Executive Compensation's Impact on Turnover and Bank Risk-Taking:

Evidence from the UK CRD IV Bonus Cap Implementation and Abolition

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Abstract

We study the relationship between executive compensation, executive turnover and risk-taking in the banking sector. Using evidence from both the EU implementation and post-Brexit abolishment of the Capital Requirements Directive (CRD) IV bonus cap, which mandated a maximum variable-to-fixed compensation ratio at 100% (or 200% with shareholder approval) for material risk-takers, we utilise the UK environment to conduct a full policy analysis. Motivated by the unique UK environment and the systemic importance of banks, we utilize a difference-in-differences (DiD) research design where we find that the implementation and abolishment of the bonus cap is not associated with any significant changes in credit risk or systemic risk. The implementation of the bonus cap, although not associated with turnover, carries a negatively significant association with shareholder risk and performance. Further, we find that the abolishment of the bonus cap carries a negatively significant association with turnover and a positively significant association with shareholder risk and performance. We contribute to extant literature by examining not just the implementation, but also the abolishment of the bonus cap, an empirical setting that has not yet been captured.

Key words: Banks, Bonus Cap, Executive Turnover, Executive Compensation, Risk-Taking

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1. Introduction

In 2008, the world faced one of the biggest financial crises, exposing numerous weaknesses in the financial sector's corporate governance, risk management and compensation structures. The crisis however, impacted not only the financial system, but extended across all industries and countries as state interventions and bailouts became necessary in dealing with the aftermath of the financial turmoil. In response, regulatory bodies across different jurisdictions began investigations into the main causes behind the crisis, and invested efforts into reestablishing a more solidified banking system. Regulations associated with financial institution's capital adequacy, liquidity, and compensation schemes, were intensified with the objective of reducing the likelihood of future crises (BIS, 2010). One of such regulations was the bonus cap, introduced within the EU Capital Requirements Directive (CRD) IV framework. It mandated that the variable-to-fixed compensation ratio of material risk-takers be at a maximum of 100%, or 200% with shareholder approval (EBA, 2021).

The United Kingdom (UK) provides a specially intriguing and relevant environment to examine this regulatory development. Being one of the most important financial centers in the world, the UK was impacted considerably by the financial crisis and the bonus cap that followed. At the same time, the UK has been critical of the efficacy of the bonus cap, and whether regulating compensation would lead to improvements in stability, or create distortions in labor markets (House of Lords EU Committee, 2015). Thus, following Brexit, once the UK was no longer bound by EU policy, the regulators started to show a clear policy shift. In October 2023, the bonus cap was announced to have been abolished for UK banks and investment firms. The regulatory authority would thus no longer be imposing formal restrictions on variable compensation in relation to fixed pay, but rather provide factors for firms to consider when deciding on compensation schemes. The motivation behind the decision was to provide UK banks with flexibility in compensation packages and to ultimately avoid the distortions in the remuneration structure that the bonus cap had created over the years (PRA, 2023).

In light of this background, it is important to understand how the bonus cap actually impacted banks' governance and risk behaviour. This thesis therefore examines the relationship between

executive compensation, executive turnover, and banks' risk-taking in the UK banking sector, both in the implementation of the bonus cap, and its abolishment.

There exist several motivations for our research questions. Firstly, the UK provides an ideal setting for a two-way policy examination where we not only investigate the effects of regulatory implementation but the abolishment of the same, in order to gain a much deeper insight into the mechanics and effects of bonus caps compared to extant literature. Secondly, the banking sector carries systemic importance with the ability to also generate negative externalities and unintended costs on non-financial firms or society as a whole (Colonello et al., 2023). Thus, such a research question could carry societal value and useful policy implications.

According to prior literature, compensation, or specifically variable compensation, carries a vital role in affecting bank risk-taking with bonus caps seen as a regulatory option in curbing it (Thanassoulis, 2012; Efing et al., 2022; Thanassoulis and Tanaka, 2018). Moreover, variable compensation is seen as a tool for executive retention (Oyer, 2004) where bonus caps may come with the unintended effect of higher executive turnover (Murphy, 2013b). Our aim, then, is to examine to what extent bonus caps are an effective regulatory tool to curb risk-taking, and whether it compromises executive retention in banking.

A strand of extant literature argues about the negative effects of bonus caps when it comes to executive retention, specifically because of the transferable skills of banking executives (Murphy, 2013b). However, we also see empirical literature, like that of Abudy et al. (2020) who find no effects of bonus caps on turnover, and Colonello et al. (2023), who find mixed results. With regards to risk-taking, there exists a range of research that advocates for variable compensation, or shareholder-oriented incentive structures claiming it to align managers with the long-term value of the bank (Bhagat and Romano, 2009; Bebchuk and Fried, 2009). However, there are also more recent papers that advocate against shareholder-oriented incentives by pointing to the idea of shareholder protection where they are, to a large extent, protected from excess risk-taking at the expense of society (Thanassoulis and Tanaka, 2018; Anginer et al., 2018). This provides several theoretical tensions which further add importance to our research questions.

We test our hypotheses using a difference-in-differences (DiD) research design with year fixed-effects. We utilise panel data of up to 4295 executive-year observations and 787 bank-year observations collected from various sources like company annual reports, Capital IQ, LSEG Datastream, NYU V-Lab, CRSP and COMPUSTAT. We use data from UK banks in our treatment sample, spanning up to 34 banks, and data from US banks in our control sample, spanning up to 70 banks. Our data spans the period 2011 to 2016, which we address as the implementation period, and 2021 to 2025, which we address as the abolishment period. We find that the bonus cap carries no association with systemic risk and credit risk in either the implementation or abolishment period, and no association with turnover in the implementation period. We do however, observe a significant decrease in turnover post-abolishment, a significant decrease in shareholder risk and performance post-implementation, and a significant increase in shareholder risk and performance post-abolishment. Through conducting robustness tests, we find our baseline results align across several specifications, however, due to limitations mostly pertaining to small sample sizes, we restrict our conclusions to associations rather than causation.

We contribute to prior literature on several dimensions. We add to theoretical literature on bonus caps and empirical literature on variable compensation, turnover and risk-taking by examining an exogenous shock in order to bring deeper insights into this widely explored relationship. We also focus on the abolishment of the CRD IV bonus cap, a setting prior literature has not captured so far and by examining a two-way policy experiment, we contribute by presenting findings that single-event studies may not fully capture. Lastly, we also contribute to UK-oriented research like that of Kleymenova and Tuna (2021) and contrast the findings to EU-oriented studies like Colonnello et al. (2023) and Cerasi et al. (2020).

The remainder of this paper is structured as follows. Section 2 presents the institutional background surrounding CRD IV and the UK regulatory landscape. Section 3 presents our literature review. Section 4 details our data and methodology for our baseline regressions. Section 5 details our baseline findings and robustness results. Section 6 discusses these results by connecting to prior literature and also details the limitations that exist in our study. Lastly, section 7 concludes our thesis.

2. Institutional Background

2.1 Regulatory Timeline Leading Up to the Bonus Cap

The 2008 global financial crisis revealed critical weaknesses in the financial system's approach to managing risk. In the years that followed, executive compensation became a central issue in financial reform. Policymakers argued that bonus and incentive arrangements in the banking sector had contributed to instability by encouraging excessive risk-taking. In late 2009, the Financial Stability Board (FSB) released their "Principles for Sound Compensation Practices". These standards included guidelines and recommendations on e.g. how a "substantial proportion" of senior executive bankers' variable compensation should be paid with multi-year deferral where the deferral period should not be less than three years. The main goal of the principles was to reduce short-term risk-taking, and to link risk-takers' compensation with long-term performance (FSB, 2009).

Building upon these guidelines, regulatory bodies and jurisdictions across Europe started introducing limitations on remuneration practices. This was predominantly driven by the development of the CRD III legislation, which included many of the recommendations that were proposed in the FSB Principles for Sound Compensation Practices. For example, it also crucially outlined how more than 50% of the variable compensation should be given in shares or other equity-based compensation in order to incentivize long-term commitments. The formal regulation of CRD III, directive 2010/76/EU, took effect starting on January 1 2011. Simultaneously in the UK, the financial stability authority (FSA), who at the time were the main regulatory body of financial services, presented a draft for a remuneration code in early 2009, which was enacted starting 2010. The chairman of the FSA, Lord Adair Turner, had been asked to review the issues which had led to the financial crisis and mentioned in his review that "Remuneration policies should be designed to avoid incentives for undue risk taking" (Shalchi, 2021). At the time, the legally binding FSA remuneration code applied to the larger banks and building societies in the UK, and was reviewed by the FSB to be in line with their own published principles. Though, this regulation was later extended to most banks in the UK (Shalchi, 2021).

The bigger reconstruction arrived with the revised CRD IV directive, which was formally adopted during 2013 and applied starting January 2014. The regulation mandated for the first

time an explicit "bonus cap" or a constraint on the ratio of variable-to-fixed remuneration for material risk-takers such as bank executives. More formally, article 94 of Directive 2013/36/EU states that an individual's variable remuneration (e.g. cash bonuses and equity grants), which is given as a consequence of performance and results, could not exceed 100% of their fixed remuneration (e.g. salary, pensions, benefits) in a year. The regulation however allows for the ratio to be up to 200%, provided that 65% of shareholders' votes, who own more than half of all shares, give their approval (European parliament, 2013). In practice, this implied that, for example, an executive receiving a £0.5 million fixed compensation package during a year could receive at most a total of £0.5 million in bonuses and other variable grants (or £1 million if shareholders agreed to the higher cap) (EBA, 2021).

This regulation faced heavy criticism in the financial industry, and sparked a lot of debate. Some claimed that while the intention of this regulation, to realign incentives and reduce excessive risk-taking, was good, the introduction of these binding caps on variable payments had clear tensions against financial stability and competitiveness in managerial labor markets. European banks had to, in order to retain talent and skills, increase the fixed remuneration of their workers and indemnify them for their reduced variable pay. Comparisons in 2014 showed that at the managing director level, net levels of pay were lower in the UK compared to the US and Hong Kong (AFME, 2016). Arguing on this point of indemnification, its consequences for financial stability and reduced flexibility, the UK government formally contested the implementation of the bonus cap before the Court of Justice of the European Union in late 2013, but was ultimately rejected (House of Lords EU Committee, 2015). Thus, the CRD IV standards were implemented in the UK through the Financial Services Act 2013 and in the rulebooks of the FSA's successors, the Prudential Regulation Authority (PRA) (which is part of the Bank of England and focuses on banks and systemic stability), and the Financial Conduct Authority (FCA) (which oversees conduct and remuneration rules).

2.2 Implementation of the Bonus Cap and Subsequent Developments

Within the CRD IV, under article 92 (2), there exists a clause related to proportionality which states that the remuneration rules under this regulation is to be applied in a manner and to the extent that is appropriate to their "size", "internal organisation" and the "nature, scope and complexity of the institution's activities" (EBA, 2021). This is an open-ended wording which

enables different national jurisdictions to interpret proportionality in different ways. In the UK, the FCA translated the vagueness relating to proportionality into a tiered framework for the firms that fell under their regulation, and gave guidelines on what principles of the CRD IV may potentially be circumvented. However, the FCA placed importance that the levels were not automatically enforced, as firms still needed to assess their circumstances and prove to the FCA why they believed not applying the regulation was reasonable. For the dual-regulated firms (firms regulated by both the FCA and PRA), being a level 3 firm (a firm with less than 15bn £ in total assets) implied you could avoid applying rules on retained shares or other instruments, deferral, and performance adjustment, and in some cases the bonus cap (FCA, 2017).

The European Banking Authority (EBA) decided to publish a public consultation on its guidelines on sound remuneration policies in order to understand how the regulation was received. Some UK based firms argued that the notion of proportionality was critical, and that removing it could cause distortions as a consequence of increased indemnification, compliance costs and reduced focus on the more critical issues that smaller banks needed to focus on (EBA, n.d). Further, retaining the proportionality also allowed for preserved flexibility needed for smaller banks to compete, recruit, and retain staff (Paragon, 2015).

Ultimately, the European Banking Authority (EBA) drew a harder line on this issue than what many would have wanted. In their press release, they stated that the guidelines are made to help institutions in applying remuneration practices correctly. However, the disparity in national application of proportionality led to EBA's opinion that clarification through legislative action was necessary. They explain that the regulation allows for possible exemptions for "deferral arrangements and pay out of instruments" for "small and non-complex institutions" and for staff receiving only small amounts of variable remuneration. However, it was explicit that the bonus cap itself should not be subject to any exemption. Thus, there existed a tension between the EBA's interpretation and the FCA's more flexible domestic implementation (EBA, 2015).

2.3 Abolishment of the Bonus Cap in the UK

Following the exit from the EU in 2020, the UK had to decide on if they wished to retain, change or remove the EU based policies for remuneration. The EU had just previously released the newer CRD V in 2019, which implied some smaller adjustments that didn't directly concern the

bonus cap as it remained unchanged. The UK authorities had for a longer period of time been against the bonus cap, which they objected to even back in 2014 when it was initially implemented (Jones, 2023).

Thus, in 2022 the PRA and FCA released a joint public consultation where they decided to reevaluate if the bonus cap was still fitting for the UK financial market. They argued that the regulation had over time created some disruptive effects, through an "upward pressure" on fixed types of compensation. This in turn caused increases in fixed costs and hindered flexibility such as absorbing losses when you face an economic downturn. Its removal was phrased as a way to restore flexibility, strengthen the link between pay and risk outcomes, and improve the UK's international competitiveness by increasing firms' ability to attract and retain talent. The regulation on bonus caps was thus formally abolished in October 2023 (PRA, 2023).

