/\text{Tot}_{2011}$	$\text{Tot}_t/\text{Tot}_{2011}$
	(1)	(2)	(3)	(4)
Job spell F.E.	Yes	Yes	Yes	Yes

Notes: This table reports results from a within-bank specification that compares treatment and control groups. "Post Ann. (2012-15)" refers to the post-announcement period; "Post Impl. (2016-19)" refers to the post-implementation period. "T × Post Ann." and "T × Post Impl." capture the differential effects for the treated group (those previously exposed to the cap). Panel A reports results for all incumbent bank employees. Panels B-D report results by baseline total pay categories (in thousands of euros). Panels E-F report results by talent level, where talent is defined using the within-firm relative measure. All regressions include job-spell fixed effects. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Incumbents Separations

	All Separations	Voluntary	Voluntary excl. early retirement	Involuntary
	(1)	(2)	(3)	(4)
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: 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
C: 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

Notes. This table reports the effect of the bonus cap on separation rates for incumbent workers. Standard errors are clustered at the job-spell level and reported in parentheses. All specifications include job-spell fixed-effects. Significance levels: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 7: Incumbents Separations Details

	(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: 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
C: 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

Notes. This table reports the effect of the bonus cap on different separation destinations for incumbent workers. Standard errors are clustered at the job-spell level and reported in parentheses. All specifications include job-spell fixed-effects. Significance levels: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 8: Hirings

	(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
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

Notes. This table reports difference-in-differences estimates of how the composition of new hires changes in treated firms (banks) relative to control financial institutions not subject to the 20% Wbfo bonus cap. The analysis uses the full period 2008–2019 and retains one observation per worker, corresponding to the year of hire, so the design is a DiD on repeated cross-sections of hires. Outcomes are indicator variables for the origin of the hire, as labeled in columns (e.g., previously unemployed, outside finance, within-finance origin, self-employment, abroad). Standard errors are clustered at the job-spell level and reported in parentheses. Significance levels: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

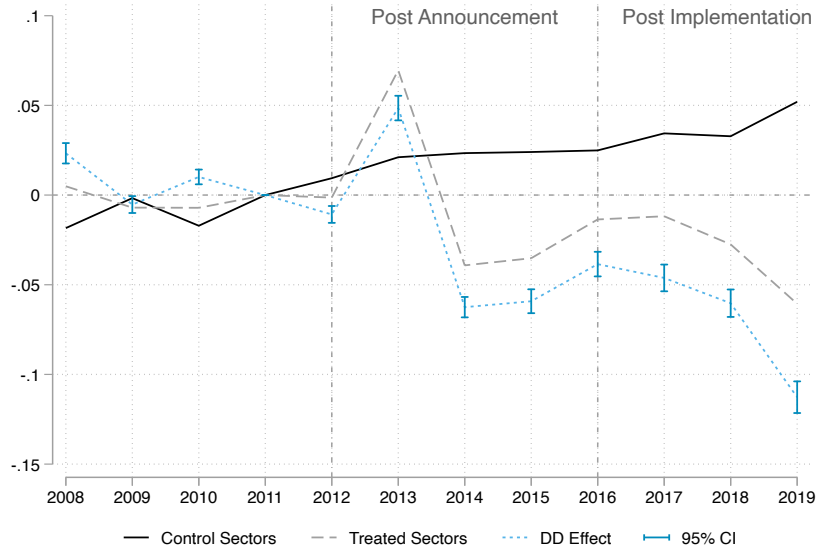
Table 9: Hirings by Previous Job Income

	(1) Prev. Job 50<Tot≤100	(2) Prev. Job 100<Tot≤175	(3) Prev. Job Tot>175	(4) Prev. Job missing
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

