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Escaping Pay-for-Performance: Evidence from Financial Regulators

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Abstract

How do financial regulators respond to performance pay? Using the staggered transition to pay-for-performance by U.S. financial regulatory agencies and newly assembled career data on 30,000 regulators, we find that performance pay increased voluntary exits by 40-54%. A parallel reform for federal executives produced similar effects. Performance pay boosts rulemaking effort, raising the likelihood of drafting a new rule by 58-63%, and improves private sector pay, as leavers earn 38% higher starting salaries. It also weakens public sector attachment through income risk and distrust of performance evaluations. As a result, regulators with better outside options and weaker intrinsic motivation leave.

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Introduction

Since the 2024 Presidential elections, federal regulators have come under a harsh spotlight. As a warning shot, in February 2025 the Department of Government Efficiency (DOGE) ordered all employees to report their top five accomplishments. More fundamentally, the new administration seeks to closely monitor federal employees and link their performance to pay.[1] The push to reward merit in the public sector mirrors a long line of corporate governance research, investigating how executives and directors respond to performance pay in particular and to labor market incentives in general (Levit and Malenko, 2016). The context of regulatory agencies is arguably even more consequential, as those officials are paid to oversee key economic functions including the banking system and securities markets. The incentives provided to regulators affect the quality of their oversight, with consequences that extend to the industries and markets they regulate. Yet despite decades of experimentation with performance-based pay in regulatory agencies, these reforms remain poorly understood and little is known about their unintended consequences. In particular, while performance pay may encourage effort, it can also create dissatisfaction or distort incentives in ways that undermine the goals the reforms were designed to achieve.

This backdrop motivates our central research question: how do regulators respond to performance pay? We address this question by exploiting the staggered transition to performance pay by U.S. financial regulatory agencies over several decades. Using newly assembled employee-level data that track careers both within and beyond government, we find that the introduction of performance pay led to a substantial rise in voluntary exits from the public sector.[2] This result is counterintuitive: pay in the private sector is even more sensitive to performance and poor performance can lead to bigger pay cuts. Since pay cuts are rare in the government, a wage contract with a performance component resembles a call option, where performance pay increases the volatility and hence the contract value. Nevertheless,

[1] See, for instance, a Presidential Memorandum from January 2025 (90 FR 8481).

[2] We find a comparable response to an independent performance pay reform for federal executives.

our results show that performance pay raises the value of the outside option even more, resulting in exits.

To interpret this response, we show how performance pay reshapes the landscape via two broad economic channels: it improves the outside pay and weakens the appeal of the public sector. With stronger incentives, regulators exert more effort. This increases potential salary gains in the private sector, likely by providing better signal to potential employers. At the same time, performance pay weakens the appeal of a public sector job due to greater income risk and aversion toward the government's performance evaluation process. The result is selective attrition: regulators with more lucrative private sector opportunities exit, leaving behind regulators with stronger intrinsic attachment to the public sector.

Our findings reveal a central trade-off in the design of incentives within the public sector. Performance pay enhances productivity among regulators, yet simultaneously raises the value of the outside option for those most responsive to incentives. Thus, reforms intended to reward excellence can unintentionally accelerate the revolving door and alter the composition of the regulatory workforce. This insight connects directly to the revolving door literature: while prior studies focus on whether the revolving door induces regulatory leniency or capture, our paper identifies a novel *cause* of the revolving door – compensation design itself. The mechanisms we uncover – the joint determination of effort and exit under performance pay, and the role of the outside option and credible evaluation – may extend beyond regulators. Similar forces could arise in other settings where talent is scarce and outside options are attractive, especially if employee performance is difficult to measure.

In the first part of the paper, we examine the staggered transition to pay-for-performance (P4P) systems by seven U.S. financial regulatory agencies between 1981 and 2006.[3] The reforms aimed to link individual compensation more closely to performance evaluations, reduce

[3]Those are the Office of the Comptroller of the Currency (OCC, 1981); National Credit Union Administration (NCUA, 1991); Farm Credit Administration (FCA, 1993); Internal Revenue Service (IRS, 2000); Securities and Exchange Commission (SEC, 2002); Federal Deposit Insurance Corporation (FDIC, 2003); and Commodity Futures Trading Commission (CFTC, 2006). The sample also includes three agencies that never transition and serve as controls: the Office of Thrift Supervision, the Office of Federal Housing Enterprise Oversight, and the Financial Crimes Enforcement Network.

pay compression, and retain high-performing employees. This involved replacing nearly automatic step increases with merit-based raises and introducing wider pay bands that allowed managers to reward top performers and differentiate pay more sharply across staff. Although these agencies share broadly similar statutory mandates and labor market environments, they introduced P4P at different points in time, generating a quasi-experimental setting to study how incentive reforms reshape regulator behavior.

We source a new dataset of federal payroll records, covering over three decades of employment histories for 30,000 individual financial regulators. We supplement these records with three additional data sources: newly constructed employee-level measures of rulemaking activity from the *Federal Register*, which capture each regulator's participation in rulemaking; individual career histories from Revelio Labs, which document pre- and post-government employment and let us construct outside option measures for regulators based on the firms that hire them; and anonymous responses to the Federal Employee Viewpoint Survey, which capture how regulators perceive their agency's performance evaluation process. Together, these data provide an unusually detailed view of the internal dynamics of major financial regulatory agencies and the broader consequences of incentive reforms in those agencies.

To estimate the causal effect of P4P on turnover, we employ a stacked difference-in-differences design. Treatment is a change in pay regime, from non-P4P to P4P. Each cohort is a seven-year event window around one P4P reform, comparing agencies making the transition (treated) with agencies that have never transitioned or are yet to transition (control). We include rich fixed effects, especially agency×cohort and occupation×year×cohort, to absorb persistent differences across agencies and time-varying shocks within occupations. These ensure that the estimated effect is identified from within-cohort variation in exit rates across agencies over time. We progressively saturate the model with additional fixed effects that absorb pre-reform pay and tenure, as well as education, managerial status, age, and location, each interacted with cohort and year, so that comparisons are made among regulators who looked alike on these dimensions before the reform. Across all specifications, we find a

substantial and persistent rise in exits following the transition to performance pay, ranging from 40% to 54% relative to the sample mean.

The next set of tests reinforces the causal interpretation, by showing that other factors are unlikely to generate such repeated pattern of post-reform turnover. Employees at treated and control agencies are balanced along observable characteristics, including turnover rates, before the transition to performance pay. A formal pre-trend test cannot reject that exit rates were flat before each reform, and a power analysis rules out a pre-existing trend large enough to generate the post-reform jump on its own. The result survives excluding any single cohort or agency, alternative standard-error clustering such as a wild cluster bootstrap, and alternative sample weights. We also find no evidence that stronger external demand for regulators drove the reforms. Using Revelio data on firms hiring departing regulators and Compustat data on industry staff expenses, we construct six proxies for outside opportunities; none predicts the timing of agency adoption of performance pay.

To assess the external validity of our findings, we examine a separate reform that introduced performance-based pay for top federal executives. This group, known in government parlance as the Senior Executive Service (SES), comprises 24,000 officials managing key regulatory and policy functions across the government. Using a similar payroll dataset, we track the executives' careers before and after the shift to performance pay in 2004. In a difference-in-differences design, we find that the reform increased exit rates among treated executives by 3.5–3.7 percentage points, roughly 35% relative to the mean. The response is comparable in magnitude to that observed among financial regulators, reinforcing the causal interpretation of our results. Because the SES reform applied simultaneously across hundreds of agencies, the rise in turnover cannot be attributed to idiosyncratic conditions of any single agency. Instead, it suggests that the response to performance pay generalizes well beyond the financial regulatory domain.

To understand the causal link between performance pay and exits, we investigate two broad channels triggered by performance pay: the reforms increase potential private sector

salaries while reducing the intrinsic appeal of government service. Together, these channels jointly explain the aggregate rise in exits, the heterogeneous effects across subsamples, and the reforms' impact on other outcomes beyond turnover. Concretely, we present five key results consistent with those two channels.

First, performance pay increases rulemaking activity. Using our new regulator-level measures of rulemaking from the *Federal Register*, we find that treated regulators increase both the likelihood of contributing to at least one new regulatory document and the number of new regulatory documents, and the effect holds when comparing each regulator's own activity before and after the transition (with regulator fixed effects). The point estimates correspond to a 58–63% increase relative to the sample means.[4] By stimulating greater effort, the reforms achieved one of their intended goals – enhancing regulatory productivity, highlighting the bright side of performance pay. At the same time, the increased effort can improve the potential private sector pay and weaken the preference for a public sector job, mechanisms which we explicitly examine below.

Second, performance pay leads to improved outside job opportunities. Using Revelio data, we find that treated regulators who exited after the reform earned 38% higher starting salaries and 53% higher total compensation than comparable leavers from control agencies. These gains are especially large among regulators for whom performance-based signals should be most informative: those with shorter pre-reform tenure and those without a postgraduate degree. This pattern suggests that the reform increased regulators' value in the external labor market by providing a signal of worker quality to potential employers.

Third, performance pay increased income risk inside the government. Pay dispersion across regulators widened by 25%. Pay volatility per individual rose as well: the probability of promotion to senior rank nearly triples, and the probability of a large pay cut rises by about 73%, both relative to their sample means. These shifts indicate that performance pay raised the stakes of annual evaluations, replacing the traditional stability of government

[4]The total documents produced by treated agencies increases, while the number of coauthors per document remains unchanged.

pay with greater income risk. For risk-averse employees, this volatility undermined a key attraction of public employment and contributed to the desire to exit.[5]

Fourth, performance pay is susceptible to distrust among employees, as its effectiveness depends on credible and transparent evaluations. Because performance in the government is difficult to measure and tasks are multi-dimensional, many regulators perceived the new appraisal process as opaque or unfair. Consistent with this possibility, we find that exits were concentrated in agencies where evaluations were perceived as less transparent and among employees with few comparable peers (noisier assessments). These results suggest that performance pay weakened public sector attachment by exposing regulators to evaluation systems they viewed as unreliable, prompting those most averse to such systems to seek employment elsewhere.

Fifth, performance pay triggered selective exits based on ex-ante intrinsic motivation to serve in the public sector. We measure this using three characteristics fixed when employees entered government: whether they joined during a recession, the strength of the private-sector labor market facing their agency at entry, and the corresponding labor-market strength for their occupation. Employees who joined when private opportunities were scarce were significantly more likely to leave after the reform, whereas those who joined when opportunities were plentiful were more likely to remain. These results suggest that performance pay acted as a sorting mechanism, disproportionately increasing turnover among employees with weaker ex-ante attachment to public service.

In sum, the transition of regulators to performance pay generates a fundamental trade-off: performance pay enhances productivity among regulators, yet simultaneously raises the value of the outside option for the very employees most responsive to incentives. This leaves the overall effect on agency effectiveness and welfare ambiguous. If employees with stronger intrinsic motivation are more likely to remain, the resulting workforce may be better

[5]Income volatility may also push out more risk-averse regulators. While this may contribute to turnover, the observed rise in private sector pay, and the fact that many leavers likely move to even more risky positions, suggest that risk aversion alone cannot explain the overall pattern.

aligned with the agency's mission. But we also find that exits are concentrated among individuals with stronger outside options, presumably those who may contribute most to the agency's technical capacity. This pattern raises the concern that selective attrition may erode expertise in areas where it is most difficult to replace, even as it strengthens mission orientation among incumbents. Whether this trade-off leaves agencies more or less effective overall is an important question that we leave for future work.[6]

Our paper makes several contributions to the literatures on regulatory incentive design, the revolving door, and performance pay and compensation. First, studies tend to link regulatory performance to the level of pay (Dal Bó, Finan, and Rossi, 2013; Kalmenovitz, 2021) and to organizational features such as field offices (Gopalan, Kalda, and Manela, 2021) and supervision hours (Hirtle, Kovner, and Plosser, 2020). Our paper highlights the importance of the pay *structure* and specifically the role of performance-based pay.

Second, a large literature studies the prevalence of the revolving door, generally without considering how it is affected by the design of regulatory pay (deHaan et al., 2015; Lucca, Seru, and Trebbi, 2014; Shive and Forster, 2017; Cornaggia, Cornaggia, and Xia, 2016; Kempf, 2020; Lambert, 2019). Our work shows that pay design directly affects the revolving door: the incentive to switch sectors increases when regulators receive performance pay. More broadly, the pay structure affects the joint decision on exit and effort, and a pay policy that increases effort also raises the value of the outside option.

Third, we contribute to the literature on how performance pay affects workforce composition. In existing work, performance pay sorts in the adopting employer's favor: firms that reward performance attract more productive workers (Lazear, 2000), and the few studies of performance pay and turnover document better retention under it (O'Halloran, 2012; Artz and Heywood, 2025), as does related evidence on broad-based stock options (Aldatmaz, Ouimet, and Van Wesep, 2018). We document the opposite in the public sector and explain

[6] An earlier version of the paper formalized this trade-off in a structural model of the regulator's joint decision on effort and exit and used the estimated model to evaluate counterfactual pay policies. See the Model Appendix on the authors' website.

why. Because pay upside inside the government is capped while the private sector prices performance fully, the reform raises what regulators can earn outside, in part by revealing their value to outside employers, while income risk and distrust of government evaluations weaken the appeal of staying. Regulators with the strongest outside options and the weakest ex-ante attachment to public service are the ones who leave.[7]

Finally, an extensive literature studies performance pay in the private sector (Yermack, 2004; Frydman and Jenter, 2010; Maksimovic and Yang, 2023). We examine performance pay among federal regulators, a relatively understudied group performing economically consequential functions. A key insight in our paper is that when outside options are strong and responsive to effort, stronger pay incentives can raise effort while also increasing the value of exit. This insight parallels challenges that boards face when designing executive compensation in competitive labor markets (Edmans, Gosling, and Jenter, 2023). Interestingly, our setting differs from the corporate one in a key way: the outside option for a regulator is typically the very industry they oversee, a conflict of interest that does not arise for corporate executives.

1 Background and Data

1.1 Institutional setting

Our empirical setting exploits the staggered transition from non-performance-based pay to pay-for-performance (P4P) systems across U.S. financial agencies. The reforms shared broadly similar goals and features but differed in several respects; to conserve space, we defer the description of each agency's pay systems to Appendix A.2 and give only a brief account here. We exploit some of this heterogeneity in Section 4, where we turn to the economic mechanisms.

[7] A companion literature documents other forces affecting the composition of the financial regulatory workforce, including political affiliation (Chen, Kalmenovitz, and Sosyura, 2025).

During our sample period, seven agencies experienced a transition in pay regime from non-P4P to P4P: the Office of the Comptroller of the Currency (OCC) in 1981, the National Credit Union Administration (NCUA) in 1991, the Farm Credit Administration (FCA) in 1993, the Internal Revenue Service (IRS) in 2000, the Securities and Exchange Commission (SEC) in 2002, the Federal Deposit Insurance Corporation (FDIC) in 2003, and the Commodity Futures Trading Commission (CFTC) in 2006. Our difference-in-differences empirical strategy relies on three additional financial agencies that did not experience such a transition during our sample period: the Office of Thrift Supervision (OTS), the Office of Federal Housing Enterprise Oversight (OFHEO), and the Financial Crimes Enforcement Network (FinCEN). The first two operated under P4P from their inception, and the third remained on the General Schedule (GS) system throughout; none of the three experienced a change in pay regime.

The performance-pay reforms aimed to accomplish three goals. First, to align individual performance more closely with organizational objectives. By embedding those objectives directly into individual performance plans, regulators aimed to create a clearer link between employees' day-to-day responsibilities and the agency's mission, thereby fostering a more motivated and productive workforce (Ginsberg, 2008). Second, to reduce pay compression and financially motivate employees to exert more effort. For example, investigating the SEC's pre-reform pay structure, a commission found that "there are very limited incentives to become a manager. A 'promotion' to supervisory status often means more responsibility with no increase in pay" (U.S. Securities and Exchange Commission, 2002). Performance-based compensation was intended to widen the spread between high- and average-performing staff, and to ensure that career advancement came with meaningful financial rewards. Finally, the reforms aimed to allow financial agencies to "recruit and retain employees critical to meeting their organizational missions" (U.S. Government Accountability Office, 2007). Unlike most federal agencies, financial regulators compete directly with banks and other employers offering substantially higher pay. By restructuring their compensation systems, agencies were

able to reward productive staff with salaries closer to private sector benchmarks.

1.2 Data

In this section, we describe the primary datasets we construct for the 10 financial agencies listed in Section 1.1: the seven that transitioned to P4P during the sample period and the three that did not.

1.2.1 Payroll

We source a comprehensive dataset covering all employees who worked at any of the 10 financial agencies between 1973 and 2013. The dataset, released by BuzzFeed News following a Freedom of Information Act request, provides information on each employee's agency, occupation, hiring date, location, and compensation. To the best of our knowledge, it captures the universe of financial regulators during this period.