3. Literature Review

3.1 Executive Compensation Regulation on Turnover and Exits

The understanding of literature on executive labor markets is that compensation is not only a way for companies to incentivize performance, but serves as a critical tool in retaining and acquiring talent and competence. This is mainly due to executives being seen as skilled and mobile, with many options to utilize their competences. As a consequence of this, firms wish to shape the compensation schemes in a manner that reduces the risk of turnover by making it costlier to leave. For instance, Oyer (2004) provides evidence that executive compensation contracts are designed to adjust to managers' outside options and labor market conditions, indicating that compensation also plays a role in making it more difficult for executives to leave, rather than simply rewarding executives' effort. This becomes especially relevant when discussing external regulation that impacts the level of compensation for executives.

Examining the effects of remuneration caps on turnover of executives and banks in literature, shows us that the impact is to a large extent complex and ambiguous. Philippon and Reshef (2012) provide the historical and theoretical background for discussing regulation as a driver of talent reallocation in finance. In their long-run analysis, they show that the 1980-1990 U.S. financial deregulation was one of the leading causes behind the huge rise in financial sector wages and skill intensity relative to other industries. During this period of deregulation, the industry saw an inflow of highly skilled workers and the re-emergence of wage premiums, being classified as a "brain gain". In contrast, the paper shows that periods of tighter regulation, such as the decade following The Great Depression in the 1930s, appear associated with declines of skill levels, so called "brain drains". Given these findings, examining the effects of the introduction and subsequent abolishment of a regulation such as the CRD IV bonus cap, becomes highly relevant. Indeed, in line with this view, Murphy (2013b) argued that introducing the stricter bonus caps in the EU could backfire, as they increase incentives for risk-taking and induce banks to shift pay from variable to fixed, effectively reducing banks' global competitiveness. He predicts that by raising fixed salaries, to compensate for lost bonuses, the outcomes would eventually be a degradation of banking talent quality. This is possible as highly skilled bankers possess transferable skills and an immense amount of options for alternate employment (Murphy, 2013b).

With regards to this theoretical background, evidence from differing regulatory papers are quite mixed. Abudy et al. (2020), which studies an exogenous Israeli law that capped bank executives' pay, provides one counterpoint to the predictions made by Murphy (2013b). They examine the exogenous shock in the 2016 Israeli remuneration law that constrained executive pay in financial firms to a maximum of 35x relative to the lowest paid employee in the firm. They find that despite the magnitude of the pay reduction, the affected firms did not find systematic increases of executive turnover relative to unaffected firms. This is in line with Cerasi et al. (2020)'s findings on turnover after the introduction of FSB's "Principles for sound compensation practices". They find that alterations in the pay structure of EU executives did not correlate with higher CEO churn rates.

A paper that shows evidence contrary to Cerasi et al. (2020) and Abudy et al. (2020), and which proves to be geographically relevant for our thesis, is the paper written by Kleymenova and Tuna (2021). They find that unforced CEO turnover increased among UK banks following the introduction of the UK Remuneration Code[1], relative to other large UK firms and foreign banks. Although the UK Remuneration Code emphasized other forms of regulations on remuneration rather than a direct bonus cap, the UK results are important because they show that regulatory interventions on compensation, regardless of their exact form, can result in indirect outcomes in the form of higher voluntary CEO turnover.

The most relevant evidence on CRD IV however comes from Colonnello et al. (2023). They examine the EU bonus cap with an executive level DiD framework design and provide a nuanced view that bridges Philippon and Reshef (2012)'s findings and Abudy et al. (2020)'s counterevidence. They find an increase in the aggregate turnover among executives in the EU following the introduction of CRD IV, but show that this increase is concentrated among CEOs at underperforming banks rather than a broad voluntary industry wide exit of skilled managers.

[1] Unlike CRD IV's bonus cap, the 2010 UK Remuneration Code more so emphasised deferral and performance-vesting rules. At least 50% of bonuses had to be deferred for at least three years and must have performance-based vesting conditions. Long-term incentive plans (LTIPs) also had to have risk adjustments (Kleymenova and Tuna, 2021).

3.2 Executive Compensation Regulation on Broader Risk-Taking and Financial Stability

3.2.1 Executive compensation regulation and its effect on shareholder risk

Much of the literature on executive pay regulation and risk-taking emphasizes broad social impacts (systemic stability, externalities) rather than firm-level agency and value creation. However, there is still research which grounds itself on explaining the tension that exists between shareholders and executives.

Abudy et al. (2020)'s study on the Israeli compensation caps tests two theories: value-maximizing contracts versus rent extraction. Contrary to the view that a pay limit would reduce firm value, they find positive stock returns for affected banks when the law passed. This market reaction implies that, prior to the cap, shareholders had been "overpaying" executives which is consistent with rent extraction theories. Importantly, firm performance did not fall after the cap, implying that reducing excess pay did not hurt (and likely helped) shareholder value.

Further, Efing et al. (2015) examine banks in Austria, Germany and Switzerland over the period of 2004–2011. They find that before the Great Financial Crisis (2003–2007) banks had very high incentive pay and that this pay was strongly correlated with trading income and its volatility. Specifically, Efing et al. (2015) find that these pre-crisis bonuses were "too strong" for an optimal risk–return trade-off, i.e. executives were over-incentivized to take risks. However, during the crisis period (2008–2011), banks sharply cut bonus shares (a ~ 40% drop in trading divisions) even though overall trading profits did not fall, leaving traders under-incentivized. In other words, compensation shifted from over- to under-incentivizing risk. This truly highlights the ongoing governance challenge that shareholders face, wherein you want the managers to bear a fair share of risk and rewards. Too generous pay pre-crisis may have lured managers into excessive risk (above what shareholders would choose), while the post-crisis pullback could have discouraged profitable risk-taking.

Finally, DeYoung et al. (2013) analyze U.S. commercial banks from 1995–2006, a period spanning the Glass–Steagall repeal (1999) and credit boom. They show that industry deregulation gave banks new growth opportunities and led firms to increase CEOs' vega. Banks also shifted from an originate-and-hold to an originate-and-securitize model, thus exploiting

deregulation to achieve higher profits, albeit with higher risk. Crucially, DeYoung et al. (2013) find that boards generally reacted by moderating incentives when risk rose (reducing vega and raising performance sensitivity). However, this internal governance feedback failed in the very largest, most aggressive banks. This deepens the agency conflict: without effective internal checks, shareholders may push for regulatory limits on pay to control excessive risk-taking.

Together, these studies highlight tensions directly relevant to the UK's CRD IV bonus-cap context. Abudy et al. (2020)'s evidence suggests that if executives were previously extracting rents, capping it can raise shareholder value. Efing et al. (2015) however shows that incentive pay must be carefully calibrated to achieve an optimal balance, as overly generous bonuses lead to excessive risk, while too steep cuts will inhibit value-maximizing behaviour among agents. DeYoung et al. (2013) further demonstrate that internal governance is crucial to shareholder welfare, as misaligned incentives become especially dangerous in the very largest banks. Hence, applying this to the CRD IV setting, regulators may improve shareholder welfare by curving rent extraction, but also risk under-incentivizing managers if caps are too tight.

The second segment of this strand of literature concerns the effects of executive compensation structure, and its regulation, on societal value. Colonnello et al. (2023) state that financial firms differ from non-financial firms due to "pervasive regulatory oversight and the presence of explicit and implicit government safety nets". Both these reasons "reflect the systemic relevance of bank stress, which can generate negative externalities for non-stressed banks, non-financial firms, and house-holds", thus making research on executive compensation structure in banking socially relevant.

3.2.2 General Literature on Variable Remuneration and Risk-Taking

A portion of this strand of literature addresses the effects of variable remuneration on risk-taking and societal value without examining regulation directly. For example, Thanassoulis (2012) asks the fundamental question on why the banking industry has higher bonuses than non-financial industries. He says that "the high bonuses cannot just be the necessary incentives bankers require to work" (Thanassoulis, 2012). He first finds that banks will prefer to pay their employees in bonuses rather than fixed wages because it allows for risk-sharing. Efing et al. (2022) and Murphy (2013b) also confirm these risk-sharing motives with the latter even stating that the

“initial purpose for year-end cash bonuses was not the provision of incentives, per se, but rather a mechanism to ensure that remuneration expense would be low in years with low profitability, and high in years with high profitability” (Murphy, 2013b).

Further Thanassoulis (2012) finds that, because of intense competition for bankers in the labour market, there is a negative externality that pushes bonuses up and increases the default risk of banks. The negative externality exists because competing banks propose higher and higher bonuses that the employing bank needs to match but the costs of these higher bonuses are only for the employing bank to bear. Hence, when employing banks face these exuberant costs, default risk also increases.

Thus, from Thanassoulis (2012)’s point-of-view, the most optimum regulation is one that reduces this negative externality while also maintaining this insurance property of bonuses (or variable remuneration in general). He proposes a “modest” and “maximally weak bonus cap” on the proportion of the bank’s balance sheet that can be allowed for bonuses. This is then specific to different bank sizes, and it allows for just enough to pay for bonuses without increasing fixed wages, while also reducing the size of the negative externality. He does not, however, advocate for a “stringent bonus cap”, like the one in CRD IV. With stringent bonus caps, the intense labour competition and the negative externality will continue to exist. The only difference is that now banks are forced to pay the exuberantly high compensation costs in fixed wages, thus eliminating the insurance property of bonuses.

Other papers within this stream of literature also contain their own recommendations of optimal compensation regulation. For example, Efing et al. (2022) also emphasise the negative effect of bonus caps. The intention of CRD IV was to curb excessive risk-taking by lowering the incentives of employees, however, if bonuses are not designed to be incentive-based pay, but rather a risk-sharing mechanism, it will simply impede risk-sharing between employees and shareholders. Roche et al. (2023) propose an alternative solution with creating an independent “systemic risk authority” that would create more nuanced resolutions for compensation regulation. This is because managers, being impatient, would find ways to overcome a simple bonus cap similar to the indemnification of executives as seen in Colonello et al. (2023).

Carlson and Lazrak (2010), although not commenting specifically on bonus caps, theorise a model of a risk-averse manager having a performance-sensitive (stock) and performance-insensitive (cash) component in their compensation. Empirically, they find that as cash:stock ratios rise, risk-taking also rises because a higher fixed compensation implies a form of insurance to conduct risk-shifting. Thus, in line with Colonello et al. (2023), as managers are indemnified after the bonus-cap, risk-taking has seemed to increase because of a higher fixed component.

Thanassoulis and Tanaka (2018) introduce an added nuance of deposit insurances and bailouts in their analysis of the relationship between executive compensation and risk-taking. They find that excessive risk-taking occurs when compensation contracts are aligned heavily with shareholder interests. Certain policy instruments like paying with debt, clawbacks and malus are also shown to be ineffective. The latter two can reduce risk-taking but can also be counteracted by shareholders if they design executive pay to be more convex with equity outcomes.

Anginer et al. (2018) have similar findings, however they look at the broader corporate governance of banks. They find that shareholder-aligned corporate governance increases risk because of deposit insurances, liquidity and too-big-to-fail protections. Ongena et al. (2022) take this added nuance of protection a step further by finding that the relationship between executive compensation and risk-taking becomes more pronounced in times of recession "when the perceived probability of government support increases" (Ongena et al., 2022).

Although recent literature is not very advocative of shareholder-aligned incentive structures, earlier literature, post the financial crisis, seem to hold the opposite view. Bhagat and Romano (2009) advocate linking compensation to long-term performance by making sure that incentive compensation only consists of restricted stock and stock options. It should be restricted in the sense that it can only be unwound (sold or exercised) two to four years after the resignation or last day of the executive to prevent the executives from taking on short-term actions like public statements or stock manipulations to then unwind the equity grants immediately.

Bebchuk and Fried (2009) also similarly advocate for the restriction of equity grants but they "argue against the proposal that executives should be prevented from unwinding equity

incentives until their retirement" since this can distort the decision to retire and also undermine long-term incentives close to the time of retirement (Bebchuk and Fried, 2009).

Core and Guay (2010) is another paper that discusses this issue but instead of advocating for or against shareholder-aligned incentives, it states that if incentives are set too low, there will be unresolved agency conflicts, and if incentives are set too high, executives would want a higher total pay as a risk premium for having a high level of variable remuneration and/or they may be excessively risk-averse in the operational decisions of the company.

Thus, over time, we see how the views with regards to shareholder-aligned incentives have changed. Bhagat and Romano (2009) seems to be the most extreme in their views in advocating for shareholder-aligned remuneration. Bebchuk and Fried (2009), although taking the same side, seem to be less extreme in the issue of when equity grants can be unwound. Core and Guay (2010) seems to be a "middle-ground" while Thanassoulis and Tanaka (2018), and Anginer et al. (2018) seem to be against the idea of shareholder-aligned incentives. The reason for this shift is because of the addition of shareholder protection within the analysis of executive compensation and risk-taking. This changing paradigm could have been the basis for regulation like CRD III and CRD IV, especially with regards to the bonus cap.