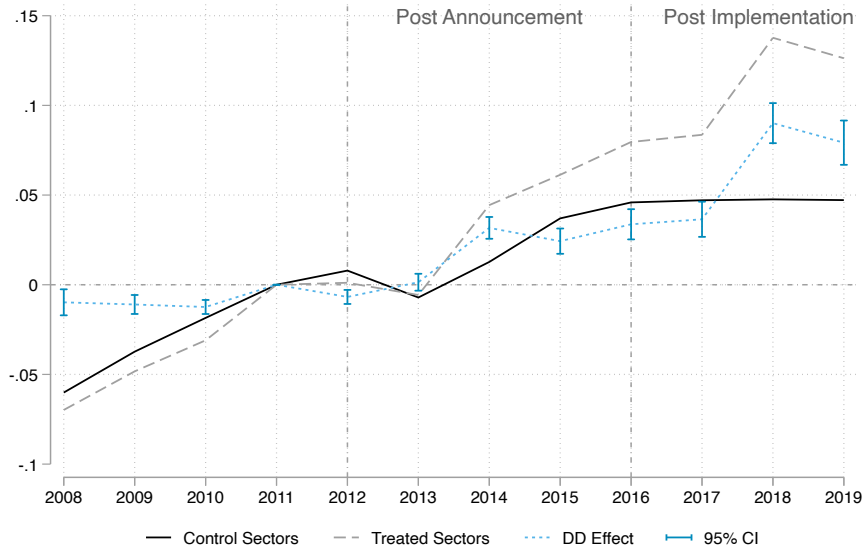
Notes. This table reports difference-in-differences estimates of how the distribution of new hires by previous-job total income changes in treated firms (banks) relative to control financial institutions not subject to the 20% Wbfo bonus cap. The dependent variables are indicators for whether the hire's previous-job total income falls in the bins shown in the column headers (e.g., €100,000–€175,000 and above €175,000), or is missing. Standard errors are clustered at the job-spell level and reported in parentheses. All specifications include job-spell fixed effects. Significance levels: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Appendix

Figure A1: Variable and Fixed pay over Baseline Total Pay



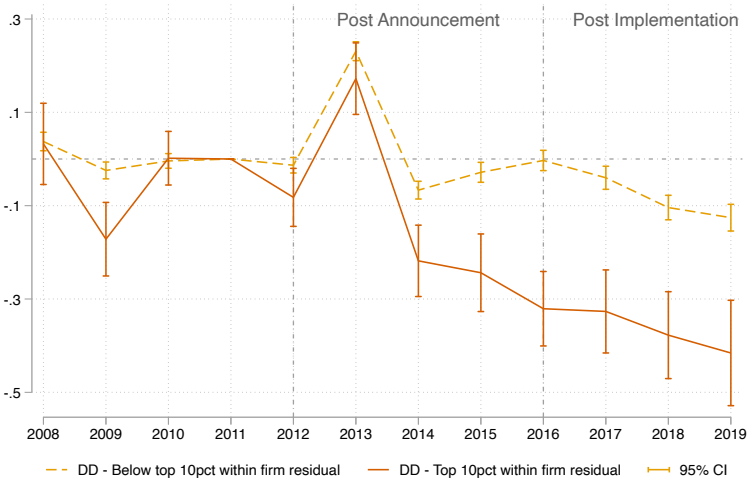
(a) Variable Pay over Baseline Total Pay