We begin with 60,124 unique financial regulators who were employed at either treated or control agencies three years before their respective reforms and track their payroll records over the seven-year window, yielding 497,896 regulator×year observations. Our empirical strategy, detailed in Section 3.1, focuses on regulators employed at financial agencies prior to the introduction of performance pay and follows them over a seven-year event window centered on the reform year. Thus, the pre-reform employment requirement ensures that we observe only regulators who were already in the agency before the reform was announced, thereby mitigating estimates being contaminated by self-selection into treatment.

To execute our identification strategy, we restrict the sample in several ways. Because our empirical design exploits within-individual variation over time and compares employees across locations and agencies, information on pay, location, education, and rank is essential. We therefore exclude regulator×year observations with missing or inconsistent entries in these fields to ensure clean identification of the treatment effect. For instance, missing location data would compromise the ability to absorb regional labor market shocks. After

these filters, the final – and main – sample consists of 31,055 unique financial regulators across 10 agencies, totaling 232,785 regulator×year observations.[8]

1.2.2 Rulemaking

Part of the analysis requires a reasonable measure of regulatory activity. To that end, we collect new data on rulemaking from the Federal Register (FR), the official daily publication of the U.S. government. The FR is the primary channel through which federal agencies announce proposed and final rules, making it an indispensable source for tracking regulatory output over time (Chen and Kalmenovitz, 2024; Kalmenovitz, Lowry, and Volkova, 2025). Starting in 2000, the FR provides detailed metadata on each rulemaking document, including the names of the individual regulators involved in drafting the rule. These features allow us to link individual regulators to specific regulatory events, providing a direct measure of regulatory effort.

Since the FR data begin in 2000, we focus on agencies in the 2002, 2003, and 2006 cohorts, for which both pre- and post-reform data are available. We restrict the sample to 2000-2009, which covers the post-reform event window of the last cohort, resulting in 2,980 regulatory documents associated with up to 1,143 unique regulators.[9] We match those to identifiable payroll records in the corresponding period, using agency affiliation along with first and last names. For individuals with complete first names, we perform a fuzzy match on the first name combined with an exact match on agency and last name. We then use the ChatGPT API to evaluate each potential match, taking into account that nicknames are sometimes used in the Federal Register (e.g., "Tim" instead of "Timothy"). Each potential match is subsequently reviewed manually to exclude false positives. For individuals with incomplete

[8]Note that this number is smaller than the counts reported in the regression tables, because the same regulator×year observation may appear in overlapping cohorts. Excluded observations are concentrated in the Financial Crimes Enforcement Network (79.7% of excluded observations) and the IRS (6.3%); for both agencies the exclusion is driven almost entirely by missing location data (98.1% and 99.3% of their respective exclusions), consistent with the sensitivity of their operations preventing disclosure of employee locations.

[9]The true number of unique regulators is somewhat smaller because names are not always recorded consistently across documents. For instance, a regulator may include a middle name in some filings but omit it in others.

first names, we proceed more cautiously given the higher risk of matching error. We begin by matching the first name initial in the payroll data to the first letter of the first name in the Federal Register, requiring exact matches on last name and agency. We then manually inspect each candidate match, retaining those where the middle name is an exact match and those with relatively uncommon last names. For example, an individual with the last name "Seifert" would be retained whereas those with "Smith" would not. When multiple matches remain for the same name, we prioritize cases with an exact first-name match. If ambiguity persists, we retain the individual identified as an attorney in the payroll data, since the vast majority of rulemakers are attorneys.

This process yields 743 unique financial regulators from six unique agencies (SEC, FDIC, OTS, FinCEN, CFTC, and OFHEO) who are also in our main sample.

1.2.3 Outside option

Parts of the analysis require information on the outside option. To that end, we combine the federal payroll dataset with detailed career histories from Revelio Labs. The Revelio data allow us to examine the outside option facing financial regulators, both at the point of entry to the public sector and at the point of exit. In addition to compensation information,[10] Revelio provides data on organizational type, seniority, and industry affiliation, enabling a richer assessment of how pay-for-performance reforms shaped regulators' labor market opportunities and mobility patterns. We measure the outside option in three levels of aggregation, and utilize each for different tests discussed below. We provide here a brief description and a detailed explanation in Appendix A.3.

At the agency level, we identify individuals whom Revelio records as having worked at one of our financial agencies for at least one year and track their subsequent career destinations. The private sector firms that hire these former regulators constitute the agency's set of

[10] Revelio imputes compensation using job characteristics such as title, company, and location (Dorn et al., 2025). While this approach could inevitably include measurement error, we are not aware of any systematic bias correlated with the timing of the performance pay treatment.

destination firms: 29,498 firms in total, including 3,605 publicly traded firms and 25,893 privately owned firms. Because the firms regulators actually join may themselves reflect the labor market conditions we seek to measure, we also construct two comparison groups that do not depend on realized moves: private sector firms operating in the same local labor markets and narrow industries as the destination firms, and publicly traded firms in the same state and broad industry. Our primary measure of the outside option is the pay offered to newly hired employees. For publicly traded firms, we additionally observe staff expense per employee and total staff expense. We aggregate each measure to the agency-year level by averaging across the relevant firms.

We follow a similar procedure to compute the outside option at the agency-occupation level. We first group the 193 federal occupational series into 10 broad occupations, and identify employee transitions across federal and private sector occupations. We then repeat the agency-level construction within occupation, so that both hiring salary and destination firms are occupation-specific.

Finally, at the individual level, we link payroll records to Revelio profiles based on key filters: last name, first initial, agency, year of entry into and exit from the agency, and implied year of birth. Because first names are recorded inconsistently across the two sources, we adjudicate nickname variants with a large language model. We supplement the matching with hand-collected LinkedIn profile URLs. The procedure links 2,965 of the 27,934 regulators whose names we can recover, spanning 9 of the 10 sample agencies; the IRS is absent because it masks the names of all its employees. As expected, matched regulators differ from unmatched regulators on observable characteristics: they are younger and shorter-tenured, better educated, better paid, and slightly less likely to exit (see Table A.1).

2 Descriptive Statistics

2.1 Career trajectories

Table 1 reports descriptive statistics for our main sample. Compensation variables are adjusted for inflation and expressed in constant 2023 USD. In Panels A and B, we focus on the government portion of their careers based on payroll records. The average regulator is 42 years old and has 12 years of government experience. The unconditional turnover rate among regulators is 5.3% and retirement rate, defined as exiting the government at age 60 or older, is 0.9%. 82.3% and 27.8% of regulators have college and postgraduate education, respectively. The average regulator earns $127,813 and the unconditional probability of experiencing a large pay cut is 4%.[11] On average, 0.6% of employees are promoted to senior rank each year.[12] In the subsample of 743 rulemakers, 16.9% initiate at least one new rulemaking draft – publishing a proposed or final rule in the *Federal Register* – in a given year. Conditional on publishing, the average annual number of new drafts started is 1.9.

In Panel C, we turn to the post-government portion of their careers. We focus on 1,013 unique financial regulators with available Revelio records who left the government,[13] and describe their first job upon leaving government service. The overwhelming majority of ex-regulators (81%) moved to for-profit firms, while a smaller share joined nonprofit organizations (9%), universities (1%), or state and local government (9%). Roughly half of those entering the private sector joined publicly listed companies. Compensation differences between the public and private sectors are stark: the average government salary prior to exit was about $135,000, compared to an average starting private sector salary of over $193,000. When including bonuses and other forms of remuneration, total private compensation av-

[11] A large pay cut is defined as a real pay cut of at least 3.03%, which is the 95th percentile of real pay cuts during our main sample.

[12] We define promotion to senior rank as advancement to positions within the EX, ES, EO, EM, CM, OE, SL, SO, SS, VE, WL, WS, XL, or XS pay plans (as defined by OPM). These positions represent the top echelons of the government workforce. Such promotions are relatively rare and represent substantial career advancement, analogous in spirit to our measure of extreme pay cuts, as both capture tail events in the distribution of pay adjustments.

[13] This is a subset of the 2,965 regulators in our sample with available Revelio records.

eraged roughly $283,000. On average, performance pay constituted nearly one-quarter of this compensation, highlighting how different the structure of pay packages is outside of government.[14]

In Figure A.1 we report additional information on the first post-government job held by revolving door financial regulators. A substantial fraction (43.5%) moved directly into high-level positions, including 3.9% at the very top tier such as CFO or CEO. Looking at industries in which these individuals landed, financial services (37.1%) and legal services (30.7%) dominate, consistent with the professional expertise of regulators and the demand for their skills in compliance-heavy sectors. Consulting (7.7%) and public sector management (6.7%) also feature prominently, the former reflecting the portability of regulatory knowledge into advisory roles.

Table A.2 describes the pre-government careers of our sample regulators. Most had worked outside the public sector before joining a financial agency: 62% previously held a position at a for-profit firm and 26% at a publicly traded company, while 29% arrived from non-financial federal agencies and smaller shares from nonprofit organizations (7%) or academia (2%). Panel B reports the seniority of the last position held before entry, and Panel C the industries from which regulators were drawn.

2.2 Pay-for-performance reforms

While the performance-pay reforms were implemented at different points in time, their unified goal was to create meaningful distinctions in pay between higher- and lower-performing employees. We illustrate this in Figure 1, which plots the inflation-adjusted salary distribution of transitioning agencies three years before (red dotted line) and three years after (blue solid line) the reform. Because the reforms occurred at different times, we use inflation-adjusted pay rather than raw pay. Prior to transition, salary distributions are relatively

[14]Because Revelio reports total compensation only for the final year of each position, we approximate starting total compensation by scaling the reported final value using the observed growth in base salary between the first and last paychecks.

compressed; afterward, they widen substantially. The standard deviation of pay rose from \$51,041 to \$62,070, a 21.6% increase. Similarly, the pay gap between the 90th and 10th percentiles expanded from \$133,110 to \$166,960, a 25.4% increase. Thus, the implementation of performance pay led to a marked widening of pay differentials among financial regulators. This provides indirect evidence that the new pay-for-performance systems were not merely "cheap talk," but rather had a demonstrable impact on regulator compensation wherever implemented. Note that the expectation of greater dispersion could also generate income risk and increase regulators' aversion to the new pay system ex-ante. We return to that in Section 4.3.

3 Impact on Exits

We begin our analysis by examining whether performance pay affected regulators' propensity to leave the government. We describe our empirical strategy, main results, and robustness tests. These baseline findings set the stage for the analysis in Section 4, which clarifies the mechanisms through which performance pay influences both effort and attrition.

3.1 Empirical strategy

To examine how the staggered transition to pay-for-performance (P4P) by financial agencies affected employees' propensity to exit government, we estimate the following stacked difference-in-differences specification:

$$Exit_{c,i,a,t} = \beta \cdot Post_{c,t} \times Treated_{c,a} + \alpha_{c,o,t} + \alpha_{c,a} + \varepsilon_{c,i,a,t}. \tag{1}$$

Here, $Exit_{c,i,a,t} = 1$ if individual i in agency a and cohort c exits government employment in year $t+1$, and 0 otherwise. $Treated_{c,a} = 1$ if agency a transitioned to P4P in cohort c, and $Post_{c,t} = 1$ in the post-transition period of cohort c. Treatment is thus a change in pay

regime, from non-P4P to P4P; an agency that operated under P4P throughout the event window is never treated.

We construct the sample using a seven-year event window (±3 years) around each agency's transition to P4P. The control group consists of agencies that had not yet transitioned to P4P during the cohort window, together with the three agencies that never changed pay regime during the sample period: OTS, OFHEO, and FinCEN (see Appendix A.2). To avoid confounding reforms, we exclude agencies that made substantial changes to their pay structure within five years before the event window.[15]

Table 2 compares treated and control employees on observable characteristics prior to the reforms. Average real pay is $109,295 among control employees and $127,636 among treated employees, a difference that is not statistically significant. Promotions to senior rank are equally rare in both groups (0.3% versus 0.2%), large pay cuts are less common among treated employees (5.5% versus 3.9%), and managerial representation is 20.9% versus 24.8%; none of these differences is statistically significant. Exit rates are identical at 7.1%, and retirement rates (1.1% versus 0.8%), age (42.0 versus 41.0 years), and tenure (12.0 versus 12.5 years) are comparable. The only statistically significant difference is college education, which is more common among treated employees (80.1% versus 87.0%); postgraduate education runs the other way (31.6% versus 28.3%) and is not significant.[16] Although balance is not strictly required for identification, the near absence of pre-reform differences – especially in exit rates – is helpful: it demonstrates that treated agencies were not already losing employees at a higher rate before the reform. This confirms the conclusion in Appendix A.2, namely that the staggered timing of the reforms was driven by agency-specific statutory authority, rather than by shocks to the demand for regulators specific to treated agencies. We return to this in Section 3.3, where we test directly whether pre-reform changes in outside pay predict the

[15] There are three such episodes in our sample. The FDIC underwent a 1989 reform that aligned its pay with other financial regulators, so we exclude it from the 1991 and 1993 cohorts. Similarly, we exclude the Farm Credit Administration from the 1991 cohort (due to the 1991 VE pay plan reform) and the CFTC from the 2002 and 2003 cohorts (due to its 2003 switch from GS to CT pay).

[16] We address concerns that pre-reform pay and educational differences explain our findings: we control for pre-reform pay and demonstrate that adding education-by-year fixed effects leaves the results unchanged.

timing of transition.

The fixed effects absorb differences across agencies, occupations, and time that could otherwise drive a gap in exit rates between treated and control regulators. All our specifications include agency×cohort ($\alpha_{c,a}$) and occupation×year×cohort ($\alpha_{c,o,t}$) fixed effects. The former absorb time-invariant differences across agencies within a given cohort, for example, persistent differences in turnover rates between the SEC and the FDIC. The latter absorb time-varying occupational shocks, such as changes in labor market conditions for lawyers, that could confound exit behavior.[17] These fixed effects also subsume the main effects of $Treated_{c,a}$ and $Post_{c,t}$. Beyond this baseline, we progressively add fixed effects for pre-reform pay and tenure, education, managerial status, age, and location, each narrowing the comparison to regulators who are alike on a further dimension. The coefficient of interest, β, captures the differential change in exit rates between treated and control agencies within the cells defined by the fixed effects of a given specification: the same occupation, cohort, and year in the baseline, and progressively narrower cells thereafter.

Standard errors are clustered at the agency×cohort level (36 clusters); we discuss alternatives in Section 3.3.

3.2 Results

Table 3 presents the main results. Column (1) is the baseline specification, with agency×cohort and occupation×year×cohort fixed effects only. Column (2) adds cohort×pre-pay quintile and cohort×pre-tenure quintile FE, so that each regulator is compared with others who occupied the same position in the pre-reform pay and tenure distributions. Column (3) adds education×year×cohort FE, allowing comparisons among employees with similar educational backgrounds in the same year.[18] Column (4) adds manager×year×cohort FE, absorbing time-varying shocks that differentially affect managers and non-managers. Col-

[17] This mirrors the GAO's approach, which compares exit rates and salaries across financial agencies within the same occupation. See Figure 3 and Table 10 of GAO-07-678.

[18] Education is grouped into high school, college, and graduate degree.

umn (5) adds age-bin×year×cohort FE, and column (6) adds city×year×cohort FE to absorb local economic conditions.

Across all six specifications, the coefficient on *Treated*×*Post* is positive and significant at the 1% level, indicating that treated agencies experienced higher turnover after transitioning to P4P. The magnitudes are economically meaningful and, if anything, grow as the specification tightens: exit propensities rise by 3.1–4.2 percentage points, or 40–54% relative to the sample mean of 7.8%. Notably, the estimate is identical in columns (1) and (2) (0.031 in both), so the effect is not an artifact of treated regulators occupying different positions in the pre-reform pay or tenure distribution; nor is it driven by the educational imbalance documented above, since the estimate moves only from 0.031 to 0.032 when education×year×cohort fixed effects are imposed in column (3).

Columns (7) and (8) split total departures into those more likely associated with transitions to private sector jobs (below age 60) and those potentially due to early retirement (age 60 or above). The estimated increase is 0.038 for the former and 0.004 for the latter. The retirement effect is positive and statistically significant, but substantially smaller. Because the two outcomes partition total departures, the estimates sum to the 0.042 effect in column (6). Roughly 90% of the increase in exits therefore comes from regulators below age 60, consistent with private sector transitions accounting for most of the response.

The key identifying assumption is that, absent the reform, exit rates in treated and control groups would have followed parallel trends. While inherently untestable, the staggered transition to P4P across multiple cohorts makes alternative explanations less plausible, since any confounder would have to account for sharp increases in exits at different agencies and times. To further probe this assumption, we estimate a dynamic version of Equation 1:

$$Exit_{c,i,t+1} = \sum_{k=-3}^{+3} \eta_k \cdot Treated_{c,a} \cdot [\mathbb{1}(Event\,Time_{c,t} = k)] + FE + \epsilon_{c,i,o,a,t}, \tag{2}$$

Here, $\mathbb{1}(EventTime_{c,t} = k)$ indicates that year t in cohort c is k years from P4P transition.