3.2.3 Empirical Literature Directly Examining Compensation Regulation

We also find literature that empirically and directly examines the effects of bonus caps or other compensation regulations by looking at real regulatory environments. For example, Colonnello et al. (2023) investigate the effects of the 2014 CRD IV bonus cap on executive turnover and risk-taking using a DiD model with 45 large EU banks. They employ several risk measures pertaining to different stakeholders like shareholders (market beta), debtholders (CDS spreads) and society (expected capital shortfall (or SRISK%) and $\Delta CoVaR$ as calculated in Adrien and Brunnermeir, 2016). Although the intention of the bonus cap was to curb excessive risk-taking, Colonello et al. (2023) find no evidence of a significant decrease in any of its risk measures. It does, however, find a significant increase in CDS spreads and SRISK% entailing higher risk levels for debtholders and society, in general.

Our study focuses on the UK landscape as it provides a unique regulatory environment. With CRD IV being implemented in 2014, it was also taken away post-Brexit in late 2023. There has been extant literature on compensation regulation within the UK. Kleymenova and Tuna (2021) examine the 2010 UK Remuneration Code, stemming from CRD III. They find a general increase in systemic risk contribution and sensitivity post-2010 for both their treatment and control groups but find that the increase in the measures for UK banks is lower than that of the control groups. In other words, the results with regards to risk seem to be in line with the intentions of the regulation (i.e. to curb excessive risk-taking incentives).

Ferri and Maber (2013) study the effects of the say-on-pay regulation instilled in the UK in 2002 which mandated a non-binding shareholder vote on executive pay. Generally, after the announcement of the regulation, they find positive abnormal returns on firms that had executive compensation structures that were value-destroying for shareholders. Moreover, they find that compensation contracts were changed mainly for firms that had high dissent from shareholders on existing compensation practices. Overall, the say-on-pay regulation was value adding for shareholders.

Provided this empirical background of the UK banking environment, to the best of our knowledge, there has not yet been a specific study in the UK on the implementation of CRD IV and its subsequent abolishment.

3.3 Contributions to Extant Literature

We contribute to both strands of literature. In terms of the tensions between executive compensation and turnover, this thesis makes three concrete contributions. First, by focusing on the regulatory episode in the UK, the introduction of the stricter compensation constraints in 2014, and their subsequent abolishment in 2023, we examine a two-way policy experiment that allows for a full-scope understanding of the CRD IV regulatory effect, something that single-event studies often cannot fully observe. Second, we add perspective to competing empirical narratives where we examine the mechanism emphasized by Philippon & Reshef (2012) and Murphy (2013b), that regulation reallocates talent, against the contrasting evidence found in Abudy et al. (2020). Third, we refine and add to prior UK-focused work such as Kleymenova & Tuna (2021), and contrast it against the findings of EU oriented studies of

Colonello et al. (2023) and Cerasi et al. (2020). By adopting a difference-in-differences methodology with event-study windows around both the 2014 intervention and 2023 deregulation, the thesis clarifies if regulation causes broad “brain drains” and subsequent ”brain gains”, with implications for policy design and the trade-offs of future remuneration rules.

In terms of the tensions between executive compensation and risk-taking, we contribute on several dimensions. Whereas Bebchuk and Fried (2009), Bhagat and Romano (2009) and Core and Guay (2010) are critical papers on existing compensation practices with policy recommendations, we study policy implementation and abolishment using real empirical data. Moreover, we add to the theoretical frameworks proposed by Thanassoulis and Tanaka (2018), Thanassoulis (2012) and the predictions by Murphy (2013a) and Murphy (2013b) by testing whether bonus caps indeed contribute to excessive risk-taking, specifically, in the UK setting.

Of course, there are empirical papers that do test the relationship between executive compensation structure and risk-taking like Abudy et al. (2020), Efing et al. (2015), Efing et al. (2022) and Carlson and Lazrak (2010). Unlike Abudy et al. (2020) and Efing et al. (2015), we focus more so on the societal and shareholder effects of changes in executive compensation structure rather than just the shareholder effects. Moreover, we also contribute to the rest of these papers by examining an exogenous shock using a quasi-experimental research design in order to further cement research on the relationship between executive compensation and risk-taking.

We contribute to UK-oriented research like Kleymenova and Tuna (2021) and Ferri and Maber (2013) by looking at the implementation and abolishment of a newer, and perhaps more stringent, regulation (i.e. the bonus cap) within the UK setting. Lastly, we contribute to Colonello et al. (2023) by employing a similar research design but zooming in into the UK landscape when CRD IV was implemented, and more importantly, examining the effects of the abolishment of the bonus cap in CRD IV along with its implementation.

3.4 Research Questions and Hypotheses

The theoretical claims and empirical findings with regards to variable compensation, compensation regulation and bonus caps on turnover and risk-taking seem incredibly mixed, and thus to explore these theoretical tensions, our research questions are as follows:

(1) Does the relationship between executive compensation and executive turnover in the UK change subsequent to the introduction of the bonus cap of CRD IV?

- H_0: The introduction of the bonus cap of CRD IV has no significant impact on turnover in the UK.
- H_1: The introduction of the bonus cap of CRD IV has a significant impact on turnover in the UK.

(2) Does the relationship between executive compensation and bank risk-taking in the UK change subsequent to the introduction of the bonus cap of CRD IV?

- H_0: The introduction of the bonus cap of CRD IV has no significant impact on risk-taking in the UK.
- H_1: The introduction of the bonus cap of CRD IV has a significant impact on risk-taking in the UK.

(3) Does the relationship between executive compensation and executive turnover in the UK change subsequent to the Post-Brexit abolishment of the bonus cap in 2023?

- H_0: The abolishment of the bonus cap of CRD IV has no significant impact on turnover in the UK.
- H_1: The abolishment of the bonus cap of CRD IV has a significant impact on turnover in the UK.

(4) Does the relationship between executive compensation and bank risk-taking in the UK change subsequent to the Post-Brexit abolishment of the bonus cap in 2023?

- H_0: The abolishment of the bonus cap of CRD IV has no significant impact on risk-taking in the UK.
- H_1: The abolishment of the bonus cap of CRD IV has a significant impact on risk-taking in the UK.

4. Data and Methodology

In this section, we detail our definitions of dependent, independent and control variables, sample selection, the Difference-in-Differences (DiD) research design and our empirical methodology.

4.1 Variable Definitions

In terms of dependent variables, for the turnover analysis, we use a variable called turnover which is a binary variable equal to 1 when an executive director leaves the board, for the UK banks, or when an executive employee leaves the company, for the US. Within the UK context, we consider an executive director a manager with executive managerial responsibility who is also part of the board. In the US context, however, we focus on executive managers regardless of board appointment given the data that is available to us (see section 3.3 Data Collection).

We also use a variable called turnover poor performance which is equal to turnover if the ROE of the bank in that specific year is lower than the median of the previous year's sampled banks, and 0 otherwise. Like Colonnello et al. (2023), we utilize this as a proxy for forced turnover. Colonello et al. (2023) states "The true nature of turnover is ultimately unobservable; we do not know if turnovers are due to executives' or employers' choices. But turnover at well-performing banks is arguably more likely to originate from executives' choices and therefore' offers a plausible approximation of voluntary turnovers. Conversely, turnover at poorly performing banks is consistent with executives being forced to leave" (Colonello et al., (2023).

Colonello et al. (2023) also emphasise the difficulties in gauging voluntary and forced turnover through media searches (as papers like Jenter and Kanaan (2015) do) due to the lack of information on most non-CEOs. Thus, they adopt a method like Jenter and Lewellen (2021) where they analyse turnover rates at different performance levels, a method that we adopt as well. The limitation with this is, however, the risk of misclassification of voluntary turnover as forced and vice versa (Colonnello et al., 2023).

With regards to risk-taking, banks are highly interconnected institutions with several stakeholders. The alteration of compensation structures for executives thus changes the agency relationship between management and stakeholders. The two key stakeholders with a direct

claim on a bank's payoffs are shareholders and debtholders and seniority differences can thus generate an agency conflict (Colonnello et al., 2023). Therefore, we measure both credit and shareholder risk and performance in our risk-taking analysis. Alongside direct claimants, public guarantees on a bank's debt carries with it the threat of negative externalities, hence, we also measure systemic risk.

We employ similar metrics to Colonnello et al. (2023) to proxy for risk on different dimensions. To measure credit risk, we use the natural logarithm of the 5-year excess credit default swap (CDS) spreads. The excess CDS spread is the difference between the CDS spread of the bank and the CDS spread of the sovereign in which the bank is located in the last quarter of the year. For the sovereign CDS spreads, we used the full restructuring contract CDS spreads for the United Kingdom Sovereignty and the full restructuring contract CDS spreads for the United States Sovereignty.

To measure for stock risk and performance, we utilise the market beta and Sharpe ratio specifically. Lastly as a proxy for systemic risk, we use SRISK%, representing the expected capital shortfall. This is defined as the capital shortfall that the entire financial sector would experience in the event of the specific bank's failure.

We then also utilise controls in conformity with past research, specifically, in Colonnello et al. (2023) using bank- and executive-level controls for our turnover analysis and only the bank level controls for our risk-taking analysis. Executive-level controls include (1) a binary variable equal to 1 if the executive is a CEO, (2) the executives' age, (3) tenure which is defined as how long an executive has been on the board of the company, in the UK, or how long an executive has been employed as an executive employee, in the US, (4) and a binary variable equal to 1 if an executive is a female. We use these to control for executive level characteristics unrelated to our research question that may confound the relationship between the treatment and turnover.

The bank level controls for our baseline regressions only include return on equity (ROE) defined as earnings from ongoing operations divided by total average book value of equity and Earnings from ongoing operations is simply interest income - interest expenses + non-interest income -

non-interest expenses - unusual items. Unusual items include restructuring costs, asset writedowns and litigation/insurance settlements. We include these variables to control for bank performance which may, similarly, confound the relationship between treatment, and turnover and risk-taking. We use solely ROE above other performance or size measures given issues of multicollinearity between such variables. We choose ROE being the best representation of performance in the banking industry, specifically, especially since ROE better explains banks' stock market values (Pennacchi and Santos, 2021). The full variable definitions table can be found in Appendix A.

4.2 Sample Selection

4.2.1 Sample Selection for the Turnover Analysis

For our turnover sample selection in our treatment group, we utilised the Prudential Regulation Authority's (PRA) list of regulated banks in the UK. Specifically, we looked at "Banks incorporated in the UK authorised to accept deposits". In total this comprised 149 banks. From this, we collected data on executives from 46 banks (which includes both group level and bank level entities) due to constraints regarding hand collection and/or a lack of data availability with regards to turnover. We also had to exclude banks that did not have a clear segmentation between their executives and non-executives. This, in total, comprised 1064 executive year observations. This initial sample, however, included certain regulated but inapplicable firms and two Iranian banks which were subject to sanctions (Mann and Denton, 2016; UK Parliament, 2007).

As for the firms that were regulated but still inapplicable, due to the open-endedness of the regulation especially with regards to the issue of proportionality, as mentioned in the Institutional Background, certain smaller UK banks, on the group level simply decided to overlook the regulation. For example, the annual report of Schroders PLC in 2013 states "clarification of the PRA's approach means this cap does not apply to us." (Schroders PLC, 2013) whereas the annual report of Rathbone Brothers PLC states "There remains uncertainty regarding the implementation of CRD IV [...] we currently intend to seek shareholder approval for a variable pay cap of 200%" (Rathbone Brothers PLC, 2013). Thus, in smaller banks, we see a disparity in the interpretation and compliance of the regulation, and hence, we deemed some of these banks

as inappropriate for our sample if they exhibited direct evidence of non-compliance to CRD IV. After excluding these firms, we were left with 1004 executive year observations and 43 banks.

After this, we merged the sample with bank-level controls where we had to drop more observations due to missing data points. Since executive-level data and bank-level data were obtained from different sources (see section 3.3 Data Collection), metrics like ROE were unavailable for certain banks in certain years. After merging these controls, we were left with 844 executive year observations and 34 banks. Of these, 512 observations were used for the analysis during the implementation of the bonus cap, and 332 for the abolishment.

As for the US executives, we started by obtaining a list of all firms within the CRSP-COMPUSTAT database alongside data on total assets as of 31st December 2025. We then filtered on the "USA" country code and the banking industry by only including banks where the first five digits of the NAICS was 52211 (which denotes commercial banking). This resulted in a list of 165 banks after which we filtered for the top 70. Since we only had 34 banks in the treatment sample, we decided to only use 70 in the control sample in order to ensure a reasonable proportion between the treatment and control sample size. We then collected executive-level data that comprised 124 263 observations and 2729 firms across North America, however, after filtering for just the largest 70 banks and removing certain observations due to lack of data availability on executive level controls, we ended up with 3468 observations and 62 banks. After merging with bank level controls and removing missing observations, we were left with 3451 observations and 62 banks. Of these, 1999 observations were used for the analysis on the implementation of CRD IV and 1452 on the abolishment.

4.2.2 Sample Selection For The Risk-Taking Analysis

For the sample selection on the risk-taking analysis for our treatment sample, we started with the PRA list of 149 firms and narrowed down the banks where we were able to find data on our risk-taking dependent variables. Rather than a top-down approach by narrowing down a large universe of data based on our applicable banks, we used a bottom up approach by selecting all the banks that were applicable for our sample, and for which data was available. With this, we found 108 bank-year observations and 12 unique banks. We then merged these observations with

the bank-level controls and found no need to drop any observations, hence retaining 108 bank-year observations and 12 banks. Of this, 48 observations were used for the implementation of the bonus cap and 60 for the abolishment.