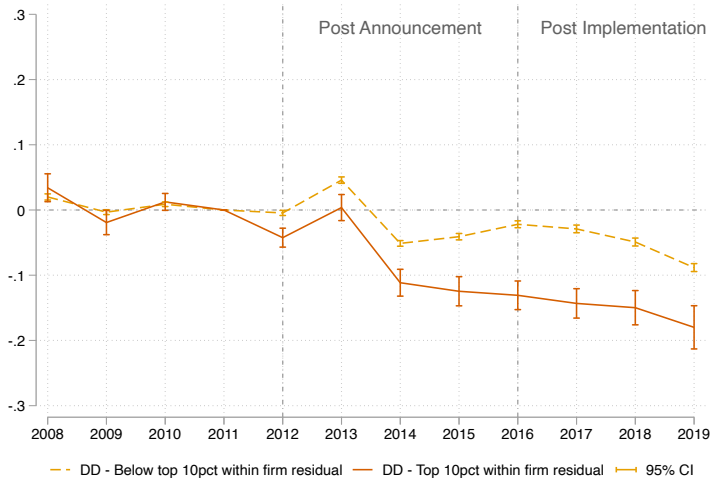
(b) Fixed Pay over Baseline Total Pay

Notes. This figure plots the regression coefficients as well as 95% confidence intervals from estimating equations (2) and (3) for the sample of incumbent workers. Incumbent workers are defined as individuals employed in the financial sector up to and including 2011. The dependent variables are variable pay over baseline total pay (as measured in 2011) in panel (a), and fixed pay over baseline total pay (as measured in 2011) in panel (b). The black line represents the evolution of outcomes for control-sector employees relative to 2011 (equation (3)). The dashed line represents the evolution of outcomes for treated-sector (bank) employees relative to 2011 (equation (3)). The blue dashed line represents the difference-in-differences estimates comparing treated and control sectors in each year relative to 2011 (equation (2)). All regressions include job-spell fixed effects and controls for age (eight five-year bins), marital status (married or in a de facto partnership), parenthood, and firm size (1–19; 20–199; 200–499; 500 or more employees). Standard errors are clustered at the job-spell level. The vertical dashed lines indicate the policy announcement period (October 2012) and implementation period (February 2015 for new hires, 2016 for existing employees).

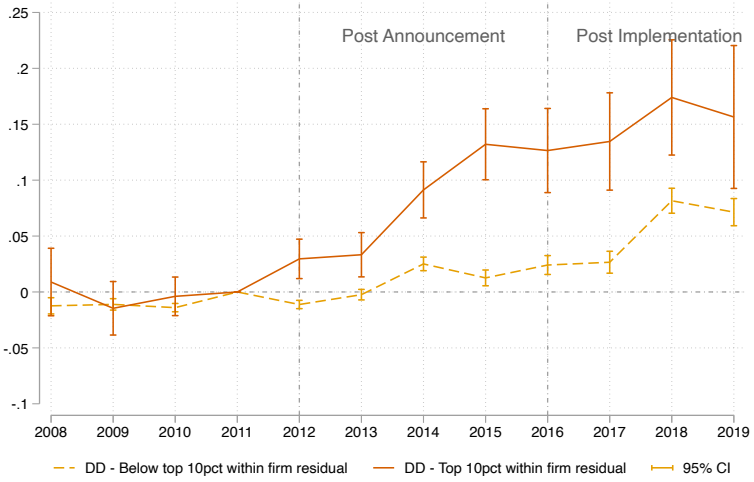
Figure A2: Effects on Pay Structure by Workers' Absolute Talent



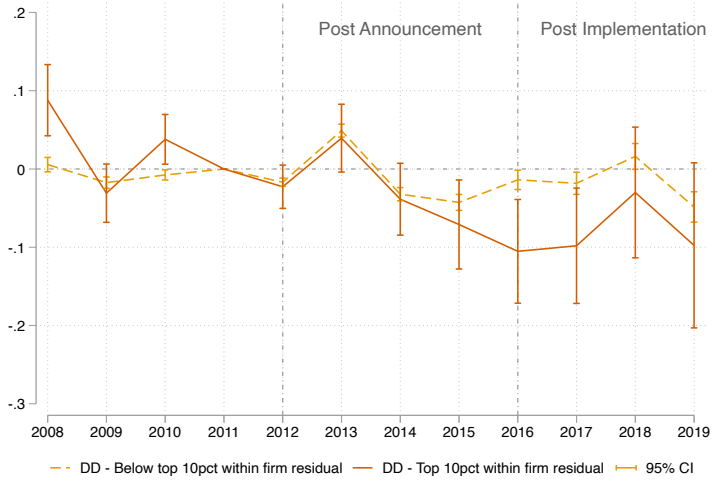
(a) Variable to fix pay ratio above 20%



(b) Variable pay over total pay



(c) Fixed pay over baseline total pay



(d) Total pay over baseline total pay

Notes. This figure plots the regression coefficients and 95% confidence intervals from estimating eq (2) for the sample of incumbent workers, separately by workers' relative talent level. Relative talent is defined as a binary indicator equal to one for individuals in the top 10% of the residual earnings distribution within their firm in 2011 (residuals obtained from eq (1)). The dependent variables are an indicator for whether the variable-to-fixed pay ratio exceeds 20% in panel (a), variable pay over total pay in panel (b), fixed pay over baseline total pay in panel (c), and total pay over baseline total pay in panel (d). All regressions include the same fixed effects and controls as in Figure 2. Standard errors are clustered at the job-spell level.