Following Miller (2023) and Borusyak, Jaravel, and Spiess (2024), we normalize the average of the three pre-transition coefficients to zero, which uses the full pre-transition period as the benchmark and improves the precision of the event-time estimates. Figure 2 plots the estimated coefficients η_k for all exits (Panel A) and exits before age 60 (Panel B). The figure shows a discrete break at the P4P transition. The three pre-transition coefficients are statistically insignificant and display no upward drift, indicating that exits were not already rising ahead of the reform. From the transition year onward, every coefficient is positive in both panels, with the largest effect occurring in the transition year itself. The two panels are nearly indistinguishable, indicating that essentially all of the dynamic response is driven by departures before age 60, consistent with columns (7) and (8) of Table 3.

We assess the pre-reform path formally in two ways. First, we test whether the treated-control gap in exit rates is constant across the three years before the transition. We cannot reject this hypothesis for either outcome, under either set of fixed effects, using either cluster-robust inference or a wild cluster bootstrap.[19] Second, following Roth (2022), we assess the power of this test to detect an economically meaningful violation of parallel trends. Specifically, we calculate the probability of detecting a linear pre-trend large enough, if continued into the transition year, to generate the observed year-0 increase in exits. This probability exceeds 92% under the baseline fixed effects and 99% under the column (6) fixed effects for both outcomes.[20] Thus, we find neither evidence of differential pre-trends nor evidence that the pre-trend tests lack power to detect a linear divergence large enough to explain the estimated treatment effect.

[19] Under the baseline fixed effects, $F(2, 35) = 0.81$ ($p = 0.451$) for $Exit$ and $F(2, 35) = 1.04$ ($p = 0.366$) for $Exit_{age \leq 60}$. The corresponding wild cluster bootstrap p-values are 0.580 and 0.464 under the baseline specification and 0.337 and 0.245 under the column (6) specification. The test asks whether exit rates at $k = -3$, $k = -2$, and $k = -1$ are equal to one another and is therefore invariant to the normalization of the plotted coefficients.

[20] The probabilities are 92.4% ($Exit$) and 94.8% ($Exit_{age \leq 60}$) under the baseline fixed effects, and 99.7% and 99.9% under the column (6) fixed effects.

3.3 Robustness

In this section we discuss potential concerns about our empirical design. Several are already addressed by the fixed effects in our tightest specification, column (6) of Table 3. Agency×cohort FE absorb persistent differences across agencies, so the estimate is not driven by agencies with chronically high turnover. Occupation×year×cohort FE absorb shocks to the outside market for an occupation, such as a surge in private demand for lawyers in a given year. Cohort×pre-pay and cohort×pre-tenure FE compare regulators who occupied the same position in the pre-reform pay and tenure distributions, and education×year×cohort FE compare regulators with the same schooling – the one dimension along which treated and control employees differ before the reform. Manager×year×cohort and age-bin×year×cohort FE absorb shocks that fall differentially on supervisors and on employees approaching retirement eligibility, and city×year×cohort FE absorb local economic conditions, such as a booming financial center. Identification therefore comes from regulators in the same occupation, city, and year, with the same education, rank, age, pre-reform pay, and tenure, who differ only in whether their agency had transitioned to performance pay. The rest of this section addresses the concerns that fixed effects cannot resolve.

First, our findings may be driven by a single event that coincides with unusually high exit rates at the treated agency. We therefore re-estimate Equation 1 dropping one unit at a time, under the saturated fixed effect specification of column (6) of Table 3. Table A.3, Panel A, removes one reform cohort in its entirety: column (1) is the full seven-cohort sample, and columns (2) through (8) each drop a single transition event. As a complementary approach, Panel B drops one agency from the sample, both as the treated agency in its own cohort and as a comparison agency in every other cohort's event window. *Treated*×*Post* remains positive and statistically significant under every exclusion, ranging from 0.025 to 0.048 against a full-sample estimate of 0.042. The lowest value comes from dropping the FDIC, and even there the estimated increase in exits is 31% of the sample mean.

A concrete possibility is that a transition to P4P is correlated with hiring sprees in the in-

dustries these agencies regulate. If Congress grants independent pay-setting authority when an agency is losing talent to a strong private market, then the exit effect could be partially attributed to stronger demand from the private sector. To examine this possibility, we test in Table A.4 whether improvements in outside options predict when agencies adopt. We construct six agency-year measures of private sector outside options, described in Appendix A.3: pay offered to new hires at firms that absorb departing regulators; the same measure for peer firms in the same local labor markets and industries; and staff expense per employee and total staff expense at both publicly traded destination firms and a broader state-by-industry comparison group. Across all six measures, changes in outside options do not predict the timing of transition. None of the coefficients is statistically significant, and their signs run against the endogenous-transition story, which requires an improving outside option to raise the probability of transition: four of the six coefficients are zero or negative, and the two largest in absolute value are both negative. Thus, the data do not support the view that agencies adopt performance pay when the private market for their regulators improves.

A separate concern is that the statistical significance of our main result may be sensitive to our choice of clustering at the agency×cohort level. In Table A.5, Panel A, we assess alternative approaches to clustering and inference. Column (1) clusters at the agency level, permitting arbitrary correlation across cohorts within an agency. Column (2) uses two-way clustering by agency and year, additionally allowing for common shocks across agencies within a given year. Column (3) reports wild cluster bootstrap inference at the agency level using Webb weights and 999 replications (Roodman et al., 2019), which is designed to improve inference when the number of clusters is small. The estimated coefficient is 0.031 under all three approaches and remains statistically significant throughout, including a wild-bootstrap p-value of 0.024.[21]

We further study clustering using the score-variance test of MacKinnon, Nielsen, and Webb (2023), which evaluates whether a candidate clustering structure adequately captures

[21] Note that the bootstrap procedure produces a p-value rather than a corresponding standard error.

the dependence relevant for inference relative to a coarser nested alternative. We implement their sequential procedure over the hierarchy of no clustering, agency×cohort clustering, and agency clustering. Given the small number of agencies, we rely on wild-bootstrap p-values. Table A.5, Panel B, reports the results. We first strongly reject no clustering in favor of agency×cohort clustering, indicating economically relevant dependence within agency×cohort cells. We then fail to reject agency×cohort clustering against the coarser agency-level alternative.[22] Thus, allowing for additional dependence across cohorts within the same agency does not materially change the score variance relevant for inference. We therefore view agency×cohort as our preferred level of clustering.

A final concern is that the share of treated employees varies across cohorts, ranging from 6.9% in the 1993 cohort to 53.1% in the 2003 cohort. As Wing, Freedman, and Hollingsworth (2024) show, such differences can cause the stacked estimator to place different implicit weights on treated and control observations across cohorts. We therefore apply alternative sample weights, such that treated and control observations receive the same relative cohort weights. Table A.6 reports the results. The weighted estimates are very similar to, and if anything slightly larger than, our baseline estimates. In the most saturated specification, the estimate increases from 0.042 to 0.044, and all six estimates remain significant at the 1% level. Thus, differences in cohort composition do not account for our findings.

In sum, the main result is not driven by any single event, clustering scheme, or differences across cohorts, and improvements in outside options do not predict when agencies transition to performance pay. We cannot fully rule out that the reforms respond to conditions in the regulated industries. But such a shock would have to raise exits at the treated agency while sparing control agencies that regulate the same financial sector, recruit from the same labor pool, and employ regulators in the same occupations and cities; leave pre-reform pay, tenure, and exit rates balanced across the two groups; produce no divergence in the three years before transition, then a jump in the transition year followed by positive exit effects

[22] The wild-bootstrap P-values for the two steps are below 0.000 and 0.268, respectively.

in every remaining year of the window; and repeat all of this across seven transition events spanning a quarter century. The executive pay reform we turn to next provides a further check, because it applied simultaneously across hundreds of agencies and cannot be traced to conditions in any single regulated industry.

3.4 Evidence from executive pay

In this section, we turn to a separate reform affecting a different population: federal executives, known in government parlance as the Senior Executive Service (SES). While our main analysis focuses on financial agencies, this section offers an independent setting in which performance pay was introduced at scale, and shows that the link between incentives and exits holds beyond our core sample. Thus, it provides out-of-sample evidence on the impact of performance pay on turnover. Moreover, this is an extension of economic interest, given the pivotal role these officials play in federal governance.[23]

Created in 1979 under the Civil Service Reform Act, the SES was designed to attract and retain senior managers just below Presidential appointees. These executives link political leadership to the federal workforce: for instance, while the EPA Administrator is a political appointee, more than 200 career executives manage its 15,000 employees. At inception, SES pay was bounded between 120% of GS-15 and 100% of Executive Level IV (EX-IV). Raises were nearly automatic and uniform. By the late 1990s, two problems emerged: the absence of genuine pay-for-performance, and pay compression, as the SES ceiling did not keep pace with GS-15 increases. A reform in 2004 addressed both issues. Agencies were required to implement OPM-certified performance appraisal systems, after which their executive pay cap rose substantially to EX-III or EX-II. Automatic raises were replaced with performance-based adjustments, creating scope for meaningful distinctions across executives.

[23] An earlier version of the paper reported another external validation. In 2004 the Federal Aviation Administration and the National Air Traffic Controllers Association agreed to expand the share of air traffic controllers whose pay was tied to performance. We found that, following the reform, exit rates among treated employees rose substantially. Because the FAA reform treated some employees and not others within a single agency, exits are identified within agency and cannot be attributed to agency-wide events such as downsizing or a change in leadership.

The 2004 executive pay reform provides a sharp shock to executive compensation. Before the reform, SES pay was compressed and performance distinctions were muted. The reform simultaneously raised the pay ceiling and tied raises explicitly to performance evaluations, thereby creating meaningful dispersion in realized compensation. We use this reform to study the impact of performance pay on exits. Based on the same comprehensive payroll records, we now focus on 23,763 unique executives across 397 agencies (156,634 executive×year observations). Executives are on average 51.5 years old, have 16.5 years of tenure, and earn $212,000 with annual growth of 1.3%. Turnover is 10.8%, and education levels are high (96.6% college, 75.4% postgraduate; see Table A.7). Similar to the pay reforms among financial agencies, the executive pay reform raised the pay range substantially: the upper bound was 12% above the lower bound before the reform, but 40% afterwards. Variation in pay also increased, with the standard deviation of pay more than doubling. These patterns confirm that pay-for-performance was implemented at scale, raising ceilings and generating heterogeneity in realized pay.

Finally, we examine how the pay reform affected executive exits. Our empirical strategy closely follows the framework used for financial agencies (Section 3.1), but with one important distinction: whereas the previous analysis exploits staggered transition to pay-for-performance across cohorts, here we study a single system-wide policy shock. Accordingly, we estimate the following difference-in-differences specification:

$$Exit_{i,t+1} = \beta \cdot Post_t \cdot Treated_i + \alpha_{o,t} + \alpha_a + \varepsilon_{i,t}. \tag{3}$$

Here, $Exit_{i,t+1} = 1$ if executive i leaves the government at time $t+1$, and $Post_t = 1$ for years $t \geq 2004$. We focus on the period 2001-2007, forming a symmetric ±3-year window around the reform. $Treated_i = 1$ for executives who were part of the Senior Executive Service (SES) pay scale prior to the reform. Because the SES pay reform applied only to agencies within the SES system, our control group consists of agencies that have never been part of SES and instead operated under independent pay systems for their managerial staff.

We further exclude agencies that were created or dissolved within ±5 years of the reform, since openings and closures can induce abnormal turnover. This restriction yields a balanced sample of 26 agencies (see Appendix A.4). Within those agencies, we limit the control group to employees in managerial roles prior to the SES reform, to ensure comparability with the treated executives. All specifications include agency fixed effects (α_a) and occupation×year fixed effects ($\alpha_{o,t}$), and we progressively add fixed effects for pre-reform pay and tenure, education×year, age×year, and location×year. Standard errors are clustered at the agency level. The coefficient of interest, β, captures the differential change in exit rates among treated executives after the reform, relative to a similarly high-ranked control group.

Table 4 presents the results. Column (1) reports the baseline specification. Column (2) adds pre-pay and pre-tenure quintile fixed effects, column (3) adds education×year fixed effects, column (4) adds age-bin×year fixed effects, and column (5) adds city×year fixed effects to account for spatial variation in employment opportunities. We find that treated executives exhibit a 3.5–3.7 percentage point increase in their probability of leaving the government relative to comparable executives in control agencies. This effect is economically large, corresponding to roughly a 35% increase relative to the sample mean, and remains stable across progressively stringent specifications. The magnitude and direction of the estimates are closely aligned with those obtained for financial regulators in Section 3.2, underscoring how the response to pay-for-performance reforms is consistent across different institutional settings.

Taken together, these results indicate that the 2004 executive pay reform substantially increased turnover among senior government officials, mirroring the pattern observed in financial agencies. The consistency of the findings across two distinct institutional contexts strengthens the causal interpretation: the SES reform applied simultaneously across hundreds of agencies with diverse missions, making it unlikely that the effect is driven by idiosyncratic factors at any single organization. Moreover, the fact that a different reform produced nearly identical responses suggests that our findings generalize beyond financial

regulators. This broader applicability motivates the next section, which examines the mechanisms underlying effort, exit, and selection under performance pay.

4 Channels

In this section we turn to study why exits rose. We show that performance pay operates through two broad channels: it improves what regulators can earn outside government, and it weakens the appeal of staying. Concretely, we present five tests in sections Section 4.1 through Section 4.5, and discuss their implications in Section 4.6.

4.1 Effort

A natural first step is to ask whether the reforms changed regulators' effort within the government. The performance pay systems were designed to link compensation more directly to measurable outcomes. If they were effective, we should see an increase in observable productivity among employees who remained in government service. To test this, we exploit the new regulator–level dataset on rulemaking activity, which records both the number of regulatory documents to which each individual contributed and an indicator for whether the regulator participated in at least one new rulemaking draft in a given year. These data allow us to quantify the response of regulators to the transition to performance pay in real time. The test fits within the broader literature on incentives and multitasking, which predicts that agents reallocate effort toward observable and contractible tasks once rewards become performance-contingent (Holmstrom and Milgrom, 1991; Prendergast, 1999).

Because the Federal Register began reporting drafter names in 2000, this test rests on the three cohorts whose event windows fall inside the data – 2002, 2003, and 2006. Using the group of 743 regulators we identify in rulemaking documents, we construct a person-year panel over the seven-year window around the transition to P4P for any rulemaker observed at least once in the pre-transition period.

Table 5 reports the results. Columns (1)–(3) use the number of regulatory documents (#*RegDoc*) as the dependent variable, while Columns (4)–(6) use an indicator for working on at least one rule ($\mathbb{1}(RegDoc)$). We use a Poisson model for the former and OLS for the latter, because the number of rules is a highly skewed count with a large mass at zero. We find that treated regulators became more productive after the transition to P4P. The coefficient on *Treated*×*Post* ranges from 0.54 to 0.58 in the Poisson regressions and from 0.13 to 0.14 in the OLS specifications. The magnitude is economically large: the propensity to produce a new regulatory document rises by 58–63% relative to the sample mean. This is consistent with a substantial increase in effort among employees who remained in government service following the implementation of performance pay reforms.

Note that our tightest specifications in Columns (3) and (6) carry the same fixed effects as column (6) of Table 3. Cohort×Occupation×Year fixed effects hold the occupational mix fixed and Cohort×Education×Year fixed effects compare regulators with the same schooling, so a treated agency does not appear more productive merely because its workforce is differently credentialed. The remaining interactions absorb managerial practices, local economic conditions, and pre-reform standing in the pay and tenure distribution, as described in Section 3.3.

Finally, we show that the effect holds within employee. Table A.10 adds cohort×regulator fixed effects, thus comparing each regulator's own rulemaking activity before and after the transition to P4P. The estimates are similar to those without regulator fixed effects. This suggests that performance pay does not simply replace regulators with more productive hires, but rather induces greater effort among those who stay.

In sum, performance pay raised measured output. Treated regulators produced substantially more rules, consistent with stronger incentives to deliver observable results.[24] The reforms thus achieved one of their intended objectives: greater effort among those who stayed. At the same time, the increase in effort provides an important backdrop for understanding

[24] In untabulated results, we find that P4P transition increases the total number of regulatory documents produced by the agency, but has no meaningful impact on the number of "coauthors" per document.

why exits rose. Higher effort can erode the intrinsic appeal of public service, increase income risk, and raise the potential salary in the private sector – factors that together contribute to greater turnover. We will examine those ripple effects in subsequent sections.

4.2 Private sector pay

In this section, we ask whether performance pay improved what regulators can earn outside government. We proceed in two steps: whether exits are concentrated among the regulators with the most to gain from leaving, and whether those who leave command higher pay.

To examine how performance pay shapes private sector opportunities, we turn to our agency-occupation measure of outside pay. We compute regulator-level measure of *Pay upside*, defined as what the private market pays workers in a regulator's occupation (averaged over the three pre-reform years) less that regulator's own pre-reform salary, both expressed in 2023 dollars. We split employees by the sample median and estimate our primary specification for each subsample. Table 6 shows that the exit response is concentrated among regulators with high pay upside. The effect of performance pay is 0.011 in the low-upside group compared with 0.051 in the high-upside group, corresponding to 18% and 55% of their respective mean exit rates. The difference between the two estimates is significant at the 1% level. Thus, regulators who stood to earn substantially more outside government before the reform account for nearly all of the subsequent increase in exits, whereas those with relatively little financial gain from leaving respond only weakly. This pattern is consistent with performance pay making regulators more responsive to the private sector opportunities already available to them.