As for the control sample, our universe of data consisted of 88 093 observations and 17 100 firms across North America. However, after filtering for only the top 70 largest banks in the US, we were left with 679 bank-year observations and 70 unique banks. We dropped no observations due to the merging of bank level controls. Of the 679 observations, 340 were used for the implementation period and 339 for the abolishment period. The sample selection tables are presented below.

Table 1: Sample Selection

Panel A: Sample selection table for turnover analysis: treatment sample		
	Executive year observations	Number of Banks/Firms
Initial sample	1064	46
After excluding inapplicable banks	1004	43
After merging bank level controls	844	34
Panel B: Sample selection table for turnover analysis: control sample		
	Executive year observations	Number of Banks/Firms
Initial sample	124,263	2,729
Only including largest 70 US banks and excluding missing executive level control rows	3468	62
After merging bank level controls	3451	62
Panel C: Sample selection table for risk-taking analysis: treatment sample		
	Bank-year observations	Number of Banks/Firms
Initial sample	108	12
After merging bank level controls	108	12
Panel D: Sample selection table for risk-taking analysis: control sample		
	Executive year observations	Number of Banks/Firms
Initial sample	88,093	17,100
Only including largest 70 US banks	679	70
After merging bank level controls	679	70

4.3 Data Collection

For our treatment sample, we hand collected data for turnover and executive-level controls using annual reports and the gov.uk website for variables like age and tenure. For our control sample, we collected executive level data from Execucomp. Execucomp, however, does not provide clear turnover metrics, thus similar to Colonnello et al. (2023), we set the turnover variable as 1 in the year when the executive last appears in the database.

As mentioned in the section on variable definitions, we focus on executives regardless of board appointment due to the structure of data in Execucomp. Execucomp reports the five most compensated executive employees within a company regardless of board position, hence, filtering for only executive directors would limit the control sample size to a great extent. Thus, aligned with Colonnello et al. (2023), we use a slightly broader definition for executives in the control group compared to the treatment group.

For both the treatment and control groups, we sourced CDS spreads data from LSEG Datastream and Capital IQ. Although Datastream gives a larger coverage in CDS spreads, for a number of banks in certain time periods, we found data that was either unavailable or CDS spreads that stayed constant (and thus may have been wrong). Hence, we complemented this with data gathered from Capital IQ. This data was on a daily frequency for both the banks and the sovereign and so the mean was taken to find yearly values.

As for the market betas in our treatment sample, we sourced the data from LSEG Datastream using the datatype for historical betas (897E) which uses the FTSE all-share index as the market index in its calculations. As these were monthly betas, we took the mean in order to calculate the annual values. As for the Sharpe ratio, we collected the monthly 10-year UK government bond yield for the risk-free rate and the monthly percentage changes in price for the stock returns. Then, subtracting the risk-free rate from the stock returns, we were able to calculate the excess returns of the banks in our sample. We then divided the yearly mean of the excess returns against the yearly standard deviation of the same. For our control sample, however, we utilised WRDS Betasuite to extract market beta and excess returns values. The market beta in Betasuite uses the CRSP Value-Weighted Index as its market index. This data was also on a monthly frequency and yearly values were calculated in the same way as that for the treatment group.

We sourced the SRISK% from NYU V-Lab. More specifically, we looked at the Global Dynamic MES under its Systemic Risk Analysis section. This was also collected on a monthly frequency, and so we calculated yearly means to use in our regressions.

Lastly, in terms of bank level controls, we collected ROE data from Capital IQ for our treatment sample, and used COMPUSTAT for the control. Whereas Capital IQ provides precalculated ROE values, we manually replicated these calculations via COMPUSTAT variables for the control group. Our descriptive statistics are shown below in table 2.

Table 2: Descriptive Statistics

	Treatment						Control					
	2011 to 2016 period			2021 to 2025 period			2011 to 2016 period			2021 to 2025 period		
Variables	N	Mean	Stdev	N	Mean	Stdev	N	Mean	Stdev	N	Mean	Stdev
Turnover	512	0.178	0.383	332	0.178	0.434	1999	0.116	0.320	1452	0.269	0.444
Log 5-year excess CDS spreads	42	4.325	0.607	35	3.394	0.833	75	4.406	0.858	75	3.795	0.760
Market Beta	48	1.459	0.605	60	1.727	0.468	340	1.173	0.399	339	1.116	0.241
Sharpe Ratio	48	0.026	0.349	60	0.147	0.357	340	0.013	0.327	339	0.012	0.199
SRISK%	36	0.094	0.109	30	0.113	0.102	320	0.010	0.039	285	0.009	0.032
Age (Years)	512	51.715	6.016	332	54.564	4.910	1999	66.957	7.267	1452	59.337	7.488
Tenure (Years)	512	3.998	3.923	332	3.283	3.407	1999	8.237	7.416	1452	9.320	8.231
ROE	252	0.043	0.163	220	0.092	0.076	435	0.103	0.055	380	0.114	0.064

This table contains the descriptive statistics of the dependent and control variables used in our statistical analyses. It has been split between the treatment (UK banks) and control (US banks) groups, and between the implementation (2011 to 2016) and abolishment (2021 to 2025) period. Variable definitions are contained in section 3.1 and the data collection process is contained in section 3.3.

In our descriptive statistics, we observe several interesting patterns. For the treatment group's Sharpe ratio, we see that the mean in the abolishment period is a much higher 0.147 compared to the implementation period where it is 0.026. Further, we see a large discrepancy between the treatment and control group's SRISK% means, with the control group exhibiting a much lower value (0.039 in the implementation period, for example) compared to the treatment group (0.094 in the implementation period). We believe this has to do with the nature of the samples. Given that we only use PRA-regulated UK banks in our sample, NYU V-Lab only contains SRISK% data on the largest firms in that overall sample. For the US, however, we use the largest 70 commercial banks, thus, NYU V-Lab provides coverage for banks of different sizes. Given that larger banks are usually more systemically risky, SRISK% is thus higher for the UK treatment bank. Lastly, we also see a much higher mean tenure for the US control group (8.237 in the implementation period, for example) compared to the UK treatment sample (3.998 in the implementation period). This is likely due to the broader definition of executives that we have

used for the US (see section 4.3) compared to the UK. Thus, it is likely that on average, an executive employee would remain an executive in a company longer than an executive director would remain a director.

4.4 Research Design And Empirical Methodology

We have employed a Difference-in-Differences (DiD) linear probability model in order to examine both the effects of the implementation and abolishment of CRD IV in the UK. We utilise an OLS research design as compared to a logit regression with regards to the turnover study given the possibility of adding year fixed-effects. Fixed-effects within logit regressions resulted in complete or quasi-complete separation due to our small sample sizes. Nonetheless, we utilised logit regressions without fixed-effects as part of our robustness tests (see section 5.3.1) and found results that aligned with our baseline OLS regressions. For our treatment sample, we utilised data from UK banks and for the control, data from US banks. With this we have also ensured that there were no related formalized regulation changes in the US similar to the UK within the two periods that we examine, with no similar compensation constraints being implemented or abolished. More specifically, our treatment binary variable, $treat$, is denoted as 1 for all UK banks and 0 for all US banks.

Colonnello et al. (2023), however, uses a variable called "treatment intensity" as their right-hand side predictor. This variable varies between executives based on how much their maximum variable-to-fixed compensation ratio changed after the implementation of the regulation. Such a variable does provide for a more nuanced regression analysis, however, in our sample, we only found executive-specific remuneration data in a few of the largest banks in the UK, and thus, to maximise on sample size, we replaced it with a binary treatment variable.

To examine the effects of the implementation of the bonus cap, our panel data spans the period between 2011 and 2016. We thus have a variable called $post$ denoted as 0 for the years of 2011 to 2013 and 1 for the years of 2014 to 2016 given that CRD IV came into effect in the UK on 1st January 2014. With regards to the abolishment of the bonus cap, our data utilises the period between 2021 and 2025 with $post$ now denoted as 0 for the period between 2021 and 2023 and 1 from 2024 to 2025 given that the bonus cap was abolished formally on the 31st of October 2023.

The DiD model for the turnover analysis is designed as follows:

$$y_{ijt} = \beta_0 + \beta_1 \bullet treat + \beta_2 \bullet post + \beta_3 \bullet treat{:}post + \gamma x_{it} + \theta z_{jt} + 1\alpha_t + \varepsilon_{ijt} \quad \textbf{(1)}$$

The DiD model for the bank risk-taking analysis is designed as follows:

$$y_{jt} = \beta_0 + \beta_1 \bullet treat + \beta_2 \bullet post + \beta_3 \bullet treat{:}post + \theta z_{jt} + 1\alpha_t + \varepsilon_{ijt} \quad \textbf{(2)}$$

The dependent variable is denoted as y_{ijt} for turnover and turnover poor performance and y_{jt} for the risk-taking variables. The independent variables are $treat$, $post$, and $treat{:}post$. The variable x_{it} represents the executive level controls and z_{jt} the bank level controls mentioned prior. Lastly, α_t represents year fixed-effects. The coefficient of interest then is β_3.

We utilised OLS regressions with year fixed-effects given that we wanted to focus on time-invariant bank variations. With a DiD research design, focusing on time-invariant within bank variations would have been preferable by adding bank fixed-effects, however, given our limited sample size, and limited number of banks, such a research design would be too exhaustive leading to overparametrization and reducing degrees of freedom to a significant extent. This then can cause estimation noise and instability. Hence, we resort to solely year fixed-effects.

To mitigate any concerns related to heteroskedasticity due to issues of serial correlation, we clustered standard errors on the bank level. Moreover, as a diagnostic test for multicollinearity, we conducted tests on the Variance Inflation Factor (VIF) and calculated values consistently below 5 for all regressions. We also constructed a Pearson's correlation matrix in order to further verify that there were no issues with multicollinearity (see Appendix B). Thus, we believe our results are not prone to issues of multicollinearity.

5. Results

In this section, we will present the empirical results of CRD IV bonus cap's regulatory effect on bank risk-taking and turnover, both in its introduction and subsequent abolishment. We draw upon equations (1) and (2) (outlined in section 4.4) in order to estimate the relationship between the regulatory changes and the respective outcome variables across the sample period. Alongside this we also present our analysis on the parallel trends assumption (see Appendix C).

5.1 Effect of Implementation of Variable Bonus Caps

5.1.1 Effects of Implementation on Turnover

The regression results presented in table 3 for the period around the implementation of the CRD IV bonus cap do not provide evidence of a statistically significant effect on executive turnover. The $treat$ coefficient explains the pre-CRD IV bonus cap implementation difference in turnover rates between the treatment and control groups, while the $treat{:}\,post$ (from now on interaction term) coefficient explains the difference-in-differences estimate, which is to what extent turnover rates changed in the UK after CRD IV relative to the US. The positive and significant coefficient on $treat$ indicates that, already prior to the CRD IV implementation, the UK executives exhibited a higher turnover rate than the control group of US executives. Further, we observe that across all the specifications, the coefficient on the interaction term exhibits statistical insignificance for both the turnover and turnover poor performance dependent variables. This thus provides no evidence that the introduction of CRD IV produced a differential effect on turnover in the UK compared with the control group. Moreover, we see no significant divergence of trend both in the pre- and post-treatment period with regards to the parallel trends assumption. Specifically, the average marginal effect (AME) is relatively flat in the pre-treatment and post-treatment period with the exception of an increase between 2014 and 2015. Hence, this supports the results around the implementation of the bonus cap not having any effect on turnover in our treatment sample.

Beyond the interaction term, the control variables reveal patterns that are worth taking note of. Age is positively and significantly associated with turnover in the standard all executive sample, and in both executive subsamples, with low coefficients of 0.007, 0.004 and 0.008 respectively. This is expected as age is a continuous variable with a wider scale of values. We can also observe

that the estimated effect is larger and more precisely estimated in the non-CEO subsample, suggesting that the turnover result is primarily driven by non-CEO executives. Given that the turnover dependent variables here are binary, these coefficients should be interpreted as increases in the turnover rate. Thus, all else being equal, a one-year increase in the executive's age is associated with a 0.7% increase in the probability of turnover in the all executives sample. However, interestingly, we observe that the age effect differs in the turnover poor performance regressions where it is positive and highly significant in the all-executive sample, insignificant among CEOs, and positive and significant among non-CEOs. This would suggest that age is only a relevant predictor of turnover in poorly performing banks when it comes to non-CEOs, and not so much for CEOs.

The CEO dummy exhibits a negative and strong statistically significant relationship with both of the dependent variables. Hence, CEOs exhibit a lower probability of turnover than the broader executives sample, and were to a lesser extent, forced to resign as a consequence of bad performance. Additionally, the length of service (Tenure) and gender (Female) are insignificant in explaining the variation in turnover. Finally, we see that the ROE variable is negative and statistically significant in the all-executive sample, and the non-CEO subsample. The reason why we observe a large coefficient on the ROE variable is due to ROE being expressed in absolute values rather than percentages. Hence, a one unit increase in ROE would entail a 100% improvement, which explains the large magnitude of the coefficient. Further, we can conclude that the reduced probability of turnover given improvement of profitability for executives, is driven by the non-CEO executives.