Table A1: Descriptive statistics, relative measure of talent

	Treated Rel. Talent YES	Treated Rel. Talent NO	Controls Rel. Talent YES	Controls Rel. Talent NO
A: Pay composition				
Variable pay (th. €)	51.156	6.963	153.193	14.771
	(83.807)	(7.379)	(485.595)	(60.020)
Fixed pay (th. €)	170.136	74.980	191.823	79.926
	(76.894)	(20.801)	(176.091)	(37.780)
Total pay (th. €)	221.292	81.943	345.016	94.697
	(133.567)	(24.431)	(603.151)	(85.568)
Variable over fixed pay (%)	0.254	0.090	0.289	0.118
	(0.213)	(0.076)	(0.239)	(0.142)
Variable over fixed pay > 20%	0.455	0.055	0.533	0.150
	(0.498)	(0.229)	(0.500)	(0.357)
Variable over total pay (%)	0.182	0.079	0.200	0.094
	(0.121)	(0.056)	(0.135)	(0.091)
Variable pay in cash (th. €)	50.372	6.944	136.801	14.582
	(81.383)	(7.279)	(477.160)	(60.011)
Variable pay in stocks (th. €)	0.785	0.019	16.392	0.190
	(10.936)	(0.774)	(70.107)	(2.057)
B: Workforce characteristics				
Female	0.147	0.234	0.110	0.155
	(0.354)	(0.424)	(0.313)	(0.362)
Age (#)	47.432	44.730	48.191	46.053
	(7.606)	(8.615)	(8.243)	(9.346)
Missing education	0.470	0.412	0.565	0.507
	(0.499)	(0.492)	(0.497)	(0.500)
Non-tertiary education	0.097	0.114	0.106	0.127
	(0.296)	(0.318)	(0.308)	(0.333)
Tertiary education	0.434	0.473	0.329	0.366
	(0.496)	(0.499)	(0.471)	(0.482)
Native	0.922	0.937	0.927	0.914
	(0.268)	(0.244)	(0.261)	(0.280)
N of job spells in 2011	3166	28345	246	2000

Notes: This table reports descriptive statistics for the incumbent sample, measured in 2011 (the year before the policy announcement). Incumbent workers are defined as individuals employed in the financial sector up to and including 2011, aged 18 or older, working full-time with a permanent contract, and earning at least €50,000 in fixed pay in 2011 (expressed in 2015 euros). The sample is further restricted to firms with at least one employee whose variable-to-fixed pay ratio exceeded 20% during the pre-policy period (2008–2011) and to firms with at least ten employees in every pre-policy year. Treated sectors refer to banks. Control sectors refer to financial institutions not subject to the 20% bonus cap. "Rel. Talent YES" identifies workers in the top 10% of the within-firm residual earnings distribution in 2011, where residuals are obtained from equation (1). Panel A reports pay composition variables. All pay amounts are in thousands of euros, deflated to 2015 euros. Variable-to-fixed pay ratios are expressed as percentages. Panel B reports workforce characteristics. Standard deviations are reported in parentheses.

Table A2: All treated and control sectors

Groups	Sectors affected by the Wbfo	Sectors not affected by the Wbfo
Banks (N=31,511)	Cooperative banks (64191); Stock credit companies (64192); Saving banks (64193); General banks (64194)	
Insurances (N=11,967)	Life insurance (65111); Insurance in-kind (65112); Non-life insurance (65120); Reinsurance (65200)	
Other treated sectors (N=2,477)	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=2,361)		Industry funds (65301); Corporate funds (65302); Pension funding for professional groups (65303); Other pension funding (65309)
Other control sectors (N=2,246)		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). For more details, see also footnote 32 of the policy report by Weitzel et al. (2018) (in Dutch). The number of individual employees (N) are provided by sector for the year 2011.

Table A3: Variable pay variability: Full Economy

	Within job spells					Across workers within j	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
SD variable pay	36,370	36,370	36,370	64,934	18,142	46,538	53,793
SD fixed pay	6,366	6,344	6,324	7,887	3,918	20,597	22,008
Number of observations	7,752,202	7,752,202	7,752,202	26,121,811	7,752,202	336,913	249,664
Job Spell FE	Yes	Yes	Yes	Yes	Yes		
Age FE		Yes	Yes	Yes	Yes		
Year FE			Yes			Yes	Yes
Age × Job Spell FE					Yes		
Firm FE interacted						Yes	Yes
Sample: 2008–2011	X	X	X		X	X	X
Sample: 2008–2019				X			
Sample: Variable pay >0							X

Notes: This table reports the residual standard deviation of variable pay and fixed pay within job spells. For each specification, we residualize pay using the set of controls and fixed effects indicated in the table and then compute the standard deviation of the resulting residuals; the number of observations is reported below each column. Columns (1)–(3) use the pre-policy period 2008-2011 and sequentially add controls for life-cycle and business-cycle variation (spell fixed effects only; then age-bin controls; then year fixed effects). Column (4) extends the sample to 2008-2019 (including the post-cap period 2012-2019). Column (5) returns to 2008-2011 and additionally allows for differential age trends across job spells (age × spell fixed effects).