We next examine whether these stronger incentives to leave are reflected in the opportunities regulators realize *ex post*. Using Revelio Labs, we identify regulators who leave government service for a private sector position and examine compensation in their first post-government job. The outcomes are starting base salary; starting total compensation, which includes base salary and performance-related compensation; and performance pay, defined

as the portion of total compensation beyond base salary. All outcomes are expressed in logs. Similar to Equation 1, the main independent variable is *Treated* × *Post*. All specifications include cohort-by-agency and cohort-by-exit-year fixed effects.

The results in Table 7, Panel A, indicate a sizable increase in outside pay for treated regulators. Relative to a control group of leavers, treated regulators earned 38% ($e^{0.322}-1$) higher starting salaries (column (1)) and 53% ($e^{0.425}-1$) higher total compensation (column (2)). Even when isolating performance-related pay, they earned about 29% ($e^{0.252}-1$) more (column (3)). These findings suggest that stronger incentives inside government not only raised effort but also enhanced the market value of regulators' skills. By saturating the model with cohort-by-agency and cohort-by-exit-year fixed effects, we rule out alternative explanations such as agency-specific pay premia, exit-year shocks (e.g., macroeconomic conditions), or stable differences in the types of employees leaving particular agencies. Identification therefore comes from within-agency, within-cohort comparisons of treated and control employees before and after the reform.[25]

What explains these gains? One possibility is that greater effort builds transferable human capital, as regulators who draft more rules acquire specialized skills that regulated firms value.[26] Another possibility is informational. Before the reform, pay moved largely with tenure, so an outside employer could not distinguish two regulators with the same title and the same years of service; performance pay reveals a regulator's type, letting the market price them accordingly. While the two explanations are difficult to disentangle, Panel B provides suggestive evidence. We estimate a specification similar to Panel A, except that we also interact $Treated \times Post$ with two characteristics: pre-reform tenure (column (1)), standardized to mean zero and unit variance, and an indicator for pre-reform education

[25] These gains do not reflect a broadly improving market for regulators. As shown in Section 3.3, movements in agency-level outside options do not predict when agencies adopt performance pay. Moreover, the cohort-by-exit-year fixed effects compare regulators who left government in the same year, absorbing any common change in hiring conditions at the time of exit. The estimates therefore capture a gain to the individual regulator, not a rising tide.

[26] This channel maps to common ideas in the revolving door literature (Kalmenovitz, Vij, and Xiao, 2025): *schooling*, whereby regulators acquire skills that raise their private sector value, and *regulatory capture*, whereby regulators take actions that benefit prospective employers.

below a postgraduate degree (column (2)). We further include all lower-order terms (the characteristic itself and its interactions with *Treated* and *Post*). We find that the gain is larger for regulators with less pre-reform tenure (column (1)) and for those without a postgraduate degree (column (2)). A one-standard-deviation increase in pre-reform tenure reduces the treatment effect by about 41%, while the treatment effect is more than twice as large for regulators without a postgraduate degree. This pattern is more consistent with employer learning: the reform raises outside pay most for workers about whom the private market had less prior information.[27]

4.3 Income risk

Having established that effort increased, we next examine how the reform affected the distribution of pay inside the government. As shown in Figure 1, the variation in salaries rose sharply after the reform, consistent with a stronger link between compensation and performance. We now investigate this in greater depth, since this pattern has two important implications. First, as mentioned in Section 2.2, greater pay dispersion provides indirect evidence of a stronger pay-for-performance mechanism. Second, expected volatility in earnings may weaken the public sector preference ex-ante, and thus contribute to rising exit rates. We therefore estimate a version of Equation 1 with a set of annual pay adjustment variables.[28]

Table 8 reports the results. The dependent variable in columns (1)–(2) is the log of pay. In columns (3)–(4), the dependent variable is an indicator equal to one if an employee is promoted to senior rank, defined as positions within the EX, ES, EO, EM, CM, OE, SL, SO, SS, VE, WL, WS, XL, or XS pay plans. On average, employees promoted to senior rank experience a 10.4% increase in real salary. Conversely, columns (5)–(6) use an indicator for large real pay cuts, defined as real pay growth at or below the 5th percentile of its pre-reform distribution, that is, a decline in inflation-adjusted pay of at least 3.03%.

[27]The fact that exits rise immediately upon transition, too soon for much skill to accumulate, is also consistent with employer learning.

[28]Since here pay is an outcome, we do not control for pre-reform pay.

These outcomes capture extreme realizations of performance-based pay. The odd-numbered columns use our baseline specification, while the even-numbered columns use the more stringent specification that additionally includes education×year, manager×year, age-bin×year, and city×year fixed effects.

The results are consistent with stronger performance incentives. The probability of receiving a promotion to senior rank rises by 0.8 percentage points, and the likelihood of experiencing a large real-pay cut increases by 2.4 percentage points, roughly 178% and 73% of their respective sample means. At the same time, we find no significant change in the level of pay: the coefficient on *Treated*×*Post* ranges between 2.8 and 3.3% and is statistically indistinguishable from zero.[29] Thus, the reform primarily affected the dispersion rather than the level of compensation. In economic terms, the introduction of performance pay heightened the stakes of annual evaluations: high-performing regulators were more likely to be rewarded, while those with weaker evaluations faced tangible financial penalties.

In sum, we document greater pay volatility following the performance pay reform, increasing both the upside and downside risk of pay. For some regulators, the heightened volatility undermines the valuable stability of government employment. Viewed in conjunction with our previous findings, this set of results sharpens the fundamental trade-off triggered by performance pay: it can boost effort among stayers, while encouraging exits among those unwilling to bear higher income risk, as it weakens their public sector preference.[30]

4.4 Aversion to government evaluation

In the public sector, performance is difficult to measure and tasks are often multi-dimensional and non-verifiable. Because of that, performance pay can backfire. If regulators distrust the new evaluation process and perceive it as opaque or poorly aligned with effort, their

[29] In untabulated results, we find no impact on pay growth – defined as current pay divided by lagged pay minus 1.

[30] Income volatility can also push out more risk-averse regulators. While this mechanism may contribute to turnover, the observed rise in private sector wages and the fact that many leavers likely move to higher-risk private positions suggest that risk aversion alone cannot explain the overall pattern.

preference for the public sector is weakened. As they feel disadvantaged under the new system, they pursue private sector opportunities, where evaluation mechanisms may often be viewed as more transparent and merit-based. We develop two empirical measures capturing distinct sources of evaluation aversion. For each measure, we divide the sample into high- and low-aversion groups and re-estimate our baseline specification (Equation 1) separately for each group. The results, reported in Table 9, show that the increase in exits is concentrated among employees more averse to the government's performance evaluation process.

Our first test, motivated by the literature on relative performance evaluation (e.g., Jayaraman et al., 2021), is based on the number of comparable peers. Intuitively, when many peers perform similar tasks, the supervisor can better filter out idiosyncratic shocks and conduct more precise evaluations (Hölmstrom, 1979; Holmstrom, 1982). Thus, evaluations are expected to be noisier when an employee has few peers and more precise when many comparable colleagues exist. We define peers as employees working in the same office and occupation one year before reform, for example, attorneys in the SEC's Philadelphia office. We split the sample at the cohort-agency median number of peers and re-estimate our baseline specification separately for each group. As shown in columns (1)–(2) of Table 9, the effect of performance pay on exits is substantially larger among employees with fewer peers (0.039) than among those with many peers (0.023), with the difference statistically significant at the 1% level. These results suggest that noisy performance assessments amplify the effects of incentive pay on exits.

Our second measure is based on the Federal Employee Viewpoint Survey (FEVS), a large-scale survey of federal employees. We focus on Question 32:[31]

> *"In my most recent performance appraisal, I understood what I had to do to be rated at different performance levels (for example, Fully Successful, Outstanding)."*

This item captures whether employees perceive evaluation standards as clear. Optional

[31] Survey results are available online (https://www.opm.gov/fevs/public-data-file/).

responses were Strongly Disagree; Disagree; Neither Agree nor Disagree; Agree; and Strongly Agree. Because it was first included in 2008 (after the last cohort transitioned to P4P), we use that year to build a cross-sectional measure of post-reform transparency. For each agency, we calculate the share of employees who disagreed or strongly disagreed with the statement. A greater share indicates that employees view the appraisal criteria as uncertain. The median disagreement share across the agencies in our sample is 22.24%. We classify agencies with above-median disagreement as opaque and those below the median as transparent. The opaque agencies are the SEC, OCC, FinCEN, and OFHEO (97,637 cohort-regulator-year observations), and the transparent agencies are the CFTC, NCUA, OTS, and IRS (65,594 observations).[32] It is important to note that survey responses were collected in 2008, well after some agencies in our sample transitioned to performance pay. As a result, it may be a noisy proxy for the transparency of performance evaluation systems at the time of the reform.

With this caveat in mind, we re-estimate Equation 1 separately for each group. Columns (3)–(4) report the results. In opaque agencies, the treatment effect is positive but not statistically distinguishable from zero (0.028), while in transparent agencies it is negative and similarly indistinguishable from zero (-0.009). However, the difference between the two estimates is significant at the 10% level ($p = 0.09$). These findings indicate that exits were concentrated in agencies where employees are dissatisfied with the performance evaluation process. Note that this is an ex-post measure based on the perceptions of those who remained, which likely attenuates the effect if the most dissatisfied employees already have exited.

Taken together, the analyses suggest that aversion toward the government's evaluation process amplified exits. Regulators were most likely to leave when a small number of peers made evaluations noisier or when performance appraisals within the government were perceived as opaque. These patterns point to a common mechanism: distrust toward the gov-

[32]The FDIC and the Farm Credit Administration did not participate in the survey, together eliminating 53,387 observations from the primary sample (49,795 from the FDIC and 3,592 from the Farm Credit Administration).

ernment's evaluation system weakened the intrinsic attachment to public service by lowering the perceived fairness and credibility of performance assessments. As a result, regulators sought environments where performance metrics were perceived as clearer and more credible. This dynamic highlights how performance pay, by exposing regulators to ambiguous evaluations, eroded the intrinsic preference for public service and thus lowered the barrier to exit.

4.5 Weak ex-ante attachment

Our earlier findings suggest that performance pay weakened the intrinsic motivation to serve in the public sector. If so, the employees who respond most strongly should be those whose attachment was weak to begin with – *prior* to the reform. We proxy for that ex-ante attachment with the state of the labor market at the moment the employee joined the government. The logic is one of revealed preference: an employee who entered when private opportunities were plentiful gave up a great deal to serve, whereas one who entered when the market was thin gave up little. The three splits reported in Table 10 capture this idea at increasing levels of resolution, and each is determined years before the reform. For each, we divide the sample in two and re-estimate Equation 1 separately on the two halves.

Columns (1)-(2) use the aggregate business cycle: whether the employee entered during a recession, dated using official NBER recession years. Recession hires are more likely to have joined for job security in the face of weak private prospects than out of belief in the agency's mission. The exit response is concentrated among them (0.054), while the estimate for those hired during expansions is less than half as large and only marginally significant (0.025); the difference is statistically significant ($p = 0.01$).

Columns (3)-(6) refine the measure. Instead of the national business cycle, we use the labor market the employee actually faced on entry (Section 1.2.3). Columns (3)-(4) measure it at the agency level: what the private sector firms that hire the agency's leavers paid new hires in the employee's entry year. Employees who entered a weak market respond with

0.053, those who entered a strong one with 0.009, indistinguishable from zero. The difference is significant at the 1% level. Columns (5)-(6) measure it at the occupation level: what the private jobs that leavers from the employee's own occupation actually enter were paying that year. The pattern is unchanged, showing that employees who entered a weak market respond with 0.054 versus 0.009 for those who entered a strong one.

In sum, all three splits point the same way. The exit response is concentrated among employees who joined when private opportunities were scarce, and is far weaker (or even absent) among those who joined when they were plentiful. Our interpretation is that the two groups differ in how much they gave up to enter public service, and those who gave up more revealed a stronger intrinsic attachment to it. Performance pay therefore sorts on that attachment: it drives out the regulators who joined when the alternative was poor, and retains those who chose the agency over a strong private market.

4.6 Joint interpretation

To summarize, performance pay motivated regulators to exert more effort (Section 4.1). It also raised what they could earn outside: exits concentrated among the regulators with the most to gain from leaving, and those who left commanded higher private sector pay (Section 4.2). Inside the government, income risk increased (Section 4.3). In the cross-section, the regulators most averse to the government's evaluation system (Section 4.4) and those with the weakest ex-ante attachment to public service (Section 4.5) were the most likely to exit.

Combined, these findings highlight the broader implications of introducing performance pay for regulators, which we illustrate in Figure 3. Performance pay strengthens incentives and thus raises effort. This increases potential salary gains in the private sector, both by building human capital and by revealing to outside employers what an individual regulator is worth. At the same time, performance pay weakens the preference for public service through multiple channels. It increases income risk by introducing pay volatility, and it

sows distrust by installing performance evaluation systems perceived as opaque and unfair. The result is selective attrition: the reforms stimulate effort while simultaneously increasing turnover and altering the workforce composition. Regulators with more lucrative private sector opportunities are more likely to leave, while those who remain have stronger intrinsic attachment to public service and lower aversion to the government's evaluation process.

In an earlier version of the paper we formalized these mechanisms in a structural model of the regulator's joint decision on effort and exit, under a pay structure that combines tenure-based and performance-linked components subject to a government-wide pay cap. The model evaluates counterfactual pay policies built on the two levers a legislator controls: pay cap and performance component. Consistent with the trade-off documented throughout the paper, strengthening performance pay raises effort but also turnover, whereas relaxing the pay cap raises effort while improving retention, so combining the two can strengthen incentives without additional exits. The model and the counterfactual analyses are available as a Model Appendix on the authors' website.

5 Conclusions

This paper examines how performance pay affects incentives and retention in the public sector, using the staggered transition to pay-for-performance systems across U.S. financial regulatory agencies. We find that performance pay increases effort and productivity among regulators but also raises exits, particularly among regulators with the most to gain in the private sector. Two channels underlie this pattern: performance pay raises effort and improves what regulators can earn in the private sector, and it weakens the preference for government service by raising income risk and by installing evaluation systems regulators distrust. The resulting trade-off underscores the challenge of incentive design in the public sector: mechanisms that successfully stimulate productivity may inadvertently encourage resignations and change the composition of the regulatory workforce.

Our evidence also reveals meaningful heterogeneity in responses. Exits are concentrated among regulators who are more averse to government performance evaluation and those with weaker ex-ante attachment to public service. These patterns highlight that employees differ in their tolerance for evaluation and in their attachment to public service. Performance pay therefore reshapes both behavior and self-selection.

Several avenues remain open for future work. First, our analysis focuses on individual behavior and partial equilibrium effects; assessing the general equilibrium and welfare implications of performance pay remains an important next step. Second, richer data on evaluation processes and peer networks could clarify how bureaucratic assessment systems shape morale and effort allocation. Finally, extending this framework to other segments of the public workforce – such as educators, law enforcement, or healthcare administrators – would help determine the scope of the trade-off between incentives and retention.

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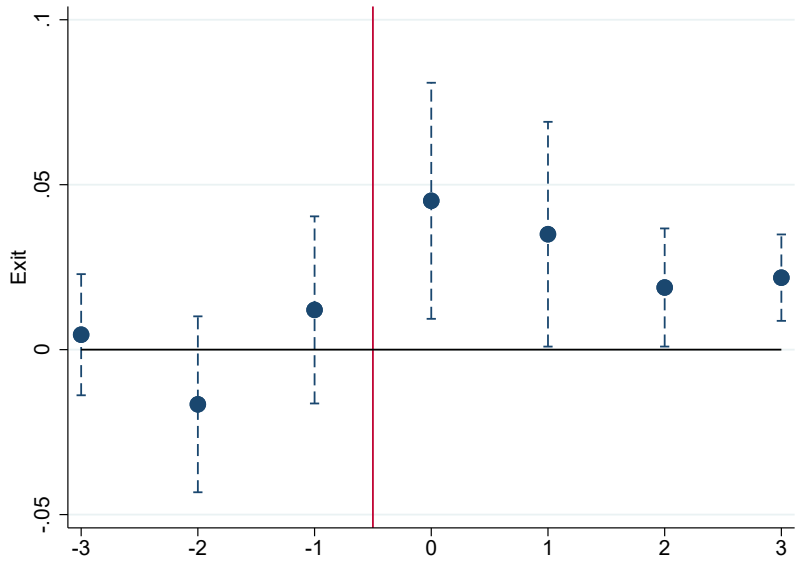
Figure 1: **Impact of Pay-for-Performance on Salary Dispersion**

The figure plots the inflation-adjusted salary distribution of financial regulatory agencies, three years before (red dotted line) and three years after (blue solid line) the transition to pay-for-performance reforms. The data are drawn from federal payroll records covering all employees in affected agencies, adjusted to constant 2023 dollars. Prior to reform, salary distributions were relatively compressed; afterward, they widened substantially. The standard deviation of pay rose by 22%, and the gap between the 90th and 10th percentiles expanded by more than 25%. The changes illustrate the shift toward merit-based pay differentiation. See Section 2.2.