Table 3: Turnover Around the Implementation of the CRD IV Bonus Cap

	Turnover			Turnover Poor Performance		
	All executives (1)	CEOs only (2)	Non CEOs only (3)	All executives (4)	CEOs only (5)	Non CEOs only (6)
treat:post	0.013	0.018	0.013	0.012	0.060	-0.003
	(0.771)	(0.712)	(0.837)	(0.759)	(0.179)	(0.941)
treat	0.166***	0.152**	0.164***	0.094***	0.045	0.103***
	(0.000)	(0.014)	(0.000)	(0.003)	(0.161)	(0.006)
Age	0.007***	0.004*	0.008***	0.005***	0.001	0.006***
	(0.000)	(0.071)	(0.000)	(0.000)	(0.245)	(0.000)
CEO	-0.110***	–	–	-0.064***	–	–
	(0.000)			(0.000)		
Female	0.037	0.102	0.036	0.014	0.129	0.011
	(0.176)	(0.592)	(0.189)	(0.534)	(0.514)	(0.603)
ROE	-0.244***	-0.174	-0.253**	–	–	–
	(0.006)	(0.289)	(0.026)			
Tenure	-0.001	-0.000	-0.000	-0.001	0.000	-0.001
	(0.540)	(0.904)	(0.942)	(0.146)	(0.869)	(0.281)
Year Fixed Effects	X	X	X	X	X	X
N	2511	520	1991	2511	520	1991
R^2	0.038	0.034	0.029	0.020	0.014	0.015

This table shows the coefficients from our DiD analysis of turnover around the implementation of the bonus cap. The estimations are based on OLS regressions with year fixed-effects. The p-values are calculated by clustering standard errors on the bank level and are denoted in parentheses. We have included both the turnover (see regressions 1-3) and turnover poor performance (see regressions 4-6) dependent variables, and the executive level and bank level controls (see section 4.1 for variable definitions). We have also conducted regressions for CEO only and non-CEO only executives. From the CEO regressions, we have omitted the CEO dummy variable given that it is a constant 1 or 0 in such regressions and from the turnover poor performance regressions, we have omitted the ROE control variable given that turnover poor performance is constructed from banks' ROE measures. Significance in the 1%, 5% and 10% levels are indicated with ***, **, * respectively.

5.1.2 Effects of Implementation on Bank Risk-Taking

In table 4, we observe the regression results examining the impact of CRD IV bonus cap on a number of variables associated with bank risk-taking. The interaction term presents a nuanced picture. First, we find a statistically significant negative effect on market beta, indicating that on average, UK banks exhibit a moderate reduction in shareholder's risk exposure during the post-regulation period. We also observe the *treat* variable to be positive and significant, entailing that prior to the introduction, on average, the treated banks exhibited higher market betas than the control group. Similarly, we find that the interaction term shows a highly significant and negative coefficient for the Sharpe ratio, suggesting that in conjunction with decreased shareholder risk exposure, the risk-adjusted returns for the institutions saw a significant decline after the regulation was introduced. The *treat* estimate however shows a positive and significant value, indicating that prior to the implementation of the bonus cap, on average, the UK banks had better risk-adjusted returns than the US banks. In contrast, the interaction variable for CDS

spreads and SRISK%, representing credit risk and systemic risk respectively, are statistically insignificant, indicating that the introduction of the bonus caps didn't lead to changes in perceived default risk or capital shortfall risk contributions within our model. In terms of the parallel trends assumption, we see no significant divergence of trend in the pre- and post-treatment period for CDS spreads and SRISK%. However, we do see a relatively flat trend in the pre-treatment period for the market betas, indicating that the parallel trends assumption indeed holds true. With regards to the Sharpe ratio, however, we observe an interesting image with a diverging, upward trend in the pre-treatment period that reverses post-treatment. Thus, although there are indications of violation of the parallel trends assumption, we still observe that the treatment may have had an effect in reversing the trend that existed prior.

With regards to ROE, we observe that this is a highly significant control variable across all regressions with the exception of CDS spreads. Notably, an increase in profitability is associated with a decrease in market beta, while it shows the contrary with regards to the Sharpe ratio.

Table 4: Risk taking Around the Implementation of the CRD IV Bonus Cap

	CDS Spreads (1)	Market Beta (2)	Sharpe Ratio (3)	SRISK% (4)
treat:post	-0.337	-0.253*	-0.206***	0.002
	(0.165)	(0.087)	(0.000)	(0.869)
treat	-0.035	0.361*	0.135***	0.087**
	(0.885)	(0.057)	(0.006)	(0.042)
ROE	-1.035	-1.994***	0.550***	-0.250**
	(0.331)	(0.000)	(0.003)	(0.013)
Year Fixed Effects	X	X	X	X
N	117	388	388	356
R^2	-0.035	0.149	0.005	0.289

This table shows the coefficients from our DiD analysis of risk-taking around the implementation of the bonus cap. The estimations are based on OLS regressions with year fixed-effects. The p-values are calculated by clustering standard errors on the bank level and are denoted in parentheses. We have included all 4 risk-taking dependent variables, namely, log of 5-year excess CDS spreads (see regression 1), market beta (see regression 2), Sharpe ratio (see regression 3) and expected capital shortfall (see regression 4). We have also included our bank level control variable, ROE (see section 4.1 for variable definitions). Significance in the 1%, 5% and 10% levels are indicated with ***, **, * respectively.

5.2 Effect of Abolishment of Variable Bonus Caps

5.2.1 Effect of Abolishment on Turnover

In table 5 we present the results surrounding the abolishment of the bonus cap. We observe that the $treat$ estimates across the majority of the regressions are positive and significant, pointing towards UK banks having higher turnover rates prior to the abolishment of the bonus cap. However, we also observe a highly significant negative relationship in the interaction term across all six specifications. In the standard turnover regressions, the estimated coefficients range from -0.433 to -0.493, indicating a substantial reduction in the turnover rate relative to the control group in the post-abolishment period. Comparatively, the turnover poor performance results are slightly lower in magnitude, but indicate that forced turnover at poorly performing banks also decreased following the abolishment of the bonus cap. Further, given that the estimations show statistical significance across all executive, CEO-only and non-CEO only subsamples, it indicates that the relationship is not driven by a single category of executives, but seems consistent across the different samples. In terms of the parallel trends assumption, although there is a relatively diverging downward trend pre-treatment, we observe a sharp fall in the average marginal effect (AME) in the first year of treatment. Thus, we do see some effects of the treatment effect even if there are indications of the assumption being violated.

With regards to the control variables, we see that age remains to be positive and statistically significant, with smaller magnitudes of effect on the rate of turnover. Thus, during the sample period, older executives were associated with higher probability of turnover. Similar to the introduction of the bonus cap, the CEO dummy is negative and statistically significant in the all executive regressions, implying that being a CEO meant a lower probability of turnover than the broader executive sample. However, contrary to the results in table 3, tenure exhibits a negative and statistically significant relationship with turnover in several of the specifications. Although with a low magnitude (due to tenure having a wider scale of values it could assume), a one-year increase in tenure corresponded to a decrease of 0.003 in the turnover rate for executives in our model, with the effect being driven by non-CEO executives. Finally, the ROE variable is negative and statistically significant across all specifications in the standard turnover regressions (1-3). A one unit increase in ROE, corresponding to a 100 percentage point increase in profitability, decreases the turnover rate by approximately 0.46 to 0.51.

Table 5: Turnover Around the Abolishment of the CRD IV Bonus Cap

	Turnover			Turnover Poor Performance		
	All executives (1)	CEO only (2)	Non CEOs only (3)	All executives (4)	CEO only (5)	Non CEO only (6)
treat:post	-0.433***	-0.493***	-0.454***	-0.306***	-0.277***	-0.354***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
treat	0.086***	0.162***	0.071*	0.064**	0.016	0.089**
	(0.006)	(0.000)	(0.083)	(0.015)	(0.590)	(0.011)
Age	0.007***	0.010***	0.006***	0.005***	0.001	0.005***
	(0.000)	(0.002)	(0.001)	(0.000)	(0.606)	(0.000)
CEO	-0.098***	–	–	-0.039***	–	–
	(0.000)			(0.003)		
Female	0.011	0.032	0.009	0.023	-0.005	0.026
	(0.692)	(0.673)	(0.752)	(0.330)	(0.935)	(0.274)
ROE	-0.501***	-0.458**	-0.507***	–	–	–
	(0.001)	(0.030)	(0.005)			
Tenure	-0.003**	0.001	-0.003**	-0.003***	-0.004**	-0.003**
	(0.031)	(0.519)	(0.049)	(0.005)	(0.026)	(0.021)
Year fixed effects	X	X	X	X	X	X
N	1784	421	1363	1784	421	1363
R^2	0.070	0.118	0.045	0.038	0.041	0.031

This table shows the coefficients from our DiD analysis of turnover around the abolishment of the bonus cap. The estimations are based on OLS regressions with year fixed-effects. The p-values are calculated by clustering standard errors on the bank level and are denoted in parentheses. We have included both the turnover (see regressions 1-3) and turnover poor performance (see regressions 4-6) dependent variables, and the executive level and bank level controls (see section 4.1 for variable definitions). We have also conducted regressions for CEO only and non-CEO only executives. From the CEO regressions, we have omitted the CEO dummy variable given that it is a constant 1 or 0 in such regressions and from the turnover poor performance regressions, we have omitted the ROE control variable given that turnover poor performance is constructed from banks' ROE measures. Significance in the 1%, 5% and 10% levels are indicated with ***, **, * respectively.

5.2.2 Effect of Abolishment on Bank Risk-Taking

In table 5 we present the results surrounding the abolishment of the bonus cap on variables associated with bank risk-taking. The interaction term, $treat{:}\,post$, once again presents a nuanced picture. With regards to market beta and Sharpe ratio, we observe a positive and statistically significant effect on UK banks post-abolishment of the bonus cap. This entails that, on average, UK banks became more systematically exposed to market risk and simultaneously started to show better risk-adjusted returns. We also observe the $treat$ variable to be positive and significant, entailing that prior to the introduction, on average, the treated banks exhibited higher market betas than the control group. With regard to CDS spreads and SRISK%, the $treat$ estimate indicates that, prior to the abolishment of the bonus cap, perceived credit risk was lower in the UK than in the US, as reflected by the negative and statistically significant coefficient for CDS spreads. At the same time, the positive and statistically significant $treat$ coefficient for SRISK% suggests that systemic risk, measured through expected capital shortfall, was higher in

the UK relative to the US prior to the abolishment. In terms of the parallel trends, similar to the implementation period, we see no significant divergence of trend in the pre- or post-treatment period with regards to CDS spreads and SRISK%. With regards to the market betas, we observe a diverging upward trend both in the pre-treatment period continuing into the post-treatment period. This could entail that the significance in the baseline regressions is not as a result of the abolishment of the bonus cap, but rather a continuation of some external trend. The significance of the beta interaction coefficient is only valid to the extent that the parallel trends assumption is not violated, however, in this case we do observe indications of violation. With regards to the Sharpe ratio, similar to the implementation period, we see a diverging downward trend pre-treatment that reverses post-treatment, thus, although indicating a violation of the assumption, we still observe that the treatment may have had a significant effect on Sharpe ratios in the treatment group.

Table 6: Risk taking Around the Abolishment of the CRD IV Bonus Cap

	CDS Spreads (1)	Market Beta (2)	Sharpe Ratio (3)	SRISK% (4)
treat:post	0.185	0.269***	0.416***	0.003
	(0.305)	(0.000)	(0.000)	(0.869)
treat	-0.535**	0.507***	-0.015	0.103**
	(0.018)	(0.000)	(0.781)	(0.012)
ROE	-1.703	0.165	0.548***	-0.091
	(0.605)	(0.636)	(0.001)	(0.250)
Year fixed effects	X	X	X	X
N	110	399	399	315
R^2	0.016	0.396	0.173	0.331

This table shows the coefficients from our DiD analysis of risk-taking around the abolishment of the bonus cap. The estimations are based on OLS regressions with year fixed-effects. The p-values are calculated by clustering standard errors on the bank level and are denoted in parentheses. We have included all 4 risk-taking dependent variables, namely, log of 5-year excess CDS spreads (see regression 1), market beta (see regression 2), Sharpe ratio (see regression 3) and expected capital shortfall (see regression 4). We have also included our bank level control variable, ROE (see section 4.1 for variable definitions). Significance in the 1%, 5% and 10% levels are indicated with ***, **, * respectively.

5.3 Robustness

5.3.1 Logit Regressions

Given that our turnover variable is binary, we saw it fit to implement a logit regression in order to find the effect of the implementation and abolishment of the bonus cap on the log-odds probability of turnover for our treatment sample. Due to the issue of separation as discussed in section 4.4, we omit any use of fixed-effects. Our research design is as follows:

$$ln(\frac{P}{1-P}) = \beta_0 + \beta_1 \bullet treat + \beta_2 \bullet post + \beta_3 \bullet treat \bullet post + \gamma x_{it} + \theta z_{jt} \quad \textbf{(3)}$$

Where P is the probability of turnover, or turnover poor performance, and $ln(\frac{P}{1-P})$, the log-odds of P. Our coefficient of interest then is β_3 which represents the increase/decrease in the log-odds probability of turnover for the treatment sample in the post-period. The results of the logit regressions (see Appendix D) are aligned, to a large extent, with that of the baseline regressions with no significance on the interaction term in the implementation period, for both the turnover and turnover poor performance dependent variables, and the all executive, CEO and non-CEO subsamples. Similarly, for the abolishment period, we see negative and significant coefficients on the interaction term for all the different regressions. One exception, however, is with regards to the CEO only regression where we use turnover poor performance as the dependent variable in the implementation period. In this case, the interaction term coefficient is positive and significant at a 10% level. With regards to this specification, we observe no significance in the baseline regressions.