Table A4: Returns to observables within variable pay jobs

			All jobs		
	VP jobs	Non-VP jobs	Baseline	Firm FE	Worker FE
	(1)	(2)	(3)	(4)	(5)
Education	.08***	.048***	.05***	.035***	
	(0)	(0)	(0)	(0)	
Experience	.956***	.629***	.636***	.554***	-.212***
	(.003)	(.002)	(.002)	(.002)	(.069)
Tenure	.053***	.071***	.077***	.083***	.013***
	(.001)	(.001)	(.001)	(.001)	(.001)
Variable-pay dummy			-.472***	-.497***	-.122***
			(.006)	(.006)	(.008)
Education × performance pay job			.03***	.026***	.008***
			(0)	(0)	(0)
Experience × performance pay job			.316***	.319***	.092***
			(.004)	(.004)	(.005)
Tenure × performance pay job			-.031***	-.005***	.03***
			(.002)	(.001)	(.001)
Number of observations	2601947	2820338	5422285	5379294	5027978

Notes: This table implements the test in Lemieux et al. (2009). "VP jobs" are job spells with at least €1,000 in variable pay; "Non-VP jobs" are job spells with variable pay below €1,000. Columns (1) and (2) report separate Mincerian regressions estimated on these two groups. Columns (3)–(5) report pooled specifications on all jobs that include a VP-job indicator and its interactions with education, experience, and tenure. The table reports the implied marginal effects of experience and tenure evaluated at the sample means. The dependent variable is log hourly wages. Controls include years of education, a cubic polynomial in experience, a squared polynomial in tenure, a married dummy, and an indicator for being born in the Netherlands, as well as year fixed effects, sector fixed effects, and year-by-municipality fixed effects. Standard errors (in parentheses) are clustered at the job-spell level. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Main industries of joiners and leavers in the period 2008–2011

	Treated		Control	
Industry (4-digit code)	Join from (1)	Leave to (2)	Join from (3)	Leave to (4)
Panel A: Treated Sectors				
Banks (6419)	58.58%	40.55%	4.76%	12.47%
Non-life insurance (6512)	9.00%	8.40%	6.32%	9.10%
Insurance agents (6622)	0.23%	1.26%	3.87%	3.09%
Stockbrokers, investment consultants etc. (6612)	1.33%	1.31%	0.95%	1.73%
Life insurance (6511)	1.00%	2.24%	3.87%	6.64%
Reinsurance (6520)	1.04%		5.30%	
Other credit granting (6492)	8.72%	2.01%	1.36%	
Panel B: Control Sectors				
Risk and damage evaluation (6621)			10.05%	3.28%
Pension funding (6530)	0.14%	0.82%		1.91%
Investment funds (6430)	0.25%		5.30%	4.37%
Trust offices, market makers, mortgage, brokers, bank and savings bank (6619)	1.54%	0.98%	2.58%	2.91%
Other services of insurance and pension funding (6629)		0.23%	10.19%	
Central banking (6411)	0.12%	1.29%		
Panel C: Other Sectors				
Holding companies (7010)	1.05%	2.07%	5.37%	7.83%
Management en business consultancy (no public relations) (7022)	1.28%	4.80%	4.01%	5.55%
Temporary employment (7820)	2.12%	5.40%	2.04%	4.46%
Warehousing and storage (5210)			2.65%	
Accounting, tax consultancy, administration (6920)	1.27%	2.10%	2.45%	1.46%
Writing, producing and publishing of software (6201)	2.33%	1.76%	1.36%	
Wholesale of hardware and heating equipment (4674)			0.75%	2.00%
Intermediation in buying, selling and renting real estate (6831)	0.35%	0.70%	1.83%	
Legal activities (6910)	0.49%	0.41%	1.02%	1.36%
Research and development on social sciences and humanities (7220)			1.29%	
Other specialized business services (7490)	0.11%	0.34%	1.22%	
General public administration (8411)	0.39%	1.20%	1.09%	
Other mining and quarrying n.e.c. (0899)				1.18%
Management of real estate (6832)	0.13%		1.15%	
Renting of real estate (6820)		0.61%	1.09%	
Computer consultancy activities (6202)	1.01%			
Number of individuals	11,139	7,729	1,472	1,099