Figure 2: **Dynamic Response of Financial Regulators to Performance Pay**

This figure traces differential exit rates around the transition to pay-for-performance by U.S. financial regulatory agencies. We report the coefficients (η_k) from the dynamic stacked difference-in-differences specification, $Y_{c,i,t+1} = \sum_{k=-3}^{+3} \eta_k \cdot Treated_{c,a} \times \mathbb{1}(EventTime_{c,t} = k) + FE + \varepsilon_{c,i,a,t}$, estimated separately for two outcomes Y: $Exit$ (all exits; Panel A) and $Exit_{age \leq 60}$ (exits net of retirements, i.e. leaving before age 60; Panel B). Following Miller (2023), coefficients are normalized so that the average of the pre-treatment coefficients equals zero; FE = the baseline fixed effects of column (1) of Table 3. Vertical bars denote 95% confidence intervals from standard errors clustered by cohort × agency. We also find no evidence of differential pre-trends: a joint test that the pre-treatment coefficients were constant before the reform yields $p = 0.45$ in Panel A and $p = 0.37$ in Panel B. Following Roth (2022), the test has 92% and 95% power, respectively, to detect a linear pre-trend sufficiently large to generate the observed year-0 treatment effect. See Section 3.2.

Panel A. *Exit*



Panel B. $Exit_{age \leq 60}$

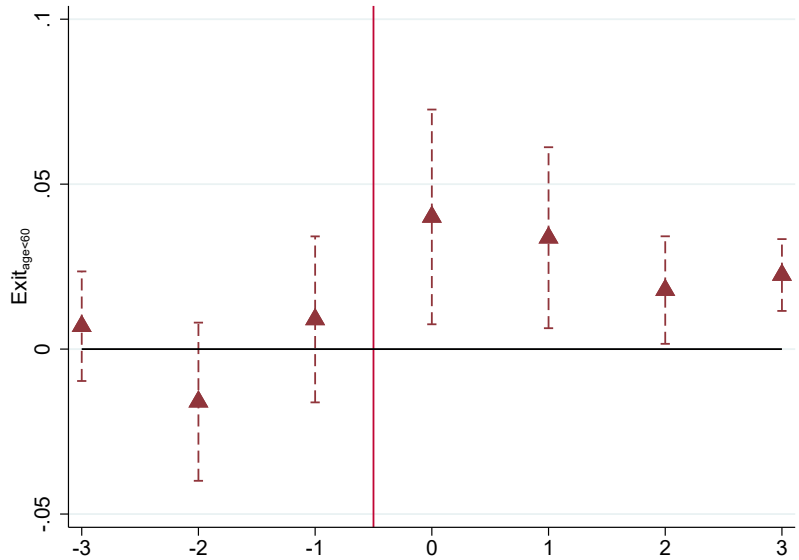


Figure 3: **Mechanisms Linking Performance Pay to Effort and Exit**

The figure illustrates the mechanisms connecting performance pay, effort, and turnover. Performance pay prompts regulators to exert greater effort, which raises potential pay in the private sector, as human capital accumulates and outside employers learn what an individual regulator is worth. At the same time, the shift to performance pay weakens the public sector preference for multiple reasons, such as aversion to government evaluation and disutility from effort and income volatility. The interaction of these forces explains the observed pattern of higher effort and more exits (average effect), and the greater impact on regulators with weaker attachment to public service and greater aversion to being evaluated inside the government (heterogeneous effect). See Section 4.6.

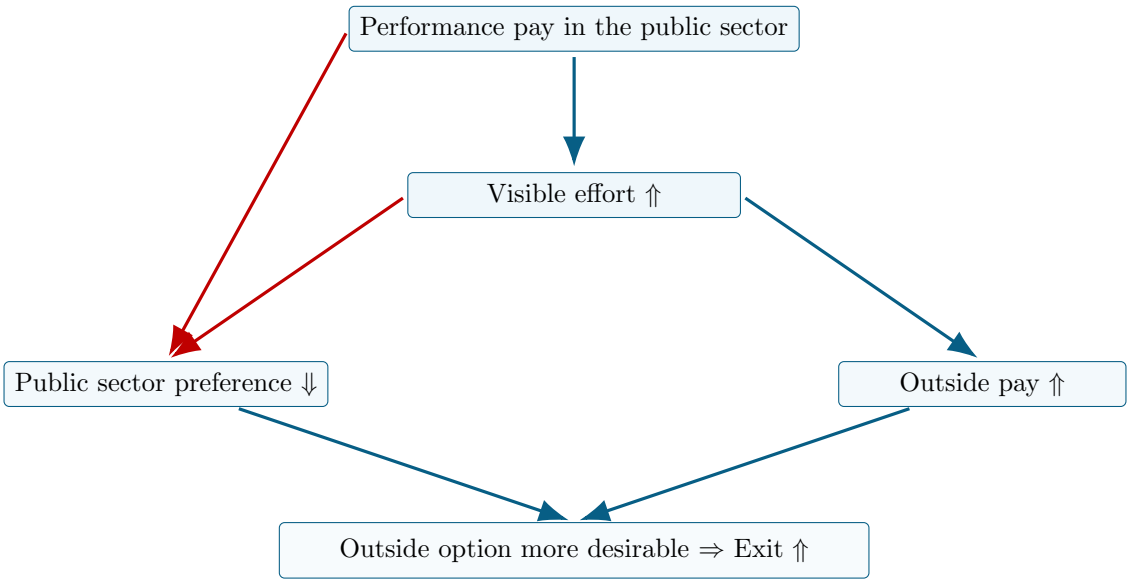


Table 1: **Career Trajectories of Financial Regulators**

Panel A uses the full career histories of the 31,055 financial regulators in our estimation sample, employed at 10 U.S. financial regulatory agencies between 1973 and 2013 (409,937 person-years). The unit of observation is the employee-year, and the sample is restricted to full-time employees with non-missing occupation, pay plan, education, salary, duty state, and supervisory status. Pay is the employee's annual base salary in constant 2023 USD. $Promotion_{senior} = 1$ if the employee is promoted to a senior rank. $Large\,Pay\,Cut = 1$ if real base pay falls by 3.03% or more relative to year $t-1$ (95th percentile). $Tenure$ is the number of years since the employee first appears in the federal personnel data. Age is imputed as the midpoint of the employee's reported five-year age band. $Manager = 1$ if the employee holds a supervisory position or a senior pay plan. $Exit = 1$ if the employee leaves the federal government in year $t+1$. $Retire = 1$ if the employee exits in year $t+1$ from an age band beginning at 60 or above. $College = 1$ ($Postgrad = 1$) if the employee has completed a college (postgraduate) degree. All indicator variables in Panel A are reported in percentage points. Panel B uses the 743 regulators engaged in rulemaking between 2000 and 2009. $\mathbb{1}(RegDoc) = 1$ if the regulator authored a proposed or final rule in year t, and $\#RegDoc$ is the number of new regulatory documents drafted conditional on non-zero. In Panel C, we focus on 1,013 financial regulators with available Revelio records who left the government. $ForProfit$, $Nonprofit$, $Academic$, and $PublicSector$ classify the destination employer, and $PublicFirm = 1$ if that employer is matched to a publicly listed firm. $Pay^{private,base}$ is base salary at the new position; $Pay^{private,total}$ is total compensation at the new position; $Pay^{private,P4P}$ is the share of total compensation that is not base salary; and $Pay^{gov't}$ is the regulator's federal base pay in the exit year. Private sector pay measures are winsorized at the 1st and 99th percentiles. All pay is in constant 2023 USD. Variable definitions are in Appendix A.1. See Section 2.1.

Statistic:	Mean	S.D.	Min	Max
Panel A. Government careers				
Pay (\$)	\$127,813	\$57,105	\$2,019	\$441,668
$Promotion_{senior}$ (0/1)	0.6	7.4	0.0	100.0
$Large\,Pay\,Cut$ (0/1)	4.0	19.7	0.0	100.0
$Tenure$ (years)	12.1	8.8	0.0	40.0
Age (years)	42.1	10.9	17.0	75.0
$Manager$ (0/1)	24.8	43.2	0.0	100.0
$Exit$ (0/1)	5.3	22.5	0.0	100.0
$Retire$ (0/1)	0.9	9.2	0.0	100.0
$College$ (0/1)	82.3	38.2	0.0	100.0
$Postgrad$ (0/1)	27.8	44.8	0.0	100.0
Panel B. Rulemaking activity				
$\mathbb{1}(RegDoc)$ (0/1)	16.9	37.5	0.0	100.0
$\#RegDoc$	1.9	1.4	1.0	14.0

Statistic:	Mean	S.D.	Min	Max
Panel C. Post-government careers				
$ForProfit$	0.81	0.39	0.00	1.00
$Nonprofit$	0.09	0.28	0.00	1.00
$Academic$	0.01	0.10	0.00	1.00
$PublicSector$	0.09	0.29	0.00	1.00
$PublicFirm$	0.47	0.50	0.00	1.00
$Pay^{private,base}$	\$193,367	\$115,258	\$26,747	\$678,112
$Pay^{private,total}$	\$283,293	\$217,883	\$30,306	\$1,330,150
$Pay^{private,P4P}$	0.23	0.16	0.00	0.70
$Pay^{gov't}$	\$134,738	\$46,983	\$33,816	\$320,637

Table 2: **Balancing Test**

This table compares pre-reform characteristics between treated and control groups. The treated group consists of employees in agencies that transitioned to pay-for-performance (P4P) during the sample period, while the control group includes agencies that had not yet implemented P4P in the same event window, as well as agencies that never changed pay regime. All variables are measured in the three-year period preceding each agency's transition. *Pay* denotes the employee's annual salary in constant 2023 USD. $Promotion_{senior} = 1$ if the employee receives promotion to a senior rank. *Large Pay Cut* = 1 for real pay cuts above the 95th percentile (3.03%). *Tenure* is the number of years since joining federal service. *Age* is the employee's age. *Manager* = 1 if the employee holds a managerial position in year t. *Exit* = 1 if the employee leaves the government in year $t+1$. *Retire* = 1 if the employee leaves the government in year $t+1$ at age 60 or older. *College* = 1 (*Postgrad* = 1) if the employee has completed a college (postgraduate) degree. Treated and control groups are comparable along observable dimensions prior to the reform, with the exception of college education. See Section 3.1.

	Control	Treated	Δ	p-value
Pay ($)	$109,295	$127,636	-$18,341	0.136
$Promotion_{Senior}$ (0/1)	0.003	0.002	0.001	0.418
Large Pay Cut (0/1)	0.055	0.039	0.016	0.686
Tenure (years)	12.000	12.538	-0.538	0.845
Age (years)	42.037	40.962	1.074	0.692
Manager (0/1)	0.209	0.248	-0.038	0.569
Exit (0/1)	0.071	0.071	0.000	0.995
Retire (0/1)	0.011	0.008	0.004	0.184
College (0/1)	0.801	0.870	-0.069	0.005
Postgrad (0/1)	0.316	0.283	0.034	0.689

Table 3: **Performance Pay and Exits**

Results from estimating Equation 1. The sample includes regulators in 10 financial agencies, organized by 7 cohorts of performance pay reforms, each cohort spanning a seven-year event window around the transition to performance pay. $Treated = 1$ for agencies transitioning to performance pay in that cohort, and $Post = 1$ in years following the transition. $Exit = 1$ if the regulator leaves government service in year $t + 1$; columns (7)-(8) decompose this into $Exit_{age \leq 60}$ (departures before age 60) and $Retire$ (departures at or above age 60). We gradually add fixed effects based on the regulator's agency, occupation, pre-reform tenure and pay (in quintiles), education (high school, college, or postgraduate degree), management position, age (five-year bins), and city. Effect (%Mean) is the coefficient on Treated×Post as a percentage of the sample mean of the respective outcome. Standard errors, clustered by agency×cohort, are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. The exit response to P4P survives the full set of fixed effects and is concentrated in departures before age 60. See Section 3.2.

Outcome:	$Exit$						$Exit_{age \leq 60}$	$Retire$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated×Post	0.031***	0.031***	0.032***	0.032***	0.037***	0.042***	0.038***	0.004***
	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)	(0.008)	(0.007)	(0.001)
Cohort×Agency FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort×Occupation×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort×Pre-Pay FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort×Pre-Tenure FE	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort×Education×Year FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Cohort×Manager×Year FE	No	No	No	Yes	Yes	Yes	Yes	Yes
Cohort×AgeBin×Year FE	No	No	No	No	Yes	Yes	Yes	Yes
Cohort×City×Year FE	No	No	No	No	No	Yes	Yes	Yes
R^2	0.026	0.036	0.036	0.037	0.063	0.070	0.062	0.226
N	231,784	231,784	231,784	231,784	231,761	229,768	229,768	229,768
Effect (%Mean)	40.04	40.19	41.06	40.55	47.92	54.38	57.26	36.55

Table 4: **Performance Pay and Exits: Out-of-Sample Evidence**

Results from estimating Equation 3, which examines the 2004 executive pay reform affecting executives – members of the Senior Executive Service (SES). $Exit = 1$ if the executive leaves the government in year $t+1$, $Post = 1$ for years $t \geq 2004$, and $Treated = 1$ for executives employed under the SES pay system prior to the reform. The control group consists of comparable managers in agencies that were not part of the SES system. We gradually add fixed effects based on the regulator's agency, occupation, pre-reform tenure and pay (in quintiles), education (high school, college, or postgraduate degree), age (five-year bins), and city. Effect (%Mean) is the coefficient on Treated×Post as a percentage of the sample mean of the respective outcome. Standard errors, clustered at the agency level, are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. The exit response is stable across specifications and closely matches the magnitude found for financial regulators in Table 3, providing out-of-sample evidence to the paper's main result. See Section 3.4.

Outcome:	*Exit*				
	(1)	(2)	(3)	(4)	(5)
Treated×Post	0.036**	0.036**	0.037**	0.035**	0.037**
	(0.018)	(0.018)	(0.018)	(0.018)	(0.017)
Agency FE	Yes	Yes	Yes	Yes	Yes
Occupation×Year FE	Yes	Yes	Yes	Yes	Yes
Pre-Pay FE	No	Yes	Yes	Yes	Yes
Pre-Tenure FE	No	Yes	Yes	Yes	Yes
Education×Year FE	No	No	Yes	Yes	Yes
AgeBin×Year FE	No	No	No	Yes	Yes
City×Year FE	No	No	No	No	Yes
R^2	0.017	0.017	0.017	0.044	0.043
N	34,924	34,924	34,924	34,922	33,972
Effect (%Mean)	34.68	34.68	36.10	34.60	36.54

Table 5: **Effect of Performance Pay on Rulemaking Activity**

Results from estimating Equation 1 with rulemaking outcomes. The sample is restricted to the 743 regulators who contributed to at least one rule and to the 2002, 2003, and 2006 reform cohorts. Columns (1)-(3) use a Poisson model for $\#RegDoc$, the number of new regulatory documents an individual worked on in a given year; and columns (4)-(6) use OLS for $\mathbb{1}(RegDoc)$, an indicator equal to 1 if the individual contributed to any new draft of a proposed or final rule that year. $Treated = 1$ for agencies transitioning to performance pay in that cohort, and $Post = 1$ in years following the transition. We gradually add fixed effects based on the regulator's agency, occupation, pre-reform tenure and pay (in quintiles), education (high school, college, or postgraduate degree), management position, age (five-year bins), and city. Effect (%Mean) is the coefficient on Treated×Post as a percentage of the sample mean of the respective outcome. Standard errors, clustered by agency×cohort, are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. Performance pay substantially increased rulemaking activity among regulators who remained in government, consistent with higher effort under stronger incentives. See Section 4.1.

Outcome:	$\#RegDoc$			$\mathbb{1}(RegDoc)$		
	(1)	(2)	(3)	(4)	(5)	(6)
Treated×Post	0.575***	0.536***	0.571***	0.127***	0.129***	0.138**
	(0.188)	(0.166)	(0.195)	(0.037)	(0.039)	(0.046)
Cohort×Agency FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort×Occupation×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort×Pre-Pay FE	No	Yes	Yes	No	Yes	Yes
Cohort×Pre-Tenure FE	No	Yes	Yes	No	Yes	Yes
Cohort×Education×Year FE	No	Yes	Yes	No	Yes	Yes
Cohort×Manager×Year	No	Yes	Yes	No	Yes	Yes
Cohort×AgeBin×Year FE	No	No	Yes	No	No	Yes
Cohort×City×Year FE	No	No	Yes	No	No	Yes
Model	Poisson	Poisson	Poisson	OLS	OLS	OLS
R^2				0.076	0.083	0.064
N	3,422	3,409	3,024	4,014	4,012	3,782
Effect (%Mean)				57.55	58.59	62.59

Table 6: **Pay Upside and the Exit Response to Performance Pay**

Results from estimating Equation 1, splitting the sample by the median *Pay upside*: the difference between the private market pay for the regulator's occupation and their own government salary, measured in the pre-reform period. $Treated = 1$ for agencies transitioning to performance pay in that cohort, and $Post = 1$ in years following the transition. $Exit = 1$ if a regulator leaves government service in year $t + 1$. All columns include fixed effects based on the regulator's agency, occupation, and pre-reform tenure and pay (in quintiles). Effect (%Mean) is the coefficient on Treated×Post as a percentage of the sample mean of the respective outcome. The p-value tests equality with the preceding column. Standard errors, clustered by agency×cohort, are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. The exit response is concentrated among regulators with the most to gain from leaving; those with little pay upside barely respond. See Section 4.2.