5.3.2 Model Specification With Bank Fixed-Effects and No Controls

There is a concern with omitted variable bias in performing regressions with solely year fixed-effects. We also address concerns with regards to limited degrees of freedom with both bank and year fixed-effects within section 4.4 on the research design. Nonetheless, we perform tests with both bank and year fixed-effects and, to a large extent, observe alignment with baseline regressions with regards to the level of significance and sign of the interaction term coefficients. One exception is with regards to the CEO only turnover poor performance regressions in the implementation period. We observe that the interaction term coefficient is positive and

significant at the 5% level. This is aligned with that of the logit regression too and this significance is simply not captured by our main research design with solely year fixed-effects. Hence, there is some specification sensitivity and this is expected with small samples, fixed-effects and a substantial number of controls. It is also important to note the alignment of results in all other specifications and we believe that this sole exception does not invalidate our baseline results. Nonetheless, we address this as a limitation and take caution in making substantive conclusions from our empirics.

Colonello et al. (2023) disregard any controls in their bank-level risk-taking regressions due to issues of endogeneity with bank return and risk. We include ROE as our sole bank-level control variable given that it significantly improves our explanatory power, however, we recognise this issue and thus also perform regressions with no controls. Nonetheless, we find that the results align with that of the baseline regressions. Specifically, we observe no significance for the interaction term coefficients for CDS spreads and SRISK% in both the implementation and abolishment period, negatively significant coefficients for market beta and Sharpe ratio in the implementation period, and positively significant coefficients for market beta and Sharpe ratio in the abolishment period.

5.3.3 Anticipation Tests

Policy changes are often preceded by public consultations, political negotiations and longer periods of drafting, allowing institutions to potentially respond ahead of the formal policy date. The main purpose behind the anticipation tests is thus to examine if banks and executives had begun adjusting their behaviour prior to the formal implementation and abolishment of the bonus cap regulation. To investigate these potential anticipatory effects, we conduct a test where the timing of the policy is shifted one year forward. We draw upon equations (1) and (2) for turnover and bank risk-taking respectively, which are estimated using the same specifications, while redefining the post variable such that it takes the value of 1 from 2013 in the implementation period and from 2023 in the abolishment period, as opposed to 2014 and 2024. By doing this, we can examine if, and how, the effects differ from our baseline regressions.

When examining Appendix E, we observe that with regard to the introduction of the bonus cap, the turnover regressions yield results that are largely consistent with the baseline regressions. The interaction term remains statistically insignificant, while all the estimates for the treatment variable have approximately the same magnitude, direction, and statistical significance across the regressions. This suggests that shifting the implementation date one year forward does not materially alter the turnover-related findings. For the bank risk-taking estimations, however, the results exhibit a larger degree of sensitivity to the anticipation tests. Specifically, the interaction term for market beta loses statistical significance and more notably, the previously significant Sharpe ratio, now exhibits insignificance for both the interaction term and the treatment variable. This indicates that the effect on the bank's risk-adjusted returns does not occur prior to the implementation of the bonus cap, but rather suggests that it is strongly connected to the actual policy implementation date.

In the abolishment period, the turnover regressions show similar directions and level of significance across all of the specifications. The difference, however, is that the interaction term drops substantially in magnitude. Across all specifications, the magnitude declines by around 35 to 45 per cent, which is quite a noteworthy reduction. If anticipatory effects had been present, one would have expected the coefficient magnitude to have increased by switching to 2023 as the implementation date in our model. However, as the results show the contrary, it would suggest that there is no evidence of an anticipatory effect on turnover, and rather strengthens the conclusion that effects started appearing starting 2024. With regards to bank risk-taking, changing the abolishment date does not appear to have any larger effects on significance levels or directions of the estimates. The main variation stems from the interaction terms' magnitude, where it increased for market beta and declined for the Sharpe ratio. Overall, the findings do not point towards any larger anticipatory effects related to policy timing. In conclusion, given the strong alignment with one exception, we believe the results presented in section 5.1 and 5.2 are robust across several specifications.

6. Discussion

This study is motivated by the concern that the bonus cap, imposed in the UK financial sector, may have altered the incentives of bank executives in manners which affected both turnover and risk-taking. Specifically, the regulation was expected to influence how compensation is structured, and, as a consequence, whether executives remain in the industry and how banks adjust their risks. With regards to this background, the empirical results of this study provide a basis for examining whether the introduction, and subsequent abolishment, of the bonus cap was associated with changes in executive behaviour and bank outcomes.

Overall, the findings of this thesis provide varied support for the hypotheses formulated in this study. With regards to the introduction of the bonus cap, the results do not provide evidence to reject the null hypothesis, entailing that the regulation had no statistically significant effect on executive turnover in the UK. Although compensation structures indeed changed following the implementation of the bonus cap, these adjustments have not resulted in noteworthy changes in turnover rates. For bank risk-taking, the evidence is more nuanced. The implementation of the bonus cap looks to have decreased risk-taking, however only with regards to shareholder risk and shareholder performance, as measured by market beta and the Sharpe ratio respectively. In that sense, the second research question's alternative hypothesis is partially supported, as the effect is not observed across all examined measures of risk.

The post-Brexit abolishment period presents a different pattern. With regards to executive turnover, the results show support for the alternative hypothesis and suggest that the abolishment of the bonus cap was associated with lower executive turnover. This is directly in contrast to the introductory period and indicates that the removal of the bonus cap had a stronger effect on retention than its implementation. With regards to bank risk-taking, the results again provide partial support for the alternative hypothesis. The abolishment of the bonus cap has increased risk-taking, but only with respect to shareholder risk and performance.

In terms of literature, our results exhibit both alignment and divergence to the empirical results or theoretical findings of previous studies. One study directly examining the effects of the implementation of the bonus cap in the EU context is Colonello et al. (2023). They find that

following the implementation of the bonus cap, the turnover rate increased significantly, driven mainly by CEOs, for the executives whose maximum variable-to-fixed compensation ratio fell below 200%. We find evidence that challenges this as we observe no significant changes in turnover rates for UK executives relative to the US environment. This discrepancy may be a product of inherent differences between the UK specifically and the EU environment as a whole, however, at the same time Colonello et al. (2023)'s treatment group is made up of a majority of large UK banks. Moreover, when Colonnello et al. (2023) conduct robustness tests using the US control group, thus somewhat matching our own research design, they find similar results to their own baseline regressions.

The difference between the findings may be a product of several factors. Firstly, Colonnello et al. (2023) utilize a treatment intensity variable rather than a binary treatment indicator in their DiD regressions. This methodology enhances the nuance in the analysis, as the treatment group will be limited to executives whose maximum variable compensation to fixed compensation ratio fell from above 200% to below 200%, thus establishing a more direct and theoretically sound connection between compensation and turnover. Secondly, Colonnello et al. (2023) examine 45 of the largest EU banks in their sample, whereas our treatment sample contains a mix of banks in different sizes. It is commonly known through prior research that variable compensation is more prevalent in larger firms (see e.g. Jensen and Murphy, 1990). As a result, the bonus cap may have had more of an impact on compensation packages in the larger banks included in Colonnello et al. (2023)'s sample than in the smaller banks in our sample. Moreover, prior research indicates an inverse relationship between variable compensation and turnover (see e.g. Murphy, 2013b), which may potentially explain the differences in results as variable compensation was likely more prevalent in larger banks before the implementation of the bonus cap.

Our results are, to a large extent, also at odds with that of Kleymenova and Tuna (2021). Their study finds that the 2010 UK Remuneration Code, although fixated on aspects of compensation regulation other than bonus caps, was associated with an increase in turnover among UK banks. In contrast, our results suggest that the bonus cap did not produce a comparable increase in UK executive turnover. In other words, these findings suggest that compensation regulation in the UK has not impacted executive retention in a uniform manner. Instead, the effect appears to

depend on the specific design and restrictiveness of the regulatory policy, where some potentially contribute to increased flight from the industry while others do not.

During the abolishment period, we observe a significant decline in turnover within the UK banking sector relative to the US. This is aligned with literature, such as the findings of Philippon and Reshef (2012). Whereas Philippon and Reshef (2012) study the effects of deregulation in general, the abolishment of the bonus cap can be viewed as a major deregulatory push that resulted in similar "brain gain" effects. At the same time, our findings differ from Philippon and Reshef (2012) in the sense that we do not find evidence of a "brain drain" following the introduction of the bonus cap.

With regards to the results on risk-taking, our findings do not align with those of Colonnello et al. (2023). Whereas Colonello et al. (2023) find a significant and positive impact of the bonus cap on CDS spreads, SRISK% and market betas, we find no significance with regards to CDS spreads and SRISK% and significance with a negative impact on market betas. Moreover, our results with regards to shareholder risk and performance are also at odds with the findings of Abudy et al. (2020). Whereas Abudy et al. (2020) do not investigate the effects of an abolishment of a bonus cap, they do find that the Israeli banking sector was characterised more by a rent-extraction environment than by a value-maximising one. In contrast, we find evidence of an increase in shareholder risk post abolishment of the bonus cap, alongside an increase in shareholder value, which is more aligned with a value-maximisation framework.

Regarding the effects on systemic and credit risk, we observe a range of recent literature advocating against shareholder-aligned incentive structures, arguing that these arrangements may encourage excessive risk-taking through risk-sharing which would result in the shareholder risk to be borne by debtholders and society (Efing et al., 2022; Thanassoulis, 2012; Murphy, 2013b; Thanassoulis and Tanaka, 2018). At the same time, however, there exists a range of older post-financial crisis literature that advocates for shareholder aligned incentive structures, claiming that it would closely tie managerial incentives to long-term firm value (Bhagat and Romano (2009); Bebchuk and Fried (2009)). Unlike any of these findings and views, our results

from the UK landscape indicate that the implementation and abolishment of the bonus cap carries no effects on risks across these two dimensions.

There is a lot of room for discussion on why our results differ substantially from previous literature when it comes to risk-taking. One possibility is the differences in the risk landscape between the UK environment and the environment(s) explored in other earlier studies. Another possibility is simply the existence of several confounding factors that distort the relationship between executive compensation and risk-taking. For example, alongside the introduction of the bonus cap, CRD IV carried with it several other implications specifically with regards to capital requirements. Moreover, during the abolishment period, there may have been some "spillover effects" from Brexit that affected risk-taking separately from that of the bonus cap. The parallel trends for market betas, for example, shows an increasingly diverging and upward going trend from 2021 to 2025, relative to the US banking sector, likely as a result of the Brexit movement rather than the result of the abolishment of the bonus cap itself.

In Colonello et al. (2023)'s research design, the analysis explores the full EU landscape and therefore utilizes country-by-year fixed-effects to account for all country-specific variation as a result of, for example, national legislation. Such a research design, for our research purposes, is not possible given that we only use the UK and the US in our analysis.

In addition to the differences of research design, existence of confounding factors and indications of violation with regards to the parallel trends assumption, there exists other limitations in our empirical methodology. Firstly, our sample size is considerably smaller than that which is used in extant literature, especially when it comes to our risk-taking analyses, thus making our estimates being based on relatively limited degrees of freedom. Alongside this, we utilise year fixed-effects which further reduce our degrees of freedom. There are also concerns with regards to omitted variable bias, given that bank fixed-effects are not included in baseline regressions and the fact that we utilise limited controls. We partially address this concern with robustness tests (see section 5.3). In addition, we also experience issues of low explanatory power in our baseline regressions, with some adjusted R^2s even being negative. We attribute this issue primarily to the small sample size rather than to overfitting, as we find that each control variable included in our

baseline regressions improved explanatory power without significantly increasing the standard error of our interaction term coefficient. Given these limitations, we refrain from making causal conclusions and instead interpret the results as evidence of associations between the implementation and abolishment of the bonus cap, executive turnover and bank risk-taking.

7. Conclusion

This thesis contributes to extant literature by examining the bonus cap as a policy instrument within the UK's unique regulatory setting, capturing both the implementation and abolishment of the bonus cap, the latter of which has not been captured in any empirical paper as of yet. With this, the thesis adds to already pre-existing discussions on the relationship between executive compensation, turnover and risk-taking within the banking sector.

The practical implications of the findings suggest bonus caps as a policy tool are ineffective. Overall, the results indicate that there is limited evidence for the implementation and abolishment of the bonus cap affecting or being associated with credit or systemic risk. Whereas the bonus cap shows some evidence of curbing shareholder risk in the implementation period, it is also seen to be paired with lower shareholder performance with the contrary observable following abolishment. Moreover, following the abolishment of the bonus cap, we observe some evidence of higher executive retention. However, we take a cautious stance due to limitations in our study, and encourage the reader to interpret the conclusions as associations rather than causations.

Several aspects of this thesis also point to prospects for future research. Colonello et al. (2023) notes that "systemic risk remains an elusive theoretical concept" and that "[they] remain agnostic and use several proxies". In this study, a single proxy for systemic risk has been utilized in order to examine the effects of the bonus cap. Thus, future research may improve insights on the societal impact and risk of such regulation by examining a wider set of measures and arrive at a more comprehensive understanding of the effect of the bonus cap. Moreover, we find interesting results with respect to turnover, as the implementation period shows no effect, whereas the abolishment period is associated with a negative effect. Hence, conducting country-specific analyses that specifically examine whether turnover had decreased in the UK, or perhaps increased in the US, may also be beneficial. Further, using several alternate control groups akin to Kleymenova and Tuna (2021) may, again, improve insights into these initial results. Since the abolishment of the bonus cap was a relatively recent regulatory event, we also see scope in replicating this research design in the future, as it would allow for a longer post-treatment period and a richer dataset.