Notes: This table is based on a sample with similar sample selection as our main analysis, i.e. individuals aged 18 or older who earn at least €50,000 in fixed pay, work full time and hold a permanent contract in the new job (for 'Join from' columns) or past job (for 'Leave to' columns). In addition, we include individuals for which we observe a previous job in the past 365 days ('Join from') or observe a next job in the next 365 days ('Leave to'). We show 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. Note that the subindustry 'Other credit granting' (6492) consists of a treated subindustry (64924) and a control subindustry (64923).

Table A6: Pooled

	$\text{Var}_t/\text{Fix}_t > 20\%$	$\text{Var}_t/\text{Tot}_{2011}$	$\text{Fix}_t/\text{Tot}_{2011}$	$\text{Tot}_t/\text{Tot}_{2011}$
	(1)	(2)	(3)	(4)
Panel A: All incumbents				
$T\times$ Post Ann. (2012–15)	0.0249***	-0.0230***	0.0207***	-0.0048
	(0.0070)	(0.0023)	(0.0026)	(0.0037)
$T\times$ Post Impl. (2016–19)	-0.0798***	-0.0624***	0.0629***	-0.0164**
	(0.0095)	(0.0032)	(0.0050)	(0.0075)
Post Ann. (2012–15)	0.0220***	0.0264***	0.0341***	0.0627***
	(0.0068)	(0.0023)	(0.0025)	(0.0037)
Post Impl. (2016–19)	0.0581***	0.0421***	0.0693***	0.1283***
	(0.0093)	(0.0031)	(0.0048)	(0.0073)
Constant	0.0955***	0.0879***	0.8772***	0.9661***
	(0.0008)	(0.0002)	(0.0005)	(0.0006)
Observations	275,876	275,884	275,884	275,884
Adj. R^2	0.3758	0.3893	0.5584	0.4289
Mean C Group 2011	0.192	0.106	0.755	1
Mean T Group 2011	0.096	0.089	0.881	1
Job spell F.E.	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes
Panel B: Abs. Talent YES				
$T\times$ Post Ann. (2012–15)	-0.1013***	-0.0923***	0.0820***	-0.0460**
	(0.0300)	(0.0121)	(0.0117)	(0.0203)
$T\times$ Post Impl. (2016–19)	-0.2975***	-0.1634***	0.1482***	-0.1285***
	(0.0324)	(0.0140)	(0.0248)	(0.0405)
Post Ann. (2012–15)	0.0468	0.0794***	-0.0116	0.0971***
	(0.0291)	(0.0119)	(0.0115)	(0.0200)
Post Impl. (2016–19)	0.0556*	0.0962***	0.0506**	0.2535***
	(0.0306)	(0.0137)	(0.0241)	(0.0399)
Constant	0.4604***	0.1831***	0.7742***	0.9659***
	(0.0042)	(0.0012)	(0.0019)	(0.0026)
Observations	25,856	25,857	25,857	25,857
Adj. R^2	0.4641	0.4804	0.6842	0.4408
Mean C Group 2011	0.652	0.245	0.513	1
Mean T Group 2011	0.468	0.185	0.768	1
Job spell F.E.	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes
Panel C: Abs. Talent NO				
$T\times$ Post Ann. (2012–15)	0.0375***	-0.0161***	0.0146***	-0.0007