Outcome:	*Exit*	
Pay upside:	Low	High
	(1)	(2)
Treated×Post	0.011**	0.051***
	(0.005)	(0.013)
Cohort×Occupation×Year FE	Yes	Yes
Cohort×Agency FE	Yes	Yes
Cohort×Pre-Pay FE	Yes	Yes
Cohort×Pre-Tenure FE	Yes	Yes
R^2	0.032	0.041
N	112,962	113,020
Effect (%Mean)	17.56	55.19
p-value (vs. preceding column)		0.00

Table 7: **Performance Pay and the Value of the Outside Option**

Panel A. Average Effect. Results from a specification similar to Equation 1, estimated on regulators who exit government service and are matched to Revelio Labs career data, with government and first post-government pay both observed. $Treated = 1$ for agencies transitioning to performance pay in that cohort, and $Post = 1$ if the regulator's exit occurs after the transition. The outcomes are the logs of the leaver's first private sector base pay, total pay, and performance pay (defined as one minus the ratio between base and total). All columns include Cohort×Agency and Cohort×Exit Year fixed effects. Standard errors, clustered by agency×cohort, are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. Treated regulators earn substantially higher private sector pay upon exit, consistent with performance pay increasing the value of the outside option. See Section 4.2.

Outcome:	$Log(Pay^{private,base})$	$Log(Pay^{private,total})$	$Log(Pay^{private,P4P})$
	(1)	(2)	(3)
Treated×Post	0.322***	0.425***	0.252**
	(0.086)	(0.135)	(0.111)
Cohort×Agency	Yes	Yes	Yes
Cohort×Exit Year	Yes	Yes	Yes
R^2	0.263	0.200	0.045
N	1,199	1,199	1,161

Panel B. Heterogeneity. This table reports a specification similar to Panel A, column (1), except that we interact $Treated \times Post$ with the leaver's characteristics: pre-reform tenure (column (1)) and an indicator for pre-reform education below a postgraduate degree (column (2)). Pre-reform tenure is standardized to mean zero and unit variance. All lower-order terms (the proxy's main effect and its interactions with $Treated$ and $Post$) are included but not reported. Standard errors, clustered by agency×cohort, are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. The outside pay gain is largest for leavers with less tenure and for those without a postgraduate degree, consistent with employer learning: the reform has the greatest effect for workers about whom the market had less prior information. See Section 4.2.

Outcome:	$Log(Pay^{private,base})$	
Proxy:	Pre-reform tenure	Not postgraduate
	(1)	(2)
Treated×Post	0.300***	0.173
	(0.085)	(0.120)
Treated×Post×Proxy	-0.124**	0.270*
	(0.058)	(0.140)
Lower interaction terms	Yes	Yes
Cohort×Agency	Yes	Yes
Cohort×Exit Year	Yes	Yes
R^2	0.273	0.276
N	1,199	1,199

Table 8: **Effect of Performance Pay on Pay Volatility**

Results from a version of Equation 1 with pay-volatility outcomes: the log of annual real salary (columns (1)–(2)), an indicator for promotion to a senior rank (columns (3)–(4)), and an indicator for a large real pay cut, defined as pre-reform real pay growth at or below the 5th percentile of its pre-reform distribution (columns (5)–(6)). $Treated = 1$ for agencies transitioning to performance pay in that cohort, and $Post = 1$ in years following the transition. For each outcome, the first column reports our baseline specification (column (1) of Table 3) and the second column reports our tightest specification (column (6) of Table 3). Effect (%Mean) is the coefficient on Treated×Post as a percentage of the sample mean of the respective outcome (not applicable for $Log(Pay)$). Standard errors, clustered by agency×cohort, are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. Performance pay left the level of pay unchanged but widened its dispersion, substantially raising the likelihood of both a promotion to senior rank and a large pay cut. See Section 4.3.

	$Log(Pay)$		$Promotion_{senior}$		$Large\ Pay\ Cut$	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated×Post	0.033	0.028	0.009*	0.008*	0.036**	0.024**
	(0.038)	(0.034)	(0.005)	(0.005)	(0.013)	(0.009)
Cohort×Agency FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort×Occupation×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort×Education×Year FE	No	Yes	No	Yes	No	Yes
Cohort×Manager×Year FE	No	Yes	No	Yes	No	Yes
Cohort×AgeBin×Year FE	No	Yes	No	Yes	No	Yes
Cohort×City×Year FE	No	Yes	No	Yes	No	Yes
R^2	0.819	0.904	0.058	0.212	0.263	0.331
N	231,784	229,768	231,521	229,505	216,408	214,451
Effect (%Mean)			188.13	177.89	111.38	73.48

Table 9: **Aversion to Government Evaluation and Exit Behavior**

Results from estimating Equation 1, splitting the sample by two proxies for dissatisfaction with the government's performance evaluation system. Columns (1)-(2) use the number of peers – employees working in the same office×occupation one year before reform – as a proxy for appraisal precision. Employees with few (more) peers face noisier (more precise) evaluations. Columns (3)-(4) use the 2008 Federal Employee Viewpoint Survey to classify agencies as opaque (transparent) when a high (low) share of employees report that performance appraisal standards are unclear, capturing perceived opacity of government evaluations. $Treated = 1$ for agencies transitioning to performance pay in that cohort, and $Post = 1$ in years following the transition. $Exit = 1$ if a regulator leaves government service in year $t+1$. All columns include fixed effects based on the regulator's agency, occupation, and pre-reform tenure and pay (in quintiles). The p-value tests equality with the preceding column. Standard errors, clustered by agency×cohort, are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. The exit response is concentrated among regulators facing noisier or more opaque government evaluations, consistent with distrust of the evaluation process amplifying the effect of pay-for-performance. See Section 4.4.

Outcome:	*Exit*			
Measure:	#Peers		Employee Survey	
Sample:	Few	Many	Opaque	Transparent
	(1)	(2)	(3)	(4)
Treated×Post	0.039***	0.023***	0.028	-0.009
	(0.008)	(0.005)	(0.017)	(0.013)
Controls	Yes	Yes	Yes	Yes
Cohort×Agency FE	Yes	Yes	Yes	Yes
Cohort×Occupation×Year FE	Yes	Yes	Yes	Yes
Cohort×Pre-Pay FE	Yes	Yes	Yes	Yes
Cohort×Pre-Tenure FE	Yes	Yes	Yes	Yes
R^2	0.039	0.043	0.039	0.046
N	115,251	108,893	96,981	64,944
p-value (vs. preceding column)		0.00		0.09

Table 10: **Weak Ex-Ante Attachment and Exit Response to Performance Pay**

Results from estimating Equation 1, splitting the sample by three proxies for ex-ante attachment to public service, each fixed at the employee's entry and determined years before the reform. Columns (1)-(2) split by the aggregate business cycle: whether the employee entered during an NBER recession. Columns (3)-(4) split by the labor market the employee's agency actually faced at entry, measured as the average starting salary paid that year by the private sector firms that later hire the agency's leavers. Columns (5)-(6) refine this to the employee's own occupation, weighting private destinations by where leavers from that occupation actually land. $Treated = 1$ for agencies transitioning to performance pay in that cohort, and $Post = 1$ in years following the transition. $Exit = 1$ if a regulator leaves government service in year $t + 1$. All columns include fixed effects based on the regulator's agency, occupation, and pre-reform tenure and pay (in quintiles). The p-value tests equality with the preceding column. Standard errors, clustered by agency×cohort, are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. The exit response is concentrated among regulators who joined public service when private opportunities were scarce, and is weak or absent among those who joined during strong private markets – consistent with performance pay sorting on ex-ante attachment to public service. See Section 4.5.

Outcome:	*Exit*					
Measure:	Economic Environment		Labor Market at Entry (Destination)		Labor Market at Entry (Occupation)	
Sample:	Recession	Non-Recession	Weak	Strong	Weak	Strong
	(1)	(2)	(3)	(4)	(5)	(6)
Treated×Post	0.054***	0.025*	0.053***	0.009	0.054***	0.009
	(0.007)	(0.013)	(0.007)	(0.013)	(0.017)	(0.013)
Cohort×Agency FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort×Occupation×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort×Pre-Pay FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort×Pre-Tenure FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.048	0.049	0.056	0.041	0.048	0.039
N	67,995	104,934	86,439	83,327	69,891	70,007
p-value (vs. preceding column)		0.01		0.00		0.00

Internet Appendix

A.1 Variable Definitions

Treatment variables.

- *Treated* is an indicator equal to one if an agency transitions to pay-for-performance and zero otherwise (source: HR payroll).
- *Post* is an indicator equal to one for years after the transition to performance pay and zero otherwise (source: HR payroll).

Outcome variables.

- *Exit* is an indicator equal to one if the employee leaves government service in year $t+1$ (source: HR payroll).
- $Exit_{age \leq 60}$ is an indicator equal to one if the employee exits government service at $t+1$ and is 60 or younger (source: HR payroll).
- *Retire* is an indicator equal to one if the employee exits government service at $t+1$ and is older than 60 (source: HR payroll).
- $Promotion_{senior}$ is an indicator equal to one if the employee is promoted to a senior rank; senior rank includes positions within the EX, ES, EO, EM, CM, OE, SL, SO, SS, VE, WL, WS, XL, or XS pay plans (source: HR payroll).
- *Large Pay Cut* is an indicator equal to one if the employee experiences a real pay cut of at least 3.03%, corresponding to the 95th percentile of pre-reform real pay cuts (source: HR payroll).

Employee characteristics.

- *Pay* is the annual salary of the employee expressed in constant 2023 dollars (source: HR payroll).
- *Tenure* is the number of years since the employee joined the federal government (source: HR payroll).
- *AgeBin* is an indicator for the employee's age bin; age is reported in five-year categories (15–19, 20–24, 25–29, 30–34, 35–39, 40–44, 45–49, 50–54, 55–59, 60–64, 65–69, 70–74, 75+, unspecified) (source: HR payroll).
- *Age* is the employee age proxied by the midpoint of the reported age bin (source: HR payroll).
- *College* is an indicator equal to one if the employee has completed a college degree (source: HR payroll).
- *Postgrad* is an indicator equal to one if the employee has completed a postgraduate degree (source: HR payroll).

- *Education* is a categorical variable indicating the employee's education level: high school, college, or postgraduate degree (source: HR payroll).
- *Occupation* is a categorical variable indicating the employee's occupation (e.g., financial institution examining, general attorney, accounting, etc.); there are 193 unique occupations (source: HR payroll).
- *Manager* is an indicator equal to one if the employee holds a managerial position (i.e., supervision code < 8 in HR payroll) in year t (source: HR payroll).

Effort and rulemaking.

- $\#RegDoc$ is the number of new regulatory documents to which a regulator contributes in a given year (source: Federal Register).
- $\mathbb{1}(RegDoc)$ is an indicator equal to one if the regulator contributes to at least one new regulatory document in a given year (source: Federal Register).

Private sector compensation.

- $Pay^{private,base}$ is the base salary in the employee's first private sector job after leaving government (source: Revelio).
- $Pay^{private,total}$ is the total compensation in the employee's first private sector job after leaving government, including base salary and bonuses (source: Revelio).
- $Pay^{private,P4P}$ is the fraction of total private sector compensation not accounted for by base salary (source: Revelio).
- $Pay^{gov't}$ is the regulator's federal base salary in the year of exit from (or, for pre-government analyses, entry into) government service, in constant 2023 dollars (source: HR payroll).

Non-government employers.

- $ForProfit$, $Nonprofit$, $Academic$, and $PublicSector$ are indicators classifying the employee's non-government employer (in the first job after leaving government, or the last job before joining) as a for-profit firm, a nonprofit organization (excluding universities), an academic institution, or a non-financial government agency, respectively (source: Revelio).
- $PublicFirm$ is an indicator equal to one if that employer is matched to a publicly traded firm (source: Revelio).

Channel variables.

- *Opaque* is an indicator for agencies with opaque evaluation systems; for each agency, we compute the share of employees in the 2008 Federal Employee Viewpoint Survey who disagree or strongly disagree that performance appraisal standards are clear (Question 32), and classify agencies with above-median disagreement rates (22.24%) as opaque and those below the median as transparent (source: 2008 Federal Employee Viewpoint Survey).
- *#Peers* is the number of employees in the same office and occupation one year prior to performance pay reforms (source: HR payroll).
- *Recession Hire* is an indicator equal to one if the employee entered government service during a recession year (source: HR payroll and NBER).
- *Outside Option* is the compensation available to a regulator outside the government, constructed at the agency, occupation, and individual level; the construction of each measure is described in Appendix A.3 (source: Revelio & HR payroll).
- *Pay Upside* is what the private market pays workers in a regulator's occupation, averaged over the three pre-reform years, less that regulator's own pre-reform salary (source: Revelio & HR payroll).

A.2 Pay-for-Performance in Financial Agencies

In Section 1.1, we described the pay-for-performance reforms among financial agencies in broad strokes. In this section, we describe each performance pay system of each agency. The timing of reforms was tied to agency-specific statutory authority.[1] The earliest agency to transition (OCC) was uniquely able to experiment with performance-based adjustments due to a statutory exemption from the General Schedule dating back to 1974. The early-1990s wave followed the Financial Institutions Reform, Recovery, and Enforcement Act of 1989, which granted several regulators independent pay authority in response to the savings and loan crisis – a period of distress in the banking sector. The early-2000s wave was driven largely by pay parity concerns, as the SEC and CFTC remained on the GS system while other banking regulators had moved to performance pay. It coincided with a broader government-wide push to introduce more business-like principles and practices from the private sector.

Commodity Futures Trading Commission (CFTC) In October 2006, the CFTC began transitioning to a new performance pay system. In preparation for this transition, the agency moved in 2003 from the General Schedule (GS) pay plan to its own Commission-Track (CT) pay plan. Under the new pay-for-performance system, employees are grouped into five performance categories based on annual evaluations (U.S. Office of Personnel Management, 1998). Merit-based pay increases are awarded to employees in the higher performance categories, while those in the lowest category receive no increase. The new pay plan eliminated

[1] In the main text we further show that various proxies for private sector demand cannot predict the timing of P4P transition (see Section 3.3).

the automatic within-grade step increases, introducing instead a merit pay matrix. Each year, supervisors conduct performance appraisals to evaluate employees against established objectives and competency standards. These ratings are reviewed by higher-level managers and the human resources office. Employees can be placed anywhere within their assigned salary range, with increases tied both to the performance rating and to the employee's position relative to market benchmarks. Employees with the same performance rating in the same grade who are paid below the market rate receive larger percentage increases than those already above the benchmark. Employees in the lowest performance category do not receive salary increases.

Farm Credit Administration (FCA) The FCA transitioned to performance pay in 1993, by transitioning to the Variable Grade (VG) pay plan (Farm Credit Administration, Office of Inspector General, 2016). Each year, supervisors conduct performance appraisals to evaluate employees against established objectives and competency standards. These ratings are reviewed by higher-level managers and the human resources office. Employees can be placed anywhere within their assigned salary range, with increases tied both to the performance rating and to the employee's position relative to market benchmarks. Employees with the same performance rating in the same grade who are paid below the market rate receive larger percentage increases than those already above the benchmark. Employees in the lowest performance category do not receive salary increases.

Federal Deposit Insurance Corporation (FDIC) In 2003, the FDIC implemented a new pay scheme tying all pay raises to performance based on measurable objectives. It also launched the Corporate Success Award, to further differentiate annual merit-based pay adjustments for the entire workforce on the basis of performance (Federal Deposit Insurance Corporation, 2004). FDIC operates two separate performance systems. Non-managers must first earn a "meets expectations" rating to be eligible for performance-based increase, and are then placed into one of four pay groups based on their performance and "corporate contributions" relative to peers (U.S. Government Accountability Office, 2007). The top-level pay ratings are limited to 25% of the employees (National Treasury Employees Union, 2007). Managers and executives are rated in three categories: Meets Expectations, Marginal, and Does Not Meet Expectations (U.S. Government Accountability Office, 2007).

Financial Crimes Enforcement Network (FinCEN) FinCEN was established within the Department of the Treasury in 1990 and elevated to bureau status following the USA PATRIOT Act in 2001. During our sample period, FinCEN remained under the General Schedule (GS) pay system rather than transitioning to an independent pay-for-performance pay plan.