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9. Appendix

9.1 Appendix A: Variable Definitions Table

Variable Definitions

Variables	Databases	Definition
Turnover	Execucomp, Manually collected	Dummy variable that equals 1 if an executive leaves the board and 0 otherwise. For Execucomp, Executive turnover is set to 1 in the last year an executive is reported and 0 otherwise
Turnover (poor performance)	Manually collected, Capital IQ, LSEG Datastream	Dummy variable equal to Turnover if the ROE of the respective bank is below the median of banks sampled in the previous year and 0 otherwise.
CEO	Manually collected, Execucomp	Dummy variable that equals 1 if an executive is the CEO of the bank and 0 otherwise.
Age	Manually collected, Execucomp	The age of the executive measured in years.
Tenure	Manually collected, Execucomp	For the UK, the number of years an executive director has been appointed to the board (measured in years), and for the US, the number of years an executive has served as an executive manager (measured in years).
Female	Manually collected, Execucomp	Dummy variable that equals 1 if an executive is female and 0 otherwise.
Treat	Manually collected	Variable that equals 0 for executives in the control group and 1 for treatment group.
Post	Manually collected	Dummy variable that equals 1 from 2014 (2024) and onward and 0 otherwise.
ROE	Capital IQ, LSEG Datastream	Return on Equity.
Sharpe ratio	LSEG Datastream, WRDS Betasuite	Ratio of stock excess return over stock volatility.
Log 5-year excess CDS spread	Capital IQ, LSEG Datastream	Log of 5-year CDS excess spread. The excess spread is the difference of the CDS spread of the bank and the CDS spread of the corresponding sovereign (average over the last quarter of the year).
SRISK%	NYU V-Lab	Fraction of the entire financial sector's capital shortfall that the bank would incur in the event of a crisis.
Market Beta	LSEG Datastream, WRDS Betasuite	Market beta of the bank.

9.2 Appendix B: Correlation Matrices

Correlation Matrices

Panel A: Turnover During the Implementation of the Bonus Cap

	treat	CEO	Age	Female	roe	tenure	treat:post
treat	1.00	0.12	-0.66	-0.07	-0.32	-0.24	0.69
CEO	0.12	1.00	0.08	-0.12	-0.05	0.23	0.11
Age	-0.66	0.08	1.00	-0.05	0.19	0.41	-0.40
Female	-0.07	-0.12	-0.05	1.00	0.06	-0.05	-0.03
roe	-0.32	-0.05	0.19	0.06	1.00	0.15	-0.10
tenure	-0.24	0.23	0.41	-0.05	0.15	1.00	-0.15
treat:post	0.69	0.11	-0.40	-0.03	-0.10	-0.15	1.00

Panel B: Turnover During the Abolishment of the Bonus Cap

	treat	CEO	Age	Female	roe	tenure	treat:post
treat	1.00	0.26	-0.25	0.05	-0.15	-0.30	0.57
CEO	0.26	1.00	0.13	-0.15	-0.05	0.16	0.16
Age	-0.25	0.13	1.00	-0.20	0.05	0.50	-0.08
Female	0.05	-0.15	-0.20	1.00	0.00	-0.14	0.05
roe	-0.15	-0.05	0.05	0.00	1.00	0.13	-0.08
tenure	-0.30	0.16	0.50	-0.14	0.13	1.00	-0.16
treat:post	0.57	0.16	-0.08	0.05	-0.08	-0.16	1.00

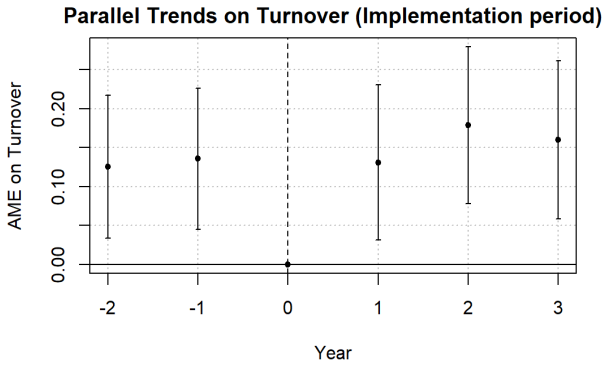
Panel C: Risk-Taking During the Implementation of the Bonus Cap

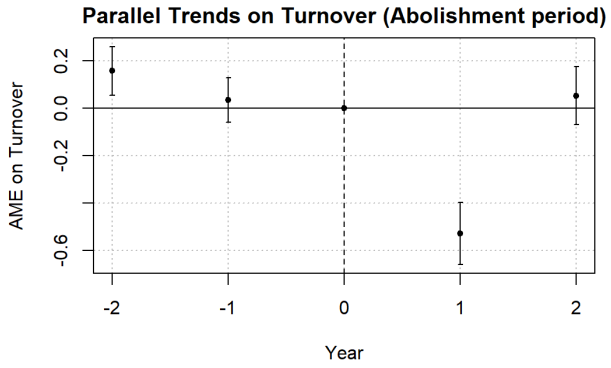
	treat	roe	treat:post
treat	1.00	-0.13	0.71
roe	-0.13	1.00	-0.02
treat:post	0.71	-0.02	1.00

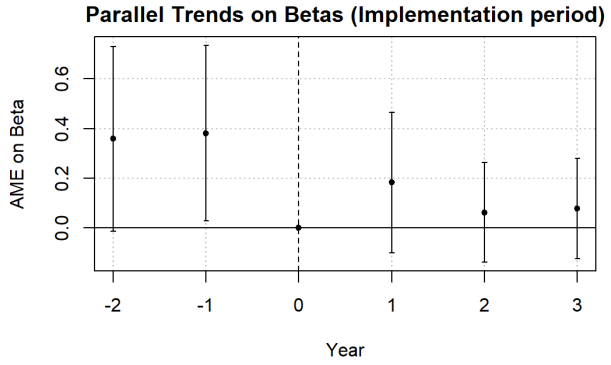
Panel D: Risk-Taking During the Abolishment of the Bonus Cap

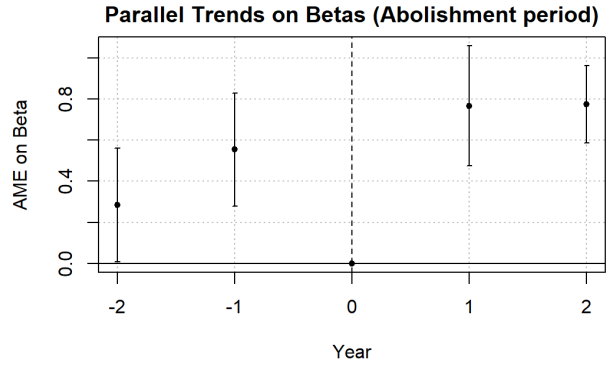
	treat	roe	treat:post
treat	1.00	-0.16	0.63
roe	-0.16	1.00	-0.11
treat:post	0.63	-0.11	1.00

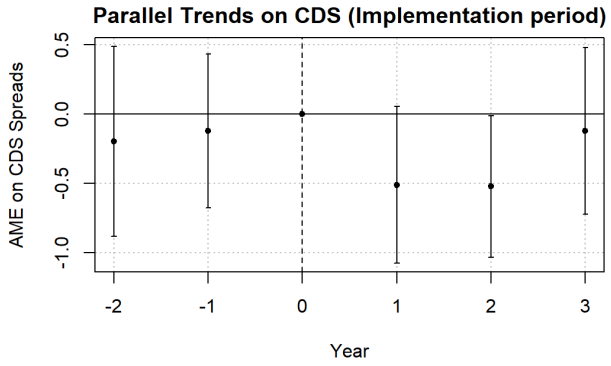
9.3 Appendix C: Parallel Trends Assumption

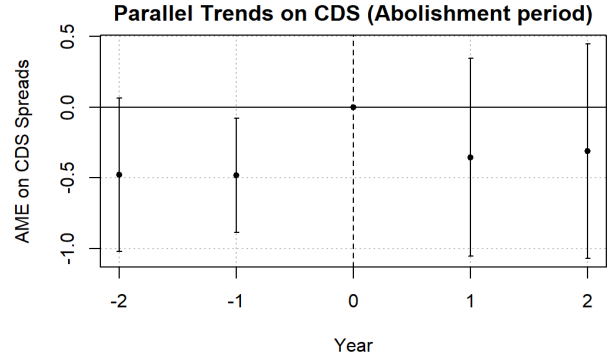


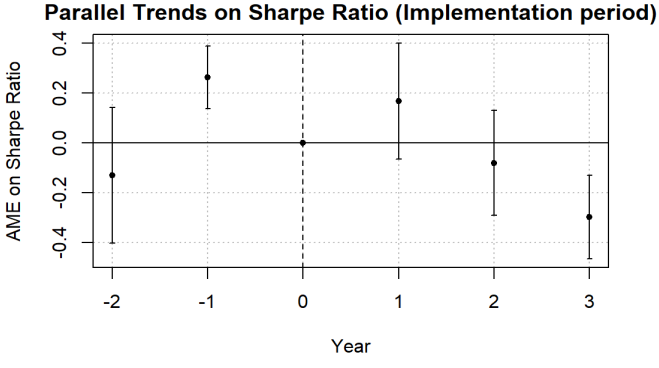


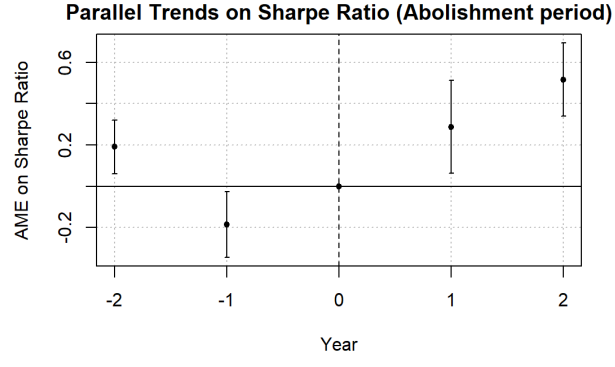




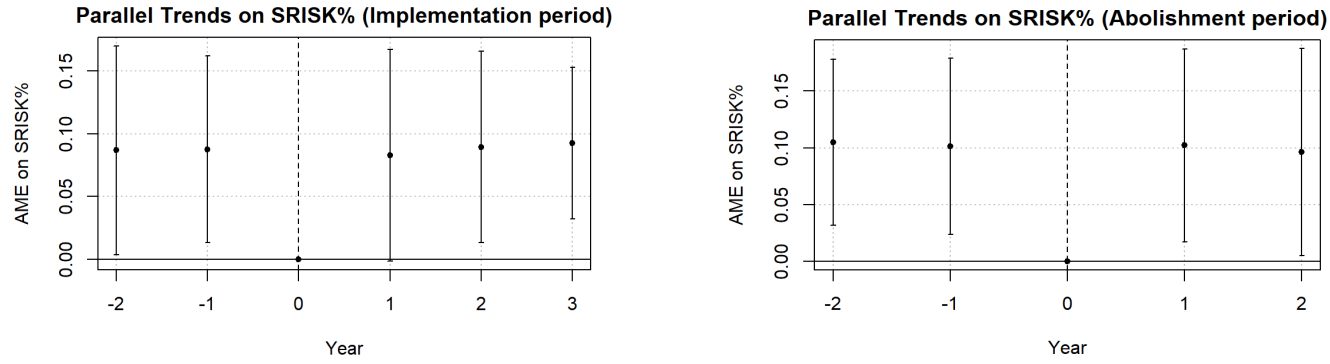








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The above figure shows our graphs to examine our parallel trends assumption. Like Colonello et al. (2023), we plot the average marginal effects (AME) of the implementation and abolishment of the bonus cap on our dependent variables.

We omit a baseline year (2013 for the implementation period and 2023 for the abolishment period, or year = 0) in our calculations, and add an indicator variable for every year equal to 1 for that specific year. Then we regress the dependent variable against the indicator variables, each also multiplied by treat, the control variables and year fixed-effects. We then plot the year-specific coefficients of the indicator variable * treat. Although we use the data from the baseline years in these regressions, we add no indicator on the right-hand side to account for it, in order to avoid perfect multicollinearity. Thus the data for the omitted baseline years are captured within the intercept and coefficients for the control variables and year fixed-effects. Hence, each AME value is the average difference between the treatment and control group for that specific year relative to the baseline year.

In order to fulfill the parallel trends assumption, we would ideally see the error bars in the pre-treatment period intercept 0 and diverge from 0 in the post-treatment period. Or we would hope to see a flat trend in the pre-period with a clear divergence in the post-period.