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	$\text{Var}_t/\text{Fix}_t > 20\%$	$\text{Var}_t/\text{Tot}_{2011}$	$\text{Fix}_t/\text{Tot}_{2011}$	$\text{Tot}_t/\text{Tot}_{2011}$
	(1)	(2)	(3)	(4)
	(0.0070)	(0.0021)	(0.0025)	(0.0036)
$T\times$ Post Impl. (2016–19)	-0.0588***	-0.0527***	0.0546***	-0.0061
	(0.0099)	(0.0032)	(0.0050)	(0.0071)
Post Ann. (2012–15)	0.0194***	0.0211***	0.0387***	0.0594***
	(0.0069)	(0.0021)	(0.0024)	(0.0035)
Post Impl. (2016–19)	0.0582***	0.0369***	0.0712***	0.1167***
	(0.0098)	(0.0031)	(0.0047)	(0.0069)
Constant	0.0580***	0.0781***	0.8878***	0.9661***
	(0.0007)	(0.0002)	(0.0005)	(0.0006)
Observations	250,020	250,027	250,027	250,027
Adj. R^2	0.2727	0.2891	0.5053	0.4270
Mean C Group 2011	0.14	0.09	0.891	1
Mean T Group 2011	0.054	0.079	0.916	1
Job spell F.E.	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes
Panel D: Rel. Talent YES				
$T\times$ Post Ann. (2012–15)	-0.0531*	-0.0751***	0.0700***	-0.0366**
	(0.0277)	(0.0112)	(0.0102)	(0.0184)
$T\times$ Post Impl. (2016–19)	-0.3142***	-0.1654***	0.1434***	-0.1015***
	(0.0331)	(0.0139)	(0.0224)	(0.0368)
Post Ann. (2012–15)	0.0045	0.0606***	0.0042	0.0901***
	(0.0268)	(0.0110)	(0.0099)	(0.0181)
Post Impl. (2016–19)	0.0618**	0.0907***	0.0619***	0.2257***
	(0.0315)	(0.0136)	(0.0216)	(0.0361)
Constant	0.4323***	0.1765***	0.7810***	0.9654***
	(0.0040)	(0.0012)	(0.0018)	(0.0025)
Observations	26,082	26,083	26,083	26,083
Adj. R^2	0.4778	0.4924	0.6887	0.4417
Mean C Group 2011	0.460	0.183	0.817	1
Mean T Group 2011	0.460	0.183	0.817	1
Job spell F.E.	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes
Panel E: Rel. Talent NO				
$T\times$ Post Ann. (2012–15)	0.0325***	-0.0174***	0.0154***	-0.0012
	(0.0071)	(0.0022)	(0.0026)	(0.0036)
$T\times$ Post Impl. (2016–19)	-0.0577***	-0.0521***	0.0551***	-0.0075
	(0.0099)	(0.0032)	(0.0050)	(0.0072)
Post Ann. (2012–15)	0.0239***	0.0225***	0.0375***	0.0596***

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	$\text{Var}_t/\text{Fix}_t > 20\%$	$\text{Var}_t/\text{Tot}_{2011}$	$\text{Fix}_t/\text{Tot}_{2011}$	$\text{Tot}_t/\text{Tot}_{2011}$
	(1)	(2)	(3)	(4)
	(0.0070)	(0.0021)	(0.0025)	(0.0035)
Post Impl. (2016–19)	0.0578***	0.0369***	0.0703***	0.1183***
	(0.0098)	(0.0031)	(0.0048)	(0.0070)
Constant	0.0605***	0.0787***	0.8871***	0.9661***
	(0.0007)	(0.0002)	(0.0005)	(0.0006)
Observations	249,794	249,801	249,801	249,801
Adj. R^2	0.2858	0.3042	0.5107	0.4267
Mean C Group 2011	0.0617	0.0800	0.920	1
Mean T Group 2011	0.0617	0.0800	0.920	1
Job spell F.E.	Yes	Yes	Yes	Yes
Year F.E.	Yes	Yes	Yes	Yes

Notes: This table reports pooled difference-in-differences estimates of the effect of the Dutch 20% bonus cap on incumbents' pay composition and total pay. The dependent variables are an indicator for whether the variable-to-fixed pay ratio exceeds 20% (column (1)), variable pay over baseline total pay (measured in 2011; column (2)), fixed pay over baseline total pay (measured in 2011; column (3)), and total pay over baseline total pay (measured in 2011; column (4)). The coefficients reported are for $T \times$ Post Ann. (2012–15) and $T \times$ Post Impl. (2016–19), where T indicates treated-sector (bank) employment. Panel A reports results for all incumbents; subsequent panels report results by talent group (as indicated in the panel headers). All specifications include job-spell fixed effects (and the same controls as in the main specifications), and standard errors (in parentheses) are clustered at the job-spell level. Significance levels: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.