Internal Revenue Service (IRS) The IRS instituted a new performance management system, and the corresponding IR pay plan, in December 1999. We therefore treat 2000 as the first post-transition year. Under the IR system, employees are rated on four levels: Outstanding, Exceeded, Met, and Less than Met. Annual performance-based increases scale with the rating. For example, in 2007, those with an Outstanding rating received a 6.5% pay

increase, while those rated "Less than Met" received no merit increase (U.S. Office of Personnel Management, 2007). Note that many non-managers, primarily unionized employees, did not transition to the IR system (Rutzick, 2007). We classify individuals as affected by the pay reform if they were originally on a managerial pay plan (pay plans ES, EX, SL) or in occupations with high transition rates to the IR: Program Management (60%), Distribution Facilities & Storage Management (50%), Financial Management (45%), and Administrative Officer (41%). We restrict the IRS sample to these likely affected individuals and define treatment status at the agency level to maintain consistency with our classification for other agencies. For robustness, we verify that our main results remain virtually unchanged when we exclude the IRS altogether (see Table A.3). Additionally, in an untabulated test, we show that our results continue to hold if we focus on program managers and senior officials.

National Credit Union Administration (NCUA) The NCUA implemented performance pay in 1991, switching from the GS pay system to the Credit Union (CU) pay system. Under the CU regime, employees receive a performance rating score ranging from 0 to 300 to determine their pay increases. Employees with the same score receive the same percentage increase, while those rated "unsatisfactory"/"minimally successful" receive no increase (U.S. Office of Personnel Management, 2007).

Office of Federal Housing Enterprise Oversight (OFHEO) The OFHEO has operated a performance-based pay system since its inception in 1992. Employees are appraised on a five-level scale for each element in their performance plans. The standards define the midpoint, "fully successful", and specify what raters should examine to judge performance against that benchmark. Each element is scored, producing a total performance score. Merit increases are tied directly to the overall rating, so employees can anticipate their pay adjustment from the rating received (U.S. Office of Personnel Management, 2007).

Office of the Comptroller of the Currency (OCC). In 1981, the OCC became one of the first federal agencies to adopt pay-for-performance. At that time, most federal agencies still relied on incremental, across-the-board salary increases.[2] To implement this change, the OCC switched its own pay plan, known as CP (U.S. Office of Personnel Management, 2007). Under the OCC's performance pay system, each employee's performance is evaluated annually by their manager. To promote fairness and consistency, managers often consult with peers and human resources when assigning ratings. Employees are then placed into one of four performance categories, and only those in the top two categories are eligible for merit-based pay increases (U.S. Government Accountability Office, 2007).

Office of Thrift Supervision (OTS) The OTS implemented a performance pay system in 1991, shortly after its inception (U.S. Government Accountability Office, 2007).[3] Under the OTS pay system, employees were evaluated on a five-level performance rating scale. Pay increases were tied to these ratings, with managers retaining discretion to determine the

[2] See U.S. Merit Systems Protection Board (2007).

[3] Although the agency was officially created in 1989, our data indicate that it first employed staff in 1990.

specific raise within the allowable range for each category. For example, in 2006, OTS employees in the lowest two rating categories received no pay increase (U.S. Office of Personnel Management, 2007).

Securities and Exchange Commission (SEC) The SEC shifted to an independent, performance-based pay system in 2002 after Congress enacted the Pay Parity Act. In the process, the SEC switched from GS to its own SK/SO pay plan (U.S. Securities and Exchange Commission, 2002). Similar to the FDIC, the SEC has a separate system for managers and non-managers. Non-managers are first rated as acceptable or unacceptable by their supervisors. Those rated unacceptable do not receive annual pay adjustments. Employees rated acceptable then enter a second phase in which they are placed into four contribution categories ("highest quality," "high quality," "quality," "no significant contribution beyond acceptable"). Finally, compensation committees review the results of each employee and recommend merit increases (U.S. Office of Personnel Management, 2007). Managers are appraised under a separate performance system aligned with organizational goals, and senior officers have written performance plans for each fiscal year and required progress reviews, with ratings used to inform merit pay and awards (U.S. Securities and Exchange Commission, 2007; U.S. Government Accountability Office, 2007).

A.3 Measuring Outside Option

In this section, we describe how we construct the set of measures capturing the value of the outside option. Those are constructed at the agency, agency-occupation, and individual level, in annual frequency.

A.3.1 Agency level

Our first measure is at the agency level. We first match each of the 10 sample agencies to Revelio to obtain their company IDs. Several agencies occupy more than one: the IRS alone spans eleven, including the Taxpayer Advocate Service and IRS Criminal Investigation. We treat individuals recorded at the Office of Federal Housing Enterprise Oversight as Federal Housing Finance Agency employees, since it was absorbed into the FHFA in 2008 and Revelio users describe the predecessor and successor entities interchangeably.

We then extract every individual in Revelio who has ever worked for an agency, keeping those with at least one year of recorded tenure, a proxy for full-time positions. Because this procedure only finds people whose profile names the agency as an employer, we supplement it with regulators identified by matching federal payroll records to Revelio profiles, using the procedure described below. This is useful because OTS no longer exists and so has no formal company name in Revelio.

Next, for each individual we take every position beginning after their agency tenure ended. The firms these individuals join constitute the agency's private destination-firms set: 29,498 firms among which 3,605 are publicly listed and 25,893 are privately owned.

Since the firms regulators actually join may themselves respond to the pay dynamics under test, we construct two comparison groups that do not condition on realized moves. The

first consists of firms sharing a destination firm's metropolitan area and detailed industry. We restrict the group to private sector firms at which at least 10 individuals are ever observed. The size restriction removes a large mass of very small firms whose average pay would rest on a handful of imputed salaries; the median firm in the unrestricted population has three recorded employees. The restriction applies to destination and non-destination firms, so the comparison group is defined without reference to where regulators actually went. This gives 645,004 firms. The second consists of Compustat firms sharing a destination firm's headquarter state and three-digit industry, giving 21,086 firms. Compustat's historical industry code is sparsely populated before the mid-1980s, so we fill it within firm from adjacent years before assigning industries.

Finally, we derive the relevant measures of outside option. Revelio reports a salary at the start and at the end of each position. Our primary agency-level measure is the average starting salary among positions that *begin* in a given year, which is a wage actually paid in that year. For publicly traded firms we also use staff expense per employee and total staff expense from Compustat. Following Harford, He, and Qiu (2026), we impute staff expense for firms that do not report it as selling, general, and administrative expense multiplied by the industry-year average ratio of staff expense to that quantity. We average each measure across firms within an agency and year.

A.3.2 Occupation level

Our second measure is at the agency-occupation level. Importantly, we do not impose a one-to-one correspondence between federal and private occupations. Instead, we use the post-government jobs of regulators to estimate the mix of private occupations relevant for each federal occupation. Estimating this distribution requires enough observed movers, however, and the 193 occupational series in the payroll records are far too granular. We therefore first collapse these series into 10 occupational groups, reported in Table A.8, and then estimate the transition patterns from those groups into each private sector occupation based on O*NET, reported in Table A.9.

With the transition matrix in hand we construct the agency-occupation measures of outside option. The construction parallels our agency-level measure, with two modifications. First, the destination firm sets are occupation-specific. A firm enters an agency's destination set for a given occupation only if a worker leaving that agency from that occupation subsequently joins the firm. Second, starting pay is measured within private sector occupation: for each firm-year, we average the starting salaries of newly hired employees in a given O*NET occupation rather than across all employees.

For each federal occupation group, we estimate the distribution of private occupations entered by its leavers and use this distribution to value the corresponding agency-occupation destination firms. The weights are estimated from all post-exit positions held by linked regulators, excluding positions at the regulator's own agency. Every O*NET occupation observed among these transitions is retained separately – 227 in total – so we neither pool uncommon destinations into a residual category nor impose a minimum-share cutoff.[4] We

[4]Groups with few observed transitions yield noisy destination shares. We therefore shrink each group's shares toward a common prior, with weight $n_g/(n_g + K)$ on the group's own data, where n_g is its number of

then average across firms within agency, occupation, and year and express all measures in 2023 dollars.

Two limitations are worth noting. First, the weights are estimated using regulators whom we can match to a Revelio profile, and matched and unmatched regulators differ on some observables (Table A.1). Second, the weights are based on realized post-government moves.

A.3.3 Individual level

This appendix describes how we link individual payroll records to Revelio profiles. We begin with 27,934 regulators with recoverable names in our main sample, before the cumulative restrictions described in Section 1.2.[5] We recover each employee's payroll records including their name, agency, year of entry into and exit from that agency, and year of birth inferred from the reported age bins.

Next, we identify all Revelio profiles holding a position in one of the sample agencies. A Revelio profile is a candidate for a payroll record when the two share a last name, a first initial, and an agency; names are compared after stripping accents, punctuation, and case (more on that below). This condition retains 9,022 distinct profiles. For each candidate pair we compute the difference between the two sources in the year of entry into the agency, in the year of exit, and in the implied year of birth.[6] We require the entry-year and exit-year differences to be at most two years each, and we discard pairs whose implied years of birth differ by more than four.

If the payroll record contains a complete first name that differs from the profile's, we test whether the two names correspond to the same given name. To account for nicknames, we compile a dictionary of roughly 90 common nickname equivalences, such as Jim and James or Robert and Bob, resolving most such pairs. We submit the 4,823 unresolved pairs to a large language model (`gpt-4.1-nano`), which is asked only whether two first names refer to the same given name, allowing for nicknames and diminutives, and which returns a binary judgment. Payroll records and Revelio profiles sometimes carry only a first initial. In those cases, we require the entry-year, exit-year, and implied-birth-year conditions to be satisfied, with the age tolerance tightened to three years. A pair missing an exit date or an education record is therefore dropped.

At this point, a regulator may be matched to several profiles, and a profile could match several regulators. We resolve these cases by retaining the pair with the smallest difference in the year of entry into the agency; if ties remain, the smallest difference in the year of exit; and if ambiguity persists, the smallest difference in the implied year of birth. The rule is applied first within agency and then across the sample. Thirty-two links survive all three criteria as exact ties; we retain them, which is why the 2,965 matched regulators below are

observed transitions and $K = 50$. The prior weights the ten groups' destination profiles equally rather than pooling transitions; pooling would tilt the prior toward attorneys, who account for 55 percent of observed moves. Results are nearly identical using unshrunk shares (correlation 0.985).

[5] We exclude the IRS, which masks the names of all of its employees, and a small number of records at other agencies that carry no recoverable name.

[6] On the Revelio side, entry and exit are the earliest start and end years recorded across the profile's positions at that agency, and the implied year of birth is the earliest recorded bachelor's degree graduation year minus 22.

linked to 2,992 distinct profiles.

Our procedure cannot identify a regulator whose profile omits the agency or records a different surname. To handle such cases, we hand-collect information from LinkedIn for a subset of 248 cases and compare them to the information available in Revelio. Candidates for hand collection were drawn from the unreached population in randomly selected batches, together with an exhaustive sweep of the smallest agency (OFHEO). Of those 248, we verified 238 matches to a Revelio profile.

By the end of this process, we were able to identify Revelio records for 2,965 regulators, spanning 9 of the 10 sample agencies (IRS names are fully masked in the payroll records). Table A.1 compares matched and unmatched regulators.

A.4 Executive Pay Reforms

In Section 3.4, we discuss the executive pay reform. The difference-in-differences specification (Equation 3) requires a control group consisting of agencies that were not affected by the reform. It includes the African Development Foundation; Christopher Columbus Fellowship Foundation; U.S. Tax Court; Department of Agriculture, National Appeals Board; Department of Education, Advisory Councils and Committees; Department of Education, National Assessment Governing Board; Department of Treasury, Office of Thrift Supervision; Export-Import Bank of the United States; Federal Election Commission; Federal Financial Institutions Examination Council; Government Publishing Office; International Boundary Commission (U.S. and Canada); International Joint Commission (U.S. and Canada); James Madison Memorial Fellowship Foundation; Medicare Payment Advisory Commission; Morris K. Udall Scholarship Foundation; National Security Council; Pension Benefit Guaranty Corporation; Office of Administration within the Executive Office of the President; Presidio Trust; Smithsonian Institution; Smithsonian Institution, John F. Kennedy Center for the Performing Arts; Smithsonian Institution, National Gallery of Art; Smithsonian Institution, Woodrow Wilson International Center for Scholars; U.S. Holocaust Memorial Council; and Utah Reclamation Mitigation and Conservation Commission.

Figure A.1: **The Outside Option of Financial Regulators**

The figure summarizes the first private sector positions of 1,013 former financial regulators, based on manually linked information derived from payroll records and Revelio Labs career data. The first private sector position is the earliest Revelio position starting no earlier than one year before, and no later than three years after, the fiscal year of exit, excluding positions at the agency the regulator just left. Panel A shows the distribution of seniority levels in these initial private sector roles: 43.5% transition directly into director-level or higher positions, while close to two-fifths begin in junior or associate roles. Panel B reports the industries that absorb former regulators, for the 801 observations with a classified industry. Financial services (37.1%) and legal services (30.7%) dominate, followed by consulting (7.7%). See Section 2.1.

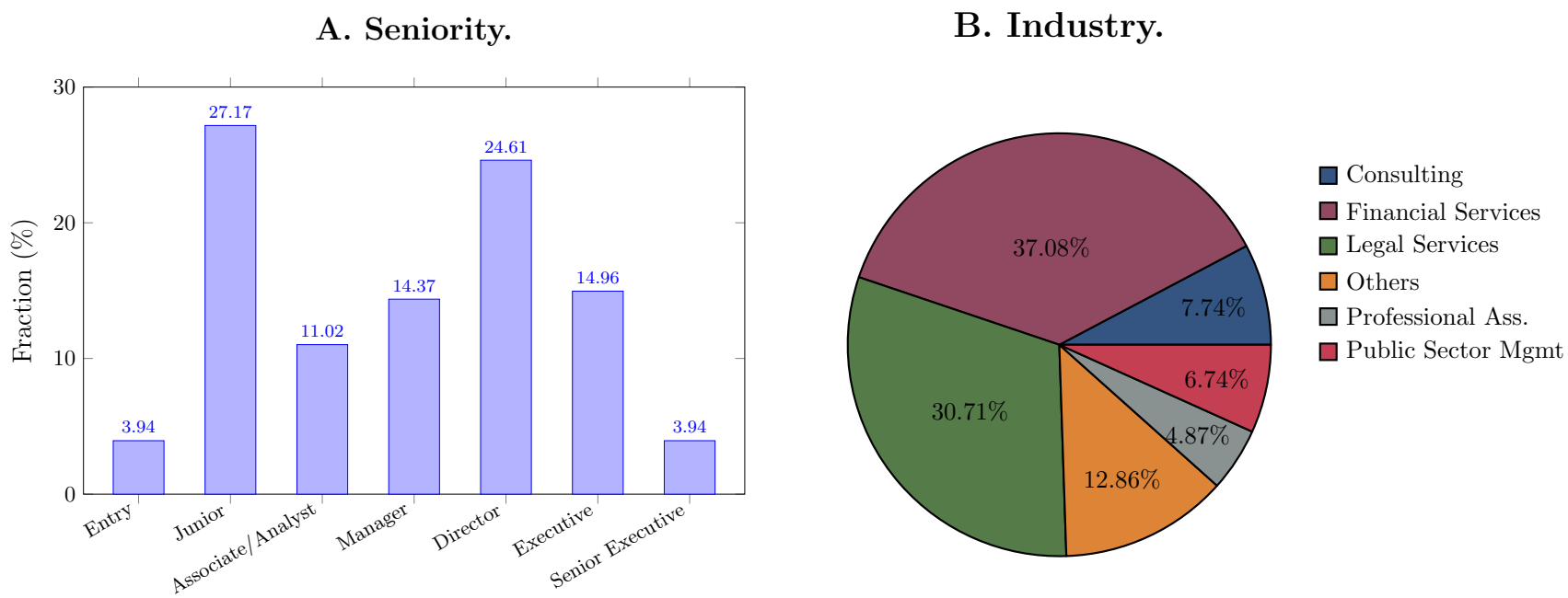


Table A.1: **LinkedIn-Matched and Unmatched Regulators**

This table compares characteristics between LinkedIn-matched and unmatched subsamples. Pay denotes the employee's annual salary in constant 2023 USD. $Tenure$ measures the number of years since entry into federal service. Age is approximated using the midpoint of the employee's reported age bin. $Manager = 1$ if the employee holds a managerial position in year t. $Exit = 1$ if the employee leaves government service in year $t + 1$. $College = 1$ ($Postgrad = 1$) indicates that the employee has completed a college (postgraduate) degree. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. See Section 1.2.3 and Appendix A.3.

LinkedIn	(1) Matched	(2) Unmatched	(3) Difference
$Pay(\$)$	\$141,409	\$122,152	19,256***
$Manager(0/1)$	0.224	0.241	-0.017***
$Exit(0/1)$	0.071	0.078	-0.007***
$Tenure(years)$	10.203	13.514	-3.312***
$Age(years)$	37.320	43.188	-5.868***
$College(0/1)$	0.953	0.822	0.130***
$Postgrad(0/1)$	0.525	0.302	0.223***
N	20,994	211,791	

Table A.2: **Pre-Government Careers of Financial Regulators**

Panel A: Prior employer. This table reports the pre-government careers of financial regulators. The data are from Revelio Labs and include unique financial regulators who held positions outside of the government. $ForProfit$ Firm refers to private sector, for-profit companies. $Nonprofit$ includes nonprofit organizations, excluding universities. *Academic* refers to academic institutions. $PublicSector$ indicates employment in non-financial agencies. $PublicFirm$ refers to publicly traded companies. $Pay^{gov't}$ denotes the regulator's first annual salary at financial agencies (in 2023 dollars). $Pay^{private,base}$ is the employee's final base salary (in 2023 dollars) before joining the government. *Total Private Comp.* is the employee's final total compensation (Base salary + Bonus in 2023 dollars) before joining financial agencies. *Performance pay* is the final performance pay ($1 - \frac{Private\ Base\ Salary}{Total\ Private\ Comp.}$) before financial agencies. See Section 2.1.