9.4 Appendix D: Logit Regressions

Logit Regression for Turnover Around the Implementation of the Bonus Cap

	Turnover			Turnover Poor Performance		
	All executives (1)	CEOs only (2)	Non CEOs only (3)	All executives (4)	CEOs only (5)	Non CEOs only (6)
Intercept	-6.454***	-7.977***	-6.515***	-7.723***	-7.993**	-7.875***
	(0.000)	(0.001)	(0.000)	(0.000)	(0.020)	(0.000)
treat:post	0.058	0.345	0.022	0.136	2.290*	-0.090
	(0.840)	(0.659)	(0.946)	(0.715)	(0.085)	(0.827)
treat	1.526***	2.613***	1.383***	1.482***	1.848	1.477***
	(0.000)	(0.002)	(0.000)	(0.000)	(0.126)	(0.000)
post	0.208	-0.286	0.248*	0.230	-1.597	0.317*
	(0.149)	(0.636)	(0.098)	(0.215)	(0.151)	(0.098)
Age	0.069***	0.066**	0.069***	0.076***	0.055	0.078***
	(0.000)	(0.044)	(0.000)	(0.000)	(0.248)	(0.000)
CEO	-1.162***	–	–	-1.227***	–	–
	(0.000)			(0.000)		
Female	0.358	1.756	0.323	0.248	2.783**	0.184
	(0.101)	(0.140)	(0.146)	(0.390)	(0.037)	(0.536)
ROE	-1.804**	-2.003	-1.804**	–	–	–
	(0.012)	(0.321)	(0.019)			
Tenure	0.006	0.007	-0.000	-0.018	0.019	-0.013
	(0.548)	(0.828)	(0.988)	(0.179)	(0.678)	(0.350)
N	2511	520	1991	2511	520	1991

This table shows the coefficients from our DiD analysis of turnover around the implementation of the bonus cap. The estimations are based on logit regressions with no fixed-effects. The p-values are denoted in parentheses. We have included both the turnover (see regressions 1-3) and turnover poor performance (see regressions 4-6) dependent variables, and the executive level and bank level controls (see section 4.1 for variable definitions). We have also conducted regressions for CEO only and non-CEO only executives. From the CEO regressions, we have omitted the CEO dummy variable given that it is a constant 1 or 0 in such regressions and from the turnover poor performance regressions, we have omitted the ROE control variable given that turnover poor performance is constructed from banks' ROE measures. Significance in the 1%, 5% and 10% levels are indicated with ***, **, * respectively.

Logit Regression for Turnover Around the Abolishment of the Bonus Cap

	Turnover			Turnover Poor Performance		
	All executives (1)	CEOs only (2)	Non CEOs only (3)	All executives (4)	CEOs only (5)	Non CEOs only (6)
Intercept	-4.187***	-9.931***	-3.592***	-5.174***	-4.848**	-5.301***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.013)	(0.000)
treat:post	-2.558***	-4.167***	-2.564***	-2.553***	-3.181***	-2.880***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
treat	0.654***	2.460***	0.468*	0.864***	1.086	0.982***
	(0.003)	(0.000)	(0.064)	(0.002)	(0.149)	(0.001)
post	2.392***	4.223***	2.168***	2.426***	3.720***	2.279***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Age	0.049***	0.107***	0.041***	0.045***	0.024	0.048***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.435)	(0.000)
CEO	-0.712***	–	–	-0.412**	–	–
	(0.000)			(0.040)		
Female	0.064	0.459	0.053	0.206	-0.103	0.240
	(0.719)	(0.532)	(0.772)	(0.320)	(0.903)	(0.259)
ROE	-3.481***	-4.317*	-3.411***	–	–	–
	(0.000)	(0.061)	(0.003)			
Tenure	-0.017*	-0.008	-0.017	-0.031***	-0.061**	-0.028**
	(0.074)	(0.758)	(0.110)	(0.006)	(0.044)	(0.027)
N	1784	421	1363	1784	421	1363

This table shows the coefficients from our DiD analysis of turnover around the abolishment of the bonus cap. The estimations are based on logit regressions with no fixed-effects. The p-values are denoted in parentheses. We have included both the turnover (see regressions 1-3) and turnover poor performance (see regressions 4-6) dependent variables, and the executive level and bank level controls (see section 4.1 for variable definitions). We have also conducted regressions for CEO only and non-CEO only executives. From the CEO regressions, we have omitted the CEO dummy variable given that it is a constant 1 or 0 in such regressions and from the turnover poor performance regressions, we have omitted the ROE control variable given that turnover poor performance is constructed from banks' ROE measures. Significance in the 1%, 5% and 10% levels are indicated with ***, **, * respectively.

9.5 Appendix E: Anticipation Regressions

Anticipation Test for Turnover Around the Implementation of the Bonus Cap

	Turnover			Turnover Poor Performance		
	All executives (1)	CEOs only (2)	Non CEOs only (3)	All executives (4)	CEOs only (5)	Non CEOs only (6)
treat:post	0.013	0.024	0.013	-0.013	0.040	-0.029
	(0.740)	(0.644)	(0.780)	(0.725)	(0.363)	(0.493)
treat	0.164***	0.144**	0.161***	0.109***	0.051	0.120***
	(0.000)	(0.041)	(0.000)	(0.003)	(0.207)	(0.005)
Age	0.007***	0.004*	0.008***	0.005***	0.002	0.006***
	(0.000)	(0.073)	(0.000)	(0.000)	(0.233)	(0.000)
CEO	-0.110***	–	–	-0.063***	–	–
	(0.000)			(0.000)		
Female	0.037	0.102	0.036	0.014	0.130	0.012
	(0.175)	(0.591)	(0.188)	(0.512)	(0.516)	(0.581)
ROE	-0.246***	-0.178	-0.255**	–	–	–
	(0.007)	(0.282)	(0.028)			
Tenure	-0.001	-0.000	-0.000	0.001	0.000	-0.001
	(0.543)	(0.909)	(0.947)	(0.141)	(0.938)	(0.282)
Year Fixed Effects	X	X	X	X	X	X
N	2511	520	1991	2511	520	1991
R^2	0.038	0.034	0.029	0.020	0.010	0.015

This table shows the coefficients from our DiD anticipation analysis of turnover around the implementation of the bonus cap. The estimations are based on OLS regressions with year fixed-effects. The first post-treatment year is 2013, instead of 2014. The p-values are calculated by clustering standard errors on the bank level and are denoted in parentheses. We have included both the turnover (see regressions 1-3) and turnover poor performance (see regressions 4-6) dependent variables, and the executive level and bank level controls (see section 4.1 for variable definitions). We have also conducted regressions for CEO only and non-CEO only executives. From the CEO regressions, we have omitted the CEO dummy variable given that it is a constant 1 or 0 in such regressions and from the turnover poor performance regressions, we have omitted the ROE control variable given that turnover poor performance is constructed from banks' ROE measures. Significance in the 1%, 5% and 10% levels are indicated with ***, **, * respectively.

Anticipation Test for Risk-Taking Around the Implementation of the Bonus Cap

	CDS Spreads (1)	Market Beta (2)	Sharpe Ratio (3)	SRISK% (4)
treat:post	-0.092	-0.206	-0.055	0.000
	(0.736)	(0.101)	(0.351)	(0.969)
treat	-0.160	0.371*	0.067	0.087**
	(0.569)	(0.056)	(0.252)	(0.036)
ROE	-1.205	-2.019***	0.511**	-0.250**
	(0.242)	(0.000)	(0.016)	(0.013)
Year fixed effects	X	X	X	X
N	117	388	388	356
R^2	-0.049	0.145	-0.006	0.289

This table shows the coefficients from our DiD anticipation analysis of risk-taking around the implementation of the bonus cap. The estimations are based on OLS regressions with year fixed-effects. The first post-treatment year is 2013, instead of 2014. The p-values are calculated by clustering standard errors on the bank level and are denoted in

parentheses. We have included all 4 risk-taking dependent variables, namely, log of 5-year excess CDS spreads (see regression 1), market beta (see regression 2), Sharpe ratio (see regression 3) and expected capital shortfall (see regression 4). We have also included our bank level control variable, ROE (see section 4.1 for variable definitions). Significance in the 1%, 5% and 10% levels are indicated with ***, **, * respectively.

Anticipation Test for Turnover Around the Abolishment of the Bonus Cap

	Turnover			Turnover Poor Performance		
	All executives (1)	CEOs only (2)	Non CEOs only (3)	All executives (4)	CEOs only (5)	Non CEOs only (6)
treat:post	-0.277***	-0.281***	-0.287***	-0.200***	-0.158***	-0.215***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.003)
treat	0.098***	0.159***	0.082	0.075***	0.014	0.093**
	(0.010)	(0.001)	(0.118)	(0.010)	(0.673)	(0.033)
Age	0.007***	0.009***	0.006***	0.004***	0.001	0.005***
	(0.000)	(0.002)	(0.001)	(0.000)	(0.670)	(0.000)
CEO	-0.098***	–	–	-0.039***	–	–
	(0.000)			(0.002)		
Female	0.009	0.026	0.007	0.021	-0.009	0.025
	(0.752)	(0.760)	(0.808)	(0.360)	(0.892)	(0.310)
ROE	-0.492***	-0.465*	-0.495***	–	–	–
	(0.001)	(0.070)	(0.004)			
Tenure	-0.003**	-0.001	-0.003*	-0.003***	-0.004**	-0.003**
	(0.035)	(0.637)	(0.057)	(0.006)	(0.030)	(0.025)
Year Fixed Effects	X	X	X	X	X	X
N	1784	421	1363	1784	421	1363
R^2	0.047	0.046	0.027	0.022	0.009	0.015

This table shows the coefficients from our DiD anticipation analysis of turnover around the abolishment of the bonus cap. The estimations are based on OLS regressions with year fixed-effects. The first post-treatment year is 2023, instead of 2024. The p-values are calculated by clustering standard errors on the bank level and are denoted in parentheses. We have included both the turnover (see regressions 1-3) and turnover poor performance (see regressions 4-6) dependent variables, and the executive level and bank level controls (see section 4.1 for variable definitions). We have also conducted regressions for CEO only and non-CEO only executives. From the CEO regressions, we have omitted the CEO dummy variable given that it is a constant 1 or 0 in such regressions and from the turnover poor performance regressions, we have omitted the ROE control variable given that turnover poor performance is constructed from banks' ROE measures. Significance in the 1%, 5% and 10% levels are indicated with ***, **, * respectively.

Anticipation Test for Risk-Taking Around the Abolishment of the Bonus Cap

	CDS Spreads (1)	Market Beta (2)	Sharpe Ratio (3)	SRISK% (4)
treat:post	0.048	0.320***	0.249***	-0.002
	(0.786)	(0.000)	(0.001)	(0.914)
treat	-0.494***	0.422***	0.003	0.103***
	(0.006)	(0.003)	(0.962)	(0.012)
ROE	-1.818	0.184	0.552***	-0.091
	(0.579)	(0.611)	(0.003)	(0.260)
Year fixed effects	X	X	X	X
N	110	399	399	315
R^2	0.013	0.404	0.101	0.331

This table shows the coefficients from our DiD anticipation analysis of risk-taking around the abolishment of the bonus cap. The estimations are based on OLS regressions with year fixed-effects. The first post-treatment year is 2023, instead of 2024. The p-values are calculated by clustering standard errors on the bank level and are denoted in parentheses. We have included all 4 risk-taking dependent variables, namely, log of 5-year excess CDS spreads (see regression 1), market beta (see regression 2), Sharpe ratio (see regression 3) and expected capital shortfall (see regression 4). We have also included our bank level control variable, ROE (see section 4.1 for variable definitions). Significance in the 1%, 5% and 10% levels are indicated with ***, **, * respectively.

9.6 Appendix F: Usage of Generative AI-tools

The Generative AI platforms, ChatGPT and Gemini were used within the writing of this thesis. Specifically, we have used AI in spelling and grammar checks, idea brainstorming in the initial stages of the thesis, code generation with regards to the empirical methodology and the making of tables using LaTeX, interpretation of literature, help in navigation of empirical databases, and insights into different empirical methods and the defence for such empirical methods. We have not directly outsourced data collection or writing with AI.

These AI tools have significantly improved the quality of our thesis. With regards to code generation, it allowed us, with limited experience in R programming, to conduct complex data cleaning, assembly and regressions. Moreover, it allowed us to conduct several diagnostic and robustness tests with ease, giving us the opportunity to focus our efforts in understanding and interpreting extant literature, and writing insightful discussions using our findings. All-in-all, we outsourced the more manual tasks to AI while focusing on the tasks that required critical thinking and human intuition. Moreover, using AI to help navigate complex databases like Capital IQ and LSEG Datastream allowed us to use a variety of data sources in collecting our data which became crucial due to our already small sample sizes. As students of Accounting, Valuation and Financial Management, our expertise lies in understanding theoretical and empirical literature in Accounting and Finance, however, a large part of the thesis also relies on a strong foundation in econometrics. Especially when utilising complex regression methods like DiD analyses, our prior econometrics knowledge may be, to some extent, subpar. Thus, utilising AI to learn more about different empirical methods, and the pros and cons that come with them, we were able to adopt and defend the best research design that fit our unique research question and sample. Lastly, using AI to generate LaTeX code, or for spelling and grammar checks, allowed us to produce a thesis with academic language and presentation.

There exist several risks in the usage of AI, like hallucinations or simply the generation of incorrect outputs. Hence, when using AI to collect information, whether it be interpretations of literature, navigations of databases or coding for our empirical method, we made sure to verify the information from non-AI sources to the best of our ability. Moreover, when conducting spelling and grammar checks, or when generating LaTeX tables, we made sure to cross-check the

text or values with our original writing or outputs. Moreover, we abstained from the usage of AI when it came to writing any large portion of the thesis, or direct data collection. The insights that we have gained from AI have been several from additional knowledge when it comes to empirical methodology or different perspectives when it comes to literature. However, the main insight that we have gained is to position AI as a mere complement to human judgement and intuition. We have learnt that in the writing of a thesis, natural critical thinking is something that is incumbent and irreplaceable. Thus, we have always maintained a certain level of distance when it comes to any attachment or reliance on AI recognising its dangers and risks. Consequently, we can affirm that the work presented above is a product of our own efforts, time and intellect.