	Mean	**SD**	**Min**	**Max**
$ForProfit$	0.62	0.49	0.00	1.00
$Nonprofit$	0.07	0.26	0.00	1.00
$Academic$	0.02	0.14	0.00	1.00
$PublicSector$	0.29	0.45	0.00	1.00
$PublicFirm$	0.26	0.44	0.00	1.00
$Pay^{private,base}$	\$126,295	\$75,717	\$9,986	\$636,832
$Pay^{private,total}$	\$153,104	\$117,804	\$9,986	\$1,125,735
$Pay^{private,P4P}$	0.13	0.11	0.00	0.83
Pay^{gov}	\$113,026	\$38,915	\$33,943	\$270,710

Panel B: Seniority. This table reports the seniority level of financial regulators' last job before entering the financial agencies. Seniority is categorized into seven levels, as defined by Revelio Labs. See Section 2.1.

Seniority	**Number**	**Fraction** (%)
Senior Executive (e.g., CFO; COO; CEO)	8	1.12
Executive (e.g., Partner, Managing Director)	14	1.96
Director (e.g., Head of Legal)	64	8.98
Manager (e.g., Lead Lawyer)	71	9.96
Associate/Analyst (e.g., Attorney)	103	14.45
Junior (e.g., Legal Adviser)	348	48.81
Entry (e.g., Paralegal)	105	14.73

Panel C: Industries. This table reports the top 15 industries in which financial regulators held prior to entering financial agencies. Industry is classified using the *Revelio Industry Classification K=50* clusters. See Section 2.1.

Industry:	**Number**	**Fraction (%)**
Legal Services	241	44.71
Public Sector Management	86	15.96
Financial Services	64	11.87
Consulting and Advisory Services	42	7.79
Aerospace and Defense	28	5.19
Education Services	14	2.60
Professional and Trade Associations	12	2.23
Information Technology Services	8	1.48
Miscellaneous	7	1.30
Healthcare and Wellness Services	4	0.74
Industrial Manufacturing	4	0.74
Energy and Resources	3	0.56
Human Resources Services	3	0.56
Research and Development	3	0.56
Commercial Aviation	2	0.37

Table A.3: **Performance Pay and Exits: Excluding Agencies and Cohorts**

Panel A. Leave-One-Cohort-Out. This table re-estimates Equation 1 under the fixed effect specification of column (6) of Table 3, dropping one reform cohort at a time. Column (1) is the full sample; columns (2)–(8) exclude the cohort named in the column header (agency treated in that cohort in parentheses). Effect (%Mean) is the coefficient on Treated×Post as a percentage of the sample mean of the respective outcome. Standard errors, clustered by agency×cohort, are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. The estimated effect remains positive and significant regardless of which cohort is excluded, confirming that no single reform episode drives the result. See Section 3.3.

Outcome:	*Exit*							
	(1) Full sample	(2) Excl. 1981 (OCC)	(3) Excl. 1991 (NCUA)	(4) Excl. 1993 (FCA)	(5) Excl. 2000 (IRS)	(6) Excl. 2002 (SEC)	(7) Excl. 2003 (FDIC)	(8) Excl. 2006 (CFTC)
Treated×Post	0.042***	0.035**	0.045***	0.043***	0.043***	0.045***	0.036***	0.044***
	(0.008)	(0.014)	(0.008)	(0.008)	(0.008)	(0.008)	(0.010)	(0.008)
R^2	0.070	0.072	0.071	0.072	0.069	0.071	0.062	0.070
N	229,768	179,299	206,047	196,109	198,665	194,481	182,361	221,646
Effect (%Mean)	54.38	49.37	58.00	56.80	54.38	56.59	44.89	55.82

Panel B. Leave-One-Agency-Out. This table re-estimates Equation 1 under the fixed effect specification of column (6) of Table 3, fully purging one agency at a time from the pooled sample (as treated *and* as a control, in every cohort it appears in). Column (1) is the full sample; columns (2)–(11) exclude the named agency. Effect (%Mean) is the coefficient on Treated×Post as a percentage of the sample mean of the respective outcome. Standard errors, clustered by agency×cohort, are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. The estimated effect remains positive and significant regardless of which agency is excluded, with the FDIC exclusion producing the one meaningfully weaker estimate. See Section 3.3.

Outcome:					*Exit*						
	(1) Full sample	(2) Excl. CFTC	(3) Excl. OFHEO	(4) Excl. FinCEN	(5) Excl. IRS	(6) Excl. OCC	(7) Excl. OTS	(8) Excl. FCA	(9) Excl. FDIC	(10) Excl. NCUA	(11) Excl. SEC
Treated×Post	0.042***	0.044***	0.044***	0.048***	0.044***	0.035**	0.032**	0.043***	0.025**	0.046***	0.044***
	(0.008)	(0.008)	(0.008)	(0.007)	(0.008)	(0.014)	(0.012)	(0.008)	(0.010)	(0.008)	(0.009)
R^2	0.070	0.072	0.070	0.067	0.071	0.072	0.071	0.069	0.068	0.070	0.081
N	229,768	220,093	228,240	188,544	213,548	215,637	192,230	226,206	179,954	222,919	179,438
Effect (%Mean)	54.38	56.85	56.89	59.45	55.47	46.50	41.80	55.18	31.28	59.24	58.04

Table A.4: **The Outside Option and the Timing of Transition to Performance Pay**

We estimate a linear probability model on the agency×year panel of treated and control agencies. Outcome equals 1 in the year of transition to P4P, and Δ Outside option is the one-year change in the outside option available to the agency's regulators, divided by its own standard deviation. Starting Salary is the log average salary paid to newly hired employees; Staff Expense (per employee) is log staff expense per employee, a proxy for the going wage; Staff Expenses (total) is the corresponding log total staff expense, capturing the scale of hiring – the latter two are available only for publicly traded firms. The Realized peer group is the agency's actual destination firms – the firms its regulators join after leaving (which we compute from Revelio profiles); the Broad peer group is a broader group of firms in the same industry and location as the realized peer group. All measures are equal-weighted across firms and winsorized at the 1st and 99th percentiles. Standard errors are clustered by agency. Changes in outside options do not predict the timing of P4P adoption. See Appendix A.3 and Section 3.3.

Outcome:	Agency Transition to Pay-for-Performance					
Quantity:	Starting Salary		Staff Expense (per employee)		Staff Expenses (total)	
Peer Group:	Realized	Broad	Realized	Broad	Realized	Broad
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Outside option	-0.022	0.013	0.000	0.003	-0.039	-0.069
	(0.016)	(0.059)	(0.002)	(0.009)	(0.025)	(0.065)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.148	0.142	0.141	0.141	0.153	0.158
N	189	189	189	189	189	189

Table A.5: **Alternative Standard Errors Clustering**

Panel A. Alternative Clustering. This table re-estimates Equation 1 under the fixed effect specification of column (1) of Table 3, using alternative approaches to clustering and inference. Column (1) clusters standard errors at the agency level. Column (2) uses two-way clustering at the agency and year levels. Column (3) reports inference using a wild cluster bootstrap at the agency level with Webb weights and 999 replications. Standard errors are reported in parentheses. For column (3), the wild cluster bootstrap p-value is reported separately. The outcome is $Exit = 1$ if a regulator leaves government service in year $t + 1$. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. The estimate is robust to all three approaches, addressing concerns about inference with a small number of agencies. See Section 3.3.

Cluster:	(1) Agency	(2) Agency & Year	(3) Bootstrap
Treated×Post	0.031***	0.031**	0.031**
	(0.008)	(0.012)	
N	231,784	231,784	231,784
WCB p-value			0.0240**

Panel B. Score-Variance Tests. This table reports the MacKinnon, Nielsen, and Webb (2023) score-variance test of whether a *fine* clustering structure is adequate against a *coarser* alternative, for the two adjacent steps in the ladder none < agency×cohort < agency. H and G are the number of fine and coarse clusters. τ_σ is the test statistic (asymptotically standard normal under the null that the fine level is adequate, diverging under the alternative); P (asy.) and P (boot.) are its upper-tail P value under the asymptotic and wild-bootstrap versions of the test. The specification is the same as column (1) of Table 3. The test rejects no clustering in favor of agency×cohort clustering but cannot reject agency×cohort against the coarser agency-level alternative, supporting agency×cohort as our preferred level of clustering. See Section 3.3.

Null vs. alternative	Step	H	G	τ_σ	P asy. / boot.
None vs. agency×cohort	0	231,784	36	4.010	0.000 / 0.000
Agency×cohort vs. agency	1	36	10	0.713	0.238 / 0.268

Table A.6: **Alternative Sample Weights for Stacked Difference-in-Differences**

This table is identical to Table 3, except that we estimate Equation 1 by weighted least squares using the sample weights proposed by Wing, Freedman, and Hollingsworth (2024). We assign $Q_{c,i} = 1$ to every treated observation and $Q_{c,i} = (N_c^D/N^D)/(N_c^C/N^C)$ to every control observation, where N_c^D and N_c^C are the numbers of treated and control employees in cohort c, and N^D and N^C are the corresponding totals across all seven cohorts. Standard errors, clustered by agency×cohort, are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. The estimates closely track the unweighted results in Table 3, indicating that heterogeneity in cohort sizes does not drive our findings. See Section 3.3.

Outcome:	*Exit*					
	(1)	(2)	(3)	(4)	(5)	(6)
Treated×Post	0.034***	0.034***	0.035***	0.034***	0.040***	0.044***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.009)	(0.008)
Cohort×Agency FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort×Occupation×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort×Pre-Pay FE	No	Yes	Yes	Yes	Yes	Yes
Cohort×Pre-Tenure FE	No	Yes	Yes	Yes	Yes	Yes
Cohort×Education×Year FE	No	No	Yes	Yes	Yes	Yes
Cohort×Manager×Year FE	No	No	No	Yes	Yes	Yes
Cohort×AgeBin×Year FE	No	No	No	No	Yes	Yes
Cohort×City×Year FE	No	No	No	No	No	Yes
R^2	0.029	0.039	0.039	0.040	0.068	0.077
N	231,784	231,784	231,784	231,784	231,761	229,768
Effect (%Mean)	43.23	43.48	44.51	44.20	51.66	57.15

Table A.7: **Career Trajectories of Federal Executives**

The sample includes all federal executives who held an executive position between 1973 and 2013, focusing on the years in which they served in such positions. *Pay* denotes the executive's annual salary in constant 2023 USD winsorized at the 1st and 99th percentiles. *Tenure* is the number of years since entering the public sector, and *Age* is the executive's age. $Exit = 1$ when the executive leaves government service, regardless of rank at the time of departure. $College = 1$ and $Postgrad = 1$ indicate whether the executive holds a college or postgraduate degree, respectively. See Section 3.4.

Statistic:	Mean	Median	S.D.	Min	Max	N
Pay ($)	$212,930	$217,500	$21,909	$167,916	$251,081	156,634
Tenure (years)	16.5	16.0	9.2	0.0	40.0	156,634
Age (years)	51.5	52.0	8.3	22.0	75.0	156,633
Exit (0/1)	10.8	0.0	31.0	0.0	100.0	151,902
College (0/1)	96.6	100.0	18.1	0.0	100.0	156,634
Postgrad (0/1)	75.4	100.0	43.1	0.0	100.0	156,634

Table A.8: **Grouping Federal Occupational Series into Occupation Groups**

Federal payroll records classify each regulator into 193 occupational series, which we collapse into 10 occupation groups. *Obs.* counts person-year observations and *Series* the number of distinct occupation series in the group. The final row reports series with no private sector counterpart. See Appendix A.3.

Occupation group	**Obs.**	**%**	**Series**	**Principal federal occupational series**
Financial Examiner	60,080	25.8	2	Financial Institution Examining, Securities Compliance Examining
Attorney	39,951	17.2	8	General Attorney, Paralegal Specialist, Legal Assistance; and 5 other series
Management	37,867	16.3	10	Miscellaneous Administration and Program, Program Management, Administrative Officer; and 7 other series
Analyst	31,584	13.6	16	Import Compliance Series, Auditing, Financial Analysis; and 13 other series
Assistant & Secretary	22,197	9.5	10	Secretary, Miscellaneous Clerk and Assistant, Clerk-Typist; and 7 other series
Tech	13,208	5.7	26	Computer Specialist, Information Technology Management, Chemistry; and 23 other series
Accounting	12,722	5.5	4	Accounting, Accounting Technician, Accounting Student Trainee; and 1 other series
HR and Admin	5,274	2.3	22	Human Resources Management, Human Resources Assistance, Support Services Administration; and 19 other series
Economist	2,179	0.9	1	Economist
Communications	1,943	0.8	8	Public Affairs, Clerk-Stenographer and Reporter, Communications Management; and 5 other series
Not classified	5,780	2.5	85	Customs Aid, General Arts and Information, Visual Information; and 82 other series
Total	232,785	100.0	193	

Table A.9: **Private Sector Positions for Former Federal Regulators**

The sample includes financial regulators linked to a Revelio profile who held at least one post-government private sector position. We identify the federal occupation and the private sector occupation of each leaver. Federal occupations are grouped into 10 categories and private occupations are 227 O*NET-SOC occupations. We report the total transitions observed for the federal occupation group and the top five private sector occupations. Each federal occupation's outside option is the average salary across destination occupations, weighted by its observed shares of leavers landing in each O*NET occupation. See Appendix A.3.

Federal occupation	**N**	**Principal private destinations (share of weight)**
Financial Examiner	1,245	Financial Risk Specialists 12%; Investment Fund Managers 10%; Compliance Managers 8%; Loan Officers 7%; Treasurers and Controllers 7%
Attorney	3,234	Lawyers 55%; Compliance Managers 9%; Chief Executives 5%; Investment Fund Managers 5%; Paralegals and Legal Assistants 3%
Management	261	Chief Executives 18%; Lawyers 15%; Investment Fund Managers 7%; Legislators 6%; Treasurers and Controllers 4%
Analyst	299	Investment Fund Managers 8%; Information Technology Project Managers 8%; Financial Risk Specialists 8%; Treasurers and Controllers 7%; Loan Officers 7%
Assistant & Secretary	41	Executive Secretaries and Executive Administrative Assistants 7%; Financial Risk Specialists 6%; Legislators 5%; Lawyers 4%; Information Security Engineers 4%
Tech	71	Information Technology Project Managers 16%; Chief Executives 4%; Computer Systems Engineers/Architects 4%; Database Architects 4%; Management Analysts 4%
Accounting	502	Treasurers and Controllers 20%; Compliance Managers 12%; Financial Risk Specialists 11%; Investment Fund Managers 10%; Lawyers 7%
HR and Admin	34	Information Technology Project Managers 8%; Credit Analysts 6%; Compensation, Benefits, and Job Analysis Specialists 5%; Chief Executives 5%; Lawyers 5%
Economist	105	Economists 12%; Investment Fund Managers 11%; Economics Teachers, Postsecondary 6%; Chief Executives 5%; Financial Risk Specialists 4%
Communications	35	Chief Executives 10%; Writers and Authors 10%; Investment Fund Managers 9%; Legislators 8%; News Analysts, Reporters, and Journalists 5%

Table A.10: **Effect of Performance Pay on Rulemaking Activity: Robustness**

This table is identical to Table 5, except for the fixed effects structure. We remove the pre-reform pay and tenure FE and replace them with cohort×regulator FE. Standard errors, clustered by agency×cohort, are in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. The estimates are similar to those without employee fixed effects, suggesting the effect of performance pay on rulemaking holds within employee. See Section 4.1.

Outcome:	$\#RegDoc$			$\mathbb{1}(RegDoc)$		
	(1)	(2)	(3)	(4)	(5)	(6)
Treated×Post	0.575***	0.341**	0.503***	0.127***	0.084*	0.082
	(0.188)	(0.167)	(0.160)	(0.037)	(0.043)	(0.047)
Cohort×Agency FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort×Occupation×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort×Education×Year FE	No	No	Yes	No	No	Yes
Cohort×Manager×Year FE	No	No	Yes	No	No	Yes
Cohort×AgeBin×Year FE	No	No	Yes	No	No	Yes
Cohort×City×Year FE	No	No	Yes	No	No	Yes
Cohort×Regulator FE	No	Yes	Yes	No	Yes	Yes
Model	Poisson	Poisson	Poisson	OLS	OLS	OLS
R^2				0.076	0.292	0.270
N	3,422	2,034	1,886	4,014	3,980	3,741
Effect (%Mean)				57.55	37.95	37.27